CURRICULAR PHILOSOPHY AND STUDENTS' PERSONAL

EPISTEMOLOGIES OF SCIENCE

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In Two Volumes

Volume One

A thesis submitted to the University of Surrey in partial fulfilment of the requirements for the degree of Doctor of Philosophy, 1986
For my parents
'Virtually all our disciplines have relied on conceptions which are now incompatible with the Cartesian axiom, and with the static world view we once derived from it. For underlying the new ideas, including those of modern physics, is a unifying order, but it is not causality; it is purpose, and not the purpose of the universe and of man, but the purpose in the universe and in man. In other words, we seem to inhabit a world of dynamic process and structure. Therefore we need a calculus of potentiality rather than one of probability, a dialectic of polarity, one in which unity and diversity are redefined as simultaneous and necessary poles of the same essence'.

World Perspectives, Ruth Nanda Anshen (1971, p.250: original emphasis).
ABSTRACT

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In this thesis I employ a constructivist epistemological stance (principally influenced by that due to George Kelly) to critically examine the curricular response to contemporary notions of truth, objectivity and knowledge.

I take science education (at both Secondary and Tertiary levels) as my special reference within the education system.

An important part of my work explores students' and teachers' personal meanings of science and scientific method, i.e. alternative conceptions of science, and I see it as contributing to the growing body of research concerned with alternative conceptions in science: the 'Alternative Conceptions Movement' (ACM) in educational research.

To help articulate my views on these matters I use an augmented version of a framework or model, developed by my immediate colleagues, for conceptualising cognitive aspects of science education and the transformation of scientific knowledge. My version of this framework features components under the following main headings: 'Scientists'-Science', 'Philosophers'-Science', 'Curricular-Science', 'Teachers'-Science', 'Students'-Science', and 'Childrens'-Science'.

I argue that, suitably augmented and interpreted, Kelly's theory is capable of rationally integrating existing ACM research, together with my own.
My classroom research uses a number of complementary investigative methods, some of them novel. These may be grouped under the following three headings:

- interviews
- lesson observations
- written exercises

I present an outline of a theory of teaching which is compatible with ACM research and make recommendations for future science teaching and research.

N.B. To avoid an insidious (male) sexism and 'his/her' formulations which I find tedious, I shall use plural forms throughout this thesis, e.g. their, themself.
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If there is worth in this thesis, then that worth shall, in large part, reflect the intellectual and emotional environment which I have been privileged to share with my colleagues in the Department of Educational Studies.

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Responsibility for any errors of judgement and of typography as might remain in this thesis is mine alone.
## CURRICULAR PHILOSOPHY AND STUDENTS' PERSONAL EPISTEMOLOGIES OF SCIENCE

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Chapter 1. Origins and Aims of this Study.

'The wind blew to me through the keyhole and said: 'Come!'. The door sprang cunningly open and said: 'Go!!'
Thus Spake Zarathustra, Nietzsche.

1.1 Introduction

A review of the recent literature of science education is likely to lead the reader to conclude that the ideas about psychology of Jean Piaget dominate that field. His work, and that of his immediate co-workers, apparently constitute the "received view" for most teachers-educators and, consequently, student-teachers. A high point of acceptance was reached, for the U.K., in the mid-1960s. It was then possible to open respected textbooks on educational psychology (e.g, Stones, 1960) and find the Piagetian "stages" listed, apparently as "facts" without authorship being attributed in the index.

More recently, some books on classroom practice (e.g, UNESCO, 1980) have still presented Piagetian psychology without question or alternative. We suggest that such practices tend more towards indoctrination than towards education. Moreover, as Piagetian tests are turned into a technology (e.g. Shayer and Adey, 1981), the likelihood of their being used in classrooms increases enormously. These technologies are easy to apply. Their use can be so interpreted as to seem to promise the production of simple prescriptive answers to complex problems in teaching and learning. Apparently derived from Piagetian theory, and in keeping with its ethos, their ready use sidesteps the burgeoning academic criticism of Piagetian
doctrines, which I shall discuss later.

Discussing one of Piaget's central ideas, that of equilibrium, Richmond has pointed out that

'[ ] the equilibrium principle is closely related to the concept of readiness for learning. Readiness for this or that educational experience is another way of saying that the equilibrated structures can accommodate to a given experience. However, if intellectual activity is not best described by means of an equilibrium model then this view of readiness will not stand.'

(Richmond, 1970, p.110)

The uncritical use of Piagetian technologies may thus supply a scientistic rationale for a policy of restricted access to science education on the basis of "readiness". In times of resource scarcity, such as now, this possibility moves Piagetian technologies from the academic into the general political arena.

Now, the work of Piaget and his school (PS) may be said to embrace a constructivist theory of knowledge and, indeed, most educationalists would similarly claim so to do: a constructivist epistemology is assumed on the part of both teacher and learner, researcher and researched.

The basic constructivist stance may be summarised by the now familiar slogan "All observations are theory-laden". This commitment has the immediate pedagogic consequence that pupils are active construers of knowledge and their environment and, hence, they cannot be held to enter the classroom — or the research interview — with a 'tabula rasa' or "blank mind". Clearly, however, the basic constructivist stance
constitutes neither a theory of knowledge nor a theory of learning on its own. Any elaboration of the basic stance reveals that many different versions of constructivist epistemology are possible: the PS represents just one amongst many constructivist traditions in educational research, albeit, still the most influential one. Over the last several years, however, there has been created another body of constructivist research concerned with students' 'pre-conceptions', 'mini-theories', 'alternative frameworks', 'alternative conceptions' and the like. This sort of research has been named variously. Using an expression favoured by myself and my immediate colleagues, however, I shall refer to it as the 'Alternative Conceptions Movement' (ACM) (Gilbert and Swift, 1985). It was under the aegis of this latter tradition that I began my own research.

Whilst the ACM may be said to contribute to constructivist approaches to teaching and research, it does not yet seem to have made any existing constructivist tradition. Members of the department which I joined were already actively investigating students' alternative conceptions in science and were just beginning to explore the utility of the ideas of George Kelly as a methodology for their research. Kelly's theory, namely, 'Personal Construct Theory', or 'Personal Construct Psychology' (PCP) as it is now generally known (Fransella, 1977), was originally developed for use in clinical psychotherapy. After considerable initial scepticism, I came to see the utility of applying and extending Kelly's ideas in formal educational settings myself.
1.2 Development of the Focus of my Research

When I joined my colleagues at Surrey, their work was focussed upon students' learning in physics. Influenced both by that and by the interests and academic expertise that I already had, I chose to seek and explore students' personal understandings of ideas in biology.

During my preliminary investigations of students' understandings of concepts in biology, I noticed in interviews that they often appealed to 'scientific method', 'experimental proof' and the like, apparently as a justification for their expressed views (whether 'orthodox' or 'alternative'). As a result of these and many other varieties of experience in my early research, I became convinced that students' personal meanings of scientific methods and meta-theoretical terms were worthy of investigation in their own right and, further, that they might contribute to our understanding of the origin and maintenance of students' alternative conceptions in science. I judged this latter notion to be supported by certain of the arguments presented by Kelly and Feyerabend, whose ideas were progressively informing my research approach:

'Men not only construe their alternatives, but they construe also criteria for choosing between them'.
(Kelly, 1969, p.85)

'We concede that our epistemic activities may have a decisive influence even upon the most solid piece of cosmological furniture - they may make gods disappear and replace them by heaps of atoms in empty space'.
(Feyerabend, 1978, p.70)

At about the same time as I was drawing these conclusions, I began
studying stated objectives of science curricula (beginning with those which I was observing at the time, viz, SCISP and Nuffield 'A' level biology). I did this out of a conviction that every person who considers himself to be concerned with education must supply at least a provisional, personal answer to the question "What is education for?" in order to be able to proceed effectively, indeed, to proceed at all. By examining existing curricula objectives, then, I hoped to inform and develop my own ideas on what they should be.

I found that the development by students of skills in "scientific reasoning" and "scientific method" featured amongst the objectives of all the curricula I examined. This inference is corroborated and amplified by Fensham (1983) who proposes that current objectives of science curricula fall into the following five broad categories of concerns:

1. Concerns for the factual and theoretical (conceptual) knowledge of science.

2. Concerns for the process of scientific investigation and reasoning.

3. Concerns for practical (laboratory) investigations in science.

4. Concerns for attitudes towards science and attitudes associated with science.

5. Concerns for the relation of science to society.
This constituted a second reason for my eventual decision to change the subject of my research, viz, I resolved to investigate students' personal meanings of (rather than in) science and of scientific-method.

Now, for me to be able to make judgements in this respect, I needed to make clear in my own mind my views as to what 'science' and 'scientific-method' are — a task which necessitated an appraisal of existing philosophies of science.

Whilst I saw my research being focussed upon students' personal understandings, if the curricular objective in question was to be implemented, then teachers' personal understandings would have to be explored also. Finally, since teachers would be operating under constraints of a curriculum and examinations, an analysis of teaching and curricular materials was indicated.

Now, at the same time as I was deliberating over the focus of my intended field-research it was apparent to all concerned that there was a certain degree of confusion amongst members of the ACM as to the detailed nature of an 'alternative conception' and the role it should play within a theory of teaching and learning. I contend that this is generally the case today. Moreover, the work of the Movement has become sufficiently well known to have attracted something of a "critical backlash". Whilst no critic has questioned the basic constructivist stance, some have argued that the comprehensive application of the alternative conceptual framework in classroom research has exaggerated both the level and the prevalence of childrens' ability to theorise spontaneously. Other critics complain that
the idea "lacks precision" and "means all things to all persons". Recently, in a review of a book by some influential members of the ACM, Black argued that

'It is clear that teaching strategies have to be radically reconstructed in face of the new evidence of the varied, often seemingly bizarre, and to children - sensible explanations that already meet any needs that their holders might have. However, it is not possible to confront this problem - in particular to bridge the gap between empirical evidence and pedagogical guidance - without some theoretical model of learning married to a reconsideration of the concepts to be learned.

In both these aspects, the book reflects the weakness of the field. Analysis of the underlying scientific concepts involved is attempted in some of the chapters, but those attempts are rarely adequate and sometimes flawed. This matters, because a reconsideration of the pedagogical targets in relation to new insights into children's own ideas requires radical rethinking to construct new, elementary yet valid approaches to the conceptual world of science.

The lack of models for learning shows in two ways. Some authors offer programmes to challenge and change existing ideas; some propose that effort must concentrate on helping children express and exchange their ideas and state that overt challenge is not appropriate; and some offer no prescriptions. Such inconsistencies are nowhere analysed. More subtly, the theoretical assumptions that must guide the reported investigations are largely unexamined.'
(Black, 1986, p.18)

The need for me to articulate a meta-theory clearly and in some detail was, perhaps, rendered more urgent than to most members of the ACM due to my decision to research students' alternative conceptions of 'science' and 'scientific-method'. I hope that the results of my attempt to do so shall have general utility to the ACM and shall go some way to countering and transcending criticisms of its work.
5.3 Summary of Aims of this Research

In this thesis I have three overarching and complementary aims:

(1) To explore students' and teachers' personal epistemologies of science;

(2) To elucidate the epistemological image of science projected overall by the existing education system;

(3) To re-construe Kelly's 1955 articulation of 'Man-the-Scientist' in light of both influential traditions in epistemology and the interests and commitments informing the ACM with a view to outlining principles for a compatible 'Personal Construct Pedagogy', applicable in the first instance to science education.
Chapter 2. Philosophy and Education

'Epistemology and the philosophy of education are closely connected. When the sense of this connection gets lost the result is a period of stunted epistemology and no philosophy of education worth mentioning. We are emerging from such a period.'

Against Empiricism: Holland (1980, p.10)

2.1. Introduction

My purpose in this chapter is to present arguments which shall establish an initial plausibility to the view that epistemology is both necessary and important for education in general, and for science education in particular.

2.2. Illustrations in Use of Some Key Terms used in this Thesis

The term 'education' has long enjoyed a widespread currency within societies. Its meanings, however, have varied considerably according to purpose and context. Here, Passmore provides a useful elaboration:

'A systematic book on the philosophy of education would need [...] to distinguish between education\textsuperscript{1} (upbringing), education\textsuperscript{2} (schooling), education\textsuperscript{3} (producing educated
men) - to say nothing of education - the study of these processes' (Passmore, 1980, p.22: original emphasis).

My ideas on this subject complement those of Passmore in many ways. In particular, I like the suggestion, implicit in the above quotations, that 'philosophy of education' somehow "entails" or informs all other meanings of 'education'.

For my part, I should like to begin by adopting a broader distinction that is sometimes made between 'philosophy of education' and 'pedagogy' and to outline my personal meanings for these expressions.

(1).Philosophy of Education. Philosophers of education are primarily concerned with the aims and purposes of education ("educational values"). Their endeavours may be elucidated by considering the sorts of questions that they ask (and attempt to answer): e.g. "What should education be for?", "Who should we teach?", "What should we teach?".

(2).Pedagogy. Pedagogues, by contrast, are more concerned with the means and results of education. Their efforts may be said to fall into two categories:

(2a).Theory of Teaching. Theorists of teaching attempt to answer such questions as "How should we achieve what we want to achieve through teaching?"
(2b). **Theory of Educational Research.** Theorists of educational research attempt to answer questions such as "How should we know when we have achieved what we want to achieve through education?"

Now, as I have already intimated, I do not lay claim to any great originality in making these basic distinctions and in proposing these meanings. Nor, however, do I wish to imply that they are uncontroversial and without need of justificatory argument. My earlier stated views adumbrate a departure from the allegedly "value free", 'analytic', approach to philosophy of education which has been of pre-eminent influence in recent decades (though it is now showing signs of a general withering), in favour of a return to a more traditional, 'normative' (I prefer 'value-explicit'), one. It is partly for these reasons that I have refrained from giving formal 'definitions' of the key terms of this section. (I describe and argue against analytic philosophy of education in Chapter 10).

I would like to stress that, so far, I have characterised only the approach to (or type of) philosophy about education that I intend to take, for I believe that many, even most, of the specific educational values and pedagogic recommendations that I shall propose and endorse within my philosophy of education cannot be described as "traditional" in a way which I would accept.
My distinctions and meanings may be related back to those of Passmore in the following way:

- 'Philosophy of Education' has principal (though not exclusive) reference to Passmore's 'education$_1,3$'; similarly,
- 'Theory of Teaching' to 'education$_2$',
- 'Theory of Educational Research' to 'education$_4$'.

I should mention that by 'schooling' ('education$_2$') I mean any formal, institutionalised, education sanctioned by the (British) State. It is ultimately this sense of 'education' for which I intend this thesis to have principal pertinence.

I shall use 'educationalist' as a generic term to refer to educational researchers and philosophers of education, and 'educator' to refer to professional teachers.

I shall use 'educand' as a generic term to refer to any person engaged in formal education (i.e. 'schooling'). Sometimes, however, I shall distinguish between 'pupils' for persons engaged in Primary or Secondary education, i.e. in schools, and 'students' for persons engaged in Tertiary education, i.e. in Universities, Polytechnics, and rely upon the context to make clear any further details of these meanings.
Turning now to my personal meaning of the term 'epistemology', it may be useful to first place it within its broad context of philosophy.

Engler provides a concise description of the five main divisions of philosophy:

'Traditionally, philosophy has encompassed five types of language and study: logic, aesthetics, ethics, politics, and metaphysics. Logic is the study of correct or normative reasoning; it describes the ideal method of making inferences and drawing conclusions. Aesthetics is the study of ideal forms and beauty; it deals with the nature of the beautiful and with judgements about beauty. Ethics is the study of ideal conduct; it deals with the knowledge of good and evil. Politics is the study of ideal social organization and describes those forms of social and political structures that are most appropriate for human-beings. Metaphysics is the study of ultimate reality and attempts to coordinate what is real in the light of what is ideal.'

(Engler, 1979, p. 6: my emphasis).

Following Aristotle, however, we may further divide metaphysics into 'ontology' and 'epistemology'. Roughly speaking, the first half of Engler's description of metaphysics ('the study of ultimate reality') applies mainly to ontology; the second half ('attempts to coordinate what is real in the light of what is ideal') mainly to epistemology. Since the importance of epistemology in educational settings is the main subject of both this chapter and this thesis, I should like to elaborate further upon what I consider to be the basic nature and worth of epistemology as an area of human endeavour.

- 2.5 -
I contend that from a present day point of view - especially a Western one - it seems reasonable to suppose that to early Personkind the world must have seemed to be a terrifying and largely chaotic place, full of phenomena beyond both their control and their comprehension. If this view is accepted, then it may seem plausible further to suppose that as social organisation and language (spoken and written) gradually developed so persons came to desire and to strive not only 'to know' things about the world, e.g. sources of food and of shelter, but also 'to know how they know' such things. From contemporaneous manuscripts we can be sure that this was an important concern from at least the time of early Greek civilization and it is this second sense of knowing which evokes issues of 'epistemology' or 'theory of knowledge'.

Epistemology would seem to derive its enduring pertinence and importance from such everyday experiences as the personal, subjective, one of having made a "mistake" about some state of affairs in the world, or in settling disputes over such matters with other persons. 'Epistemology', then, is not knowledge itself; it is - or is purported to be - "knowledge-about-knowledge" ('meta-knowledge').

Many of us make implicit, unrecognized, appeals to epistemology when we claim, for example, that "seeing is believing". We are always making claims of this sort, though for most of us, most
of the time, such claims and issues are of only secondary interest. Epistemologists, by contrast, are philosophers who deliberately devote themselves to the sustained critical contemplation of just such claims and to the development of what they argue to be ever more sophisticated alternatives. Trite and pointless though many of the classical examples used by epistemologists for discussion initially may seem, e.g. the existence of tables and chairs, the importance and work of epistemology may be anticipated more compellingly as soon as we extend our epistemological attention to notions such as 'atom', 'gene' or 'chemical bond'.

2.3. Epistemology and Education

Much confusion has surrounded the possible relevance of epistemology for education. This notwithstanding, I contend that it may be argued that many epistemologists have systematically (and some not so systematically) attributed either too much or too little importance to epistemology in an educational context and that this cannot be sufficiently explained solely by examination of their detailed, personal, epistemological commitments and educational values. Hence, in this chapter, I shall attempt to present my views on only the general "nature and limits" of the importance of epistemology for education. That is to say, I hold the arguments that I shall advance in this chapter to have force with respect to any elaborated, delineable, theory of knowledge and to any such
set of educational values endorsed. By doing this, I hope to provide a clearer basis from which to develop my own detailed, personal, views in later chapters.

I shall undertake this task by arguing for two propositions. It shall help my later exposition if I present my views on the "limits" of the importance of epistemology for education first.

2.3.1. Proposition 1: 'Epistemology is neither a necessary nor a sufficient condition for philosophy of education'  

Some philosophers of education have argued that endorsement of a particular theory of knowledge (metaphysics, ontology) implies a direct or rigorously specifiable (e.g. formally deducible) commitment to a particular set of educational values or policies; and conversely, that epistemological commitments must appear amongst the basic premises of any philosophy of education.

Other philosophers of education, however, have argued strongly against such views. Thus, for example, Frankena (1970) argues against the sufficiency of epistemology for determining solutions to educational problems by considering the views of an author who seems to imply this:
'Phenix writes,

'The view one takes of the subjective-objective aspects of knowledge has significant bearing on the learning process. If knowledge is completely objective, learning consists in becoming conformed to what is outwardly true. The mind must then be repeatedly impressed with the nature of external things. Drill, memorization of well-established information, careful observation, and constant checking of facts would be some of the means of molding the understanding to agree with what is objectively so.'

This sounds plausible. But the conclusion about methods of learning and teaching does not actually follow. Even if knowledge is objective, it does not follow that repetition, drill, memorizations, etc., are necessary. Whether such methods are necessary or not depends on whether such methods are in fact needed to bring the mind to the point of conformity with outward fact, whatever that is, and this is an empirical, psychological question, not an epistemological one.

In fact, no epistemological theory can suffice, by itself, to provide a basis for drawing a conclusion about what ought to be taught or studied. Such a conclusion requires a normative or value premise as well as an epistemological one. Suppose we hold that music is not knowledge. Does it follow that it should not be taught? Not unless we also accept the normative premise that only knowledge should be taught. Or suppose we believe that mathematics is tautological. Does it follow that it should or should not be taught? That depends on what we take to be the value of such tautological knowledge.'

(Frankena, 1970, p. 19: original emphasis).

And then against the necessity in this respect:

'Thus epistemological theories are not sufficient to establish a conclusion about education. Are they necessary for doing so? Do we, for example, need an epistemological theory to help establish that mathematics should be offered in a liberal arts college? I cannot see that we do. All we need is some nonvocational line of argument to show that an offering in mathematics is desirable, and it is not hard
to imagine one which involves no epistemology. For example:

Knowledge of mathematics is intrinsically good.
Intrinsically good kinds of knowledge ought to be taught. Therefore, mathematics ought to be taught.

This argument is, no doubt, oversimplified, but it does show that while a normative or value premise is necessary, an epistemological one is not.

Hence, as Aristotle said, it is ethics and politics that determine what is to be studied, by whom, and to what extent—not epistemology.

If epistemological theories are neither necessary nor sufficient to determine answers to questions about education, two things follow. First, there may be disagreement about education between people who agree in epistemology. Even if their theory of knowledge is the same, they may use different factual or value premises and so come to different educational conclusions. Second, there may be agreement about education between people who disagree in epistemology. For instance, people may agree about the place of mathematics in the curriculum even though they have different views about its nature.

What has been said may suggest that epistemology has no relevance to questions about education. But this does not follow. Even though epistemological theories are neither necessary nor sufficient to determine the answers to questions about education, they may still constitute good reasons, or at least relevant considerations, for or against such conclusions. It may still even be that given certain value premises and certain factual assumptions, they are decisive in determining what to teach. For example, if one believes that religious doctrines constitute knowledge (an epistemological theory), then given that all kinds of knowledge should be taught, at least on the elective plan, and that religious knowledge can be acquired by teaching and only by teaching, one may and presumably will conclude that theology should be taught.'

(Frankena, 1970, p. 20: original emphasis).
Whilst I find Frankena's arguments, above, to be both compelling and exemplary in their clarity, I do not also share his sympathies (which he propounds elsewhere in his article) for analytical philosophy of education. In Chapter 5, I shall argue that such logical independence of epistemology of science and education policy cannot be held to imply independence in a broader or "total" sense and that this latter view results from a commitment to objectivist epistemology which I reject as a general meta-theory.

Once a commitment to a value premise that something ought to be taught has been made, however, the relevance of epistemology to education may then be rather different as I shall now try to show by arguing for my second proposition:

**2.3.2. Proposition 2: 'Epistemology is a necessary, though not a sufficient, condition for pedagogy'**

In my formulation of Proposition 2, I consciously echo the Platonic insight that the only thing that can be taught is knowledge; and, conversely, that if something is held to be knowledge, then it is teachable. Whilst I consider this insight to be basically sound, I should mention that I perceive this to be the only point of contact or agreement between Plato's system of thought and the views that I shall attempt to develop in this thesis. This is because Plato's elaborated epistemological views and educational values differ markedly from my own (see Chapter 10).
In light of Proposition 1, I believe that a compelling and generally applicable case for Proposition 2 may be made by arguing for three relationships between epistemology and pedagogy which are themselves intimately interrelated:

2.3.2.1. The Triadic Relationship Implied by 'Teaching'.

It has often been pointed out (e.g. Smart, 1972; Passmore, 1980) that the verb 'to teach' has two direct objects: logically, if not grammatically, all 'teaching' implies a triadic relation which, in its least formalised version, is of the form 'A teaches B to C', where 'A' = somebody who teaches, 'B' = something that is taught, and 'C' = somebody who is taught.

This logical relation does not constitute a formal definition of 'teaching' - attempts at which I believe to be pointless in principle and to fail in practice. Nevertheless, its utility does seem to lie in drawing our attention to otherwise implicit assumptions concerning the prerequisites for, and perhaps even the dynamics of, 'teaching'. These would seem to be at work in all but the most rare or idiosyncratic meanings of the term.

The familiar inference made from the triadic relation in 'teaching' is that two types or dimensions of 'knowledge' are essential for 'teaching', viz. for somebody - 'A', to teach, or try to teach,
something - 'B', to somebody - 'C'; 'A' must 'know' (in some way) both 'B' and 'C'. A teacher must 'know' both what they are teaching and who they are teaching.

I contend, however, that for any theory of teaching the triadic relation implies a requirement for something more than 'knowledge' in each sense, viz. 'meta-knowledge'. This shall inform, as a proper part, what Tomlinson (1981) has identified as a fourth element to the relation which, I contend, is essential in formal educational settings, namely, a 'learning/teaching process' (p.4, original emphasis).

With respect to 'knowledge' in the first sense, i.e. knowing something ('B'), the usual pedagogic inference may be restated as the view that before one can teach something to somebody it is necessary to know something, e.g. a subject or discipline (i.e. 'knowledge'). I point out, however, that before one can know that one is teaching something to somebody (or can attribute this, with reasons, to somebody else) it is necessary to know what it is to know something (i.e. 'epistemology') - for how else is one to judge? Thus epistemology is a necessary condition for pedagogy. It is not, however, a sufficient condition for pedagogy - this for reasons discussed in section 2.3.2, above. The "knowledge-hood" of 'knowledge' in this first sense has traditionally been the pre-eminent, though not exclusive, concern of philosophers (or more specifically, epistemologists). But as Smart reminds educationalists
'It is essential [] that we try to understand something of the nature of knowledge, for to some extent what we are teaching must influence how we teach.'

(Smart, 1972, p.11: original emphasis).

Knowledge in the second sense, i.e. knowing somebody ('C'), is not devoid of epistemological import - quite the contrary (cf. chapters 3, 4, 5). It has, however, traditionally been the pre-eminent, through not exclusive, concern of psychologists.

Now, I do not deny that one can teach with "only" 'knowledge' in each of the two senses. But for one ever to teach by intention or 'to know' (claim with reasons) this of ourselves, or of somebody else, one must have criteria for 'knowledge' in each sense. Such criteria are, at least partially, derivative of epistemology and their articulation is essential if one wishes ever to improve (or appraise) one's teaching efforts in the light of experience or to suggest guidelines and hints for good teaching practice for the benefit of others. All these things are precisely what pedagogues take as their main concerns.

Now, in the course of this thesis I shall argue that each dimension of knowledge (and meta-knowledge) cannot fruitfully be considered in complete isolation from the other, though they may sometimes usefully be distinguished. I contend that it is only in recent years that educationalists have returned to a recognition and exploration of the interdependence of each sense of knowledge and that this return has been by a different route to that of Plato.
Prior to this, many theorists of teaching tended to emphasise the importance of one type of knowledge - to the exclusion, or virtual exclusion, of the other. I believe that there are many reasons for this and that some are profound, others are trivial; some are complicated, others are straightforward (cf. sections 2.3.2.2, 2.3.2.3, below; Chapters 3, 4, 5, 10).

One reason for this divergence which may, however, be considered relatively straightforwardly (but which may not, as a consequence, be held to be necessarily without profound implications concerning the relationship between language and reason) is a fact to which I have already alluded, namely, the triadic relation in teaching is not immediately apparent in the grammar of our language.

Passmore (1980, p. 22) lucidly discusses this point by suggesting that 'teaching' is a (grammatically) 'covert' triadic relation as opposed to an 'overt' triadic relation. By way of illustration, he argues that the statements 'he teaches', 'he teaches arithmetic' and 'he teaches backward children' are all intelligible just as they stand, whereas the same cannot be said for the verb 'to give' and its derivations.

What I am suggesting here is simply that the grammatical covertness of the triadic relation in 'teaching' has resulted in a linguistic ambiguity which has, in turn, facilitated a delay in the critical recognition of the essential incompleteness of traditions which have emphasised (or may be argued to have been exclusively
concerned with) only one half of the relation, viz. either 'A teaches B' or 'A teaches C'. I suspect, however, that more than a mere "grammatical accident" has been at work on this issue and that this divergence may more fully be explained by reference to educational values and complementary epistemological commitments (see Chapters 5 and 10).

2.3.2.2. The Link between Epistemology and Theories of Learning

However it be understood, 'teaching' is concerned with the "achievement of learning", however that be understood. (Where teaching is unsuccessful this may at least be accepted to have been its principal aim).

Now, the popular meanings of 'to learn' and 'to know' would seem to be intimately related — and rightly so. Hilgard and Bower (1975) provide one demonstration of this by quoting meanings for each of these verbs from an influential dictionary (viz. the American Heritage Dictionary) and then speculating upon similarities and other relationships between them:

'Dictionaries help systematize the way concepts are used in everyday life, and such definitions illustrate that learn and know are closely related in their primary senses. To learn means "to gain knowledge through experience" but one of the meanings of "experience" is "to perceive directly with the senses" which, of course, appears initially in the definition
of know. So we complete one side of the chain. But knowledge is defined, among other things, as learning (erudition) and as familiarity or understanding gained through experience, and learning is defined as acquired knowledge. So we come full circle.'

(Hilgard and Bower, 1975, p.2: original emphasis).

The popular meanings of 'to learn' and 'to know' would thus seem to be intimately related or even inter-dependent. Yet there would seem to have been little recognition of this in "formal", "academic" theories of learning and theories of knowledge. As Holland complains, in most formal considerations of epistemology,

'You would be hard put to find [] any investigation of the concepts of education, teaching, learning and enquiry; and if you had been trained as a student of philosophy in the most famous of British universities, it might not have occurred to you to expect such a thing or deem its absence any loss.'

(Holland, 1980, p. 11).

So, once again, the issue arises: why and how have theories of teaching, generally speaking, become separated? Having introduced the idea of 'learning' into the discussion, I believe that I can now be a little more specific in my answer to this.

The epistemologists' contribution to 'knowing' has been in the first sense, i.e. knowing something, and this has been primarily construed in terms of the epistemic appraisal of mental "products" rather than "processes".
The psychologists' contribution to 'knowing', by contrast, has been mainly in the second sense, i.e. knowing somebody, and this has been construed mainly in terms of investigation of mental "processes" rather than "products". In an educational context, "knowing somebody" has tended to be interpreted as "knowing how somebody learns".

The divergence between the study of the two senses of knowing may have been amplified by, on the one hand, the long-standing infatuation by epistemologists with linguistic analysis and 'Correct Method', and, on the other hand, the psychologists' traditional interest in the philosophy of mind and with the understandable (though in some traditions, I contend, excessive) concern with maturational factors in learning since teaching has traditionally been mainly concerned with the achievement of learning in children rather than in adults.

I suggest that these differences between epistemologists and psychologists - their historical origins, their methods and traditional foci of interest - have tended to obscure their very considerable common ground, viz. for all their real and apparent differences, theorists of knowledge and theorists of learning share a fundamental concern with the growth of knowledge. Consequently, an elaboration of one is always at least a partial elaboration of the other.
"Learning", as we have seen, implies achieving knowledge (whether "created" or "acquired") and this must, in turn, imply some kind of growth in the knowledge of the learner. Similarly, 'teaching' implies at least some prior learning on the part of the teacher for, as Smart puts it

'[...] we have to learn something to teach before we are in a position to be able to teach it."

(Smart, 1972, p.7: original emphasis).

Every epistemologist is committed to the possibility of the growth of our knowledge and so, one way or another, is also committed to the possibility of learning. To explain the first part: no epistemologist questions whether knowledge (knowledge of some kind) is possible. That is to say, no epistemologist assumes that we know, or could know, nothing in the course of our individual or collective lifetimes. Equally, however, no epistemologist assumes that we know, or could know, everything at the moment of our personal birth or conception. Each epistemologist may therefore be said to propose, further articulate or criticise an explanation(s) for the fact of the growth of our knowledge.

The prime question for any epistemologist is 'How is knowledge possible?' (not 'Is knowledge possible?'). In their attempts to answer this question, however, epistemologists have somehow to avoid simply begging it ('knowledge is possible: the world is as it seems and seems as it is'). For the epistemologist, it is not
what you know but how you know it: epistemology is 'theory of (how growth in) knowledge (is possible)'.

2.3.2.3. The Link between Epistemology and Methodology

I contend that any epistemology has a methodological component. By this I wish to claim more than that epistemology and methodology overlap (although I suspect that, strictly speaking, this is their class relation), viz. that in a crucial sense, epistemology may be said to include methodology.

To explain, the epistemologist's central concern with the growth of knowledge has the consequence that any theory of knowledge that is worthy of the name must include a "mechanism" (rationale) by which knowledge claims may be appraised. Any description of such a mechanism, however, constitutes the most important part of any method.

In so far as a distinction between epistemology and methodology is valid, the former may be said to furnish us mainly with premises and arguments to do with the ultimate nature of reality (ontology) and for the possibility of knowledge; the latter mainly with complementary proposals and criticisms of rules and procedures for the achievement of knowledge, i.e. method(s).
Methodology "operationalises" epistemology: it renders epistemology usable. The utility of a particular methodology has, in turn, come to be seen as amongst the most important causes for credibility of a particular epistemology. No method, even those judged to be lowly or routine, can be said to be fully understood or can even be gainfully applied (albeit, the latter sometimes only in the very long term) without recourse to, and endorsement of, its intrinsic epistemological commitments (cf. my distinction between "strong" and "weak" senses of method, methodology, in Chapter 5).

Now, I am aware that my characterization of methodology - its role and relation with(in) epistemology - is most adequate with respect to epistemological traditions of the general sort which I prefer and of which I have yet to elaborate in detail my preferred version, viz 'constructivist' traditions (see Chapter 3). This notwithstanding, I judge my general point still to be reasonable, viz., any epistemology is, in some crucial sense, "methodology incorporating". (For traditional 'rationalists', methodology is epistemology in formation; for traditional 'empiricists', methodology is epistemology; for modern 'constructivists', methodology is epistemology in application.)

The relevance of my preceding arguments for pedagogy may be seen if one accepts that an important part of all influential theories of teaching (past and present) has been methodology of teaching and an important part of all such theories of educational research has been methodology of educational research.
Why should an epistemological understanding of methodology be desirable, even necessary? After all (it might be argued), it is surely possible successfully and consistently to implement a teaching or educational research method without any such understanding? Not so: to implement or develop a method without appreciating its epistemological commitments is to beg the basic epistemological question 'How is knowledge possible?' because it merely assumes the presupposition of both a reality to be known and the organism's possibility of knowing it - and this leads to confusion as to the fruits of such implementation or development.

As von Glaserfeld puts it

'It does not help [], to present the problem [of knowledge] in crisp terms and then to say, as Attneave [] recently did, that its solution is the business of epistemologists and that he, the psychologist, prefers to go on believing that knowledge does reflect on "existing" reality. Such an act of faith is a rationally unfounded assumption and will sooner or later interfere with the investigator's logic of theory construction and, hence, also with his interpretation of "data".'

(von Glaserfeld, 1979, p. 114 : original emphasis).

Nor, I contend, could one implement or develop a method whilst remaining epistemologically "agnostic" - not without also remaining "agnostic" with respect to the results (whether achieved or anticipated) of implementing or developing that method. In summary of these arguments, I concur with Lamb's complementary conclusion
Important research requires both adequate conceptualization and soundness of methodology; neither is, in itself, sufficient.

(Lamb, 1976, p. 415: original emphasis).

and with Cawthron and Rowell who comment that it is a conceptual fallacy to try to separate epistemology and methodology. It is a mistake, for example, to try and adopt the Piagetian distinction between 'concrete' and 'formal' thinkers for methodological purposes without realising Piaget's underlying epistemological pre-suppositions; or to adopt the Popperian or Kuhnian views of scientific progress at their face value without thinking about their implications for science teaching methodology.


The illustrations that these last authors employ, however, direct attention not only to the pedagogic significance of epistemological commitments but also an issue which until now I have introduced somewhat covertly into my discussion, viz. the plurality of epistemological traditions. This is a subject which I shall examine more directly within my next section.

2.4. Philosophy of Science and Science Education

I have so far referred to both 'knowledge' and 'epistemology' in only a "global" sense. When one considers specifically 'scientific' knowledge in the light of epistemology, however, this
Introduces special qualities and difficulties to the task. These have mainly to do with the traditionally and pre-eminently high valuation placed by scholars and general public alike upon the (perceived) achievements and activities of 'science' and which I briefly discussed in Chapter 1.

Having recently drawn attention to the epistemologist's prime concern with the growth of knowledge, however, I can now identify this as the judgement underlying the high valuation of 'science'. This is often discussed in terms of 'progress' in knowledge.

For example, George Sarton, one of the most celebrated historians of science this century claims that

'The history of science is the only history which can illustrate the progress of mankind. In fact, progress has no definite and unquestionable meaning in other fields than the field of science.'

(Sarton [1936] 1965)

A similar view is propounded by Karl Popper whom I judge to have been the most influential philosopher of science so far this century. In his preface to the first English edition (1959) of his now classical work *The Logic of Scientific Discovery* (1934), Popper argues that one of the principal theses which he proposes and shall explore in the remaining text is that
'The central problem of epistemology has always been and still is the problem of the growth of knowledge. And the growth of knowledge can be studied best by studying the growth of scientific knowledge.'


And elsewhere he claims

'[I]n science [] we can speak clearly and sensibly about making progress there. In most other fields of human endeavour there is change, but rarely progress (unless we adopt a very narrow view of our possible aims in life); for almost every gain is balanced, or more than balanced, by some loss. And in most fields we do not even know how to evaluate change.

Within the field of science we have, however, a criterion of progress: even before a theory has ever undergone an empirical test we may be able to say whether, provided it passes certain specified tests, it would be an improvement on other theories with which we are acquainted.'


Popper likewise identifies this view as a 'thesis', additional but complementary to that concerning the growth of knowledge, which he proposes to develop and defend.

Now, Mary Waring relates such judgements to science education in the following way:

'The idea of science as a 'magnificent human achievement' motivated by a disinterested search for truth and requiring bold use of the imagination and, subsequently, of severely critical testing, has given science a strong claim to be recognised as a humane study. If, however, the claim is to
be used as a justification for science education, then science teaching must reflect it in some way.'

(Waring, 1979; p.45: original emphasis).

There would seem, then, to be a good reason for supposing that our judgements as to the nature and worth of science - our 'idea of science' - should or must influence our (theory and methodology of) science teaching. Such judgements, however, presage or adumbrate a commitment to a particular 'philosophy of science'.

Klemke et al offer a useful and, I believe, fair 'preliminary characterization' (as they describe it) of the way in which philosophy of science has broadly been conceived and pursued in the 20th century:

'By one widely held conception, philosophy of science is the attempt to understand the meaning, method and logical structure of science by means of a logical and methodological analysis of the aims, methods, criteria, concepts, laws, and theories of science.'

(Klemke, Hollinger and Kline, 1980, p.2)

These authors later go on to compare and contrast philosophy of science to science itself:

'Whereas science is largely empirical, synthetic, and experimental, philosophy of science is largely verbal, analytic, and reflective. To be sure, in the works of some scientists - especially those who are in the more "theoretical" sciences - verbal, analytic and reflective features may be found. But the converse is not generally true. The activities of philosophers of science are, for
the most part not empirical or experimental, and they do not add to our store of factual knowledge of the actual world. And even in those cases where the more "philosophical" activities are found in science, they are usually not pursued with the same rigour or toward the same ends as they are by philosophers of science.'

(Klemke, Hollinger and Kline, 1980, p.5-6).

The 'ends' toward which philosophers of science direct their efforts, alluded by Klemke et al in the last quotation, are both various and the subject of an intense ongoing debate. In the broadest societal terms, however, the importance and usefulness of philosophy of science may be said to reside in the fact that it provides us with the only means by which we can 'know' (i.e. know with reasons) the activities of 'science' indeed to be such, albeit, in an otherwise undifferentiated sense. That is to say, philosophy of science furnishes us with a set of principles or criteria by which we can judge an activity, a line of research or a knowledge claim to be scientific. Those who seek to legitimize, demarcate or identify a knowledge claim or an investigative activity as scientific must explicitly elaborate and/or tacitly invoke philosophy of science.

Given my earlier arguments in this chapter, the pedagogic significance of philosophy of science is, perhaps, most apparent in the epistemological component or dimension of philosophy of science. At this point, however, I should mention my departure from 'Positivistic' traditions in philosophy of science and their...
heirs. Although I shall present my detailed reasons for this
departure in Chapters 3 and 4, a summary of them may be helpful
now. I contend that until very recent times the historical
development of philosophy of science has been dominated by
Positivistic traditions. Such traditions, however, have drawn from
only two of the main divisions and sub-divisions of philosophy
which I mentioned earlier, viz. *logic* ('analysis') and *epistemology*
— with the former in the service of the latter. Moreover,
throughout the development of their tradition, Positivists have
made strenuous attempts *systematically to exclude* all remaining
areas of philosophy. Viewed historically (i.e. as a whole),
therefore, 'philosophy of science' might better be called
*epistemology of science*: the other title seems overgenerous. I
shall argue that the Positivists attempts so to reduce and confine
philosophy of science cannot be sustained and are, given also my
endorsement of the traditional meaning of 'philosophy' (viz., 'Love
of wisdom'), profoundly unwise. This notwithstanding, I shall
continue to use generically the expression 'philosophy of science'
both as a courtesy title and out of a sense of personal commitment
and intent.

Returning to my theme, I agree with Elkana who argues that

'It is well known that there is a strong interaction between
the philosophy of science and the science of each
generation. It is less often stated clearly that there is
also an interaction between these two and the teaching
of science in so far as it is the philosophy of science which moulds the general attitudes which form the foundations of the various theories of science teaching.'

(Elkana, 1970, p.15 : my emphasis)

and that

'By philosophy of science, I mean not only the conscious formal philosophy which deals with science but also the scientist's views on the structure of the world, and his informal opinions about science.'


Elkana's second, 'informal', sense of philosophy of science might be said to shift emphasis to the principles and criteria by which a particular person effects science.

Now the context surrounding Elkana's remarks which I have recently quoted makes it clear to me that he sees no necessary antagonism between these two meanings of 'philosophy of science', indeed, quite the contrary. Nor do I. Yet, over the last 25 years or so, there has been a marked proliferation of substantial, rival, traditions in 'conscious, formal' philosophy of science and I would suggest that the differences and disagreements between protagonists have been primarily over the roles of, and relations between, these two meanings and the actual conduct of science. There is now no semblance of an orthodoxy within 'philosophy of science', construed as a discipline. Outstanding and, to greater and lesser extents, mutually incompatible traditions making a direct contribution to contemporary philosophy of science inlude those due
to Popper, Kuhn, Lakatos and Feyerabend. (I shall discuss in some
detail the ideas of the last of these four philosophers in Chapters
3 and 4).

If, as I have suggested, many of the traditional, basic,
assumptions and judgements as to the nature and worth of science
are in dispute within philosophy of science, then how – recalling
Waring's comment, quoted above – should (theories of) science
teaching 'reflect' science? Indeed, how may we defend the view
that 'science' ought to be taught at all?

To take the second point first, if it is accepted that, however we
each may know 'science', 'science as a whole has been integrated
into the system of human needs, their administration, and
satisfaction' (Weiland, 1981, p.618); and if it is accepted that
education has amongst its principal aims the enablement through
teaching of persons to satisfy their personal needs within a social
context, then 'science' must continue to be taught.

With respect to the first point, it would perhaps be wiser to start
with a more modest valuation of science. Here, I endorse a view
expressed by Paul Feyerabend:

'To start with I assume that my readers agree about progress
and good science and that they do so independently of
whatever rules or standards they adopt. For example,
I assume that they applaud the gradual acceptance of the idea of the motion of the earth or of the atomic constitution of matter in the late 19th and early 20th centuries independently of what rules and standards they think it obeys.'

(Feyerabend, 1978, p.13).

and, thence, another suggestion by Elkana:

'As all of us, scientists, teachers, philosophers, educators share the fundamental belief that science is 'good' and aims at maximum scientific knowledge for a maximum number of students, we should also aim at grounding our theories of science teaching in that philosophy of science which at present seems to us the most advanced.'


In this chapter I shall begin my personal defence of, and elaboration upon, such views as have recently been expressed by myself and others within my articulation of the model or framework which informs my entire investigation:

2.4.1. A Model for the Transformation of Scientific Knowledge in Formal Educational Settings.

Before I describe my specific model I shall consider the general case for using models in educational research.

Doran (1978) has argued strongly in favour of the use of such models, seeing each one as 'a conceptual framework describing the relationships amongst key variables present in the field of enquiry' (p. 423). He makes 4 main points. First, the use of
explicit models help clarify the initial planning of research efforts. Second, models help organize data collection and assist in the formulation of variables and hypotheses. Third, a model may help to clarify the nature of individual research efforts. Fourth, lack of models limits the impact of educational research on classroom practice and may allow the implementation of incoherent (internally contradictory) teaching programmes by default.

My model represents an augmented version of one presented in a seminar by Zylbersztajn (1980; described in written form in his Ph.D thesis of 1983). Zylbersztajn's model, in turn, incorporates certain terms and ideas developed by others of our colleagues. For clarity of exposition, and to make clear my considerable intellectual debt to all these persons, I shall first describe Zylbersztajn's model, beginning with its components.

The expressions and their specified meanings with which Zylbersztajn began to construct his model were created in light of a (then) broadly construed constructivist epistemological perspective and the pedagogic judgement that students represent the chief protagonists in any formal teaching and learning event (Zylbersztajn, 1983, Chapter 1).

Osborne (1980) suggested the expression 'Children's Science' (Sch) to describe those views of the world (composed of beliefs, expectations and meanings for words) held by pupils prior to
encountering their scientific counterparts, namely, 'Scientists' Science' (Sg), in formal science education.

In a paper written later in 1980, Gilbert, Osborne and Fensham (1982) argue that the notion of 'Children's Science' represents a rejection of the 'Blank Minded' or 'Tabula Rasa' assumption which may be inferred still to underly modern curricula (Fensham, 1983) and which 'assumes that the learner has no knowledge of a topic before being formally taught it' (Gilbert, Osborne and Fensham, 1982; p.623).

Out of similar constructivist considerations, these authors propose the expression 'Teachers' Science' (St) to connote the teacher's viewpoint or perspective in specific science topics as presented to a group of pupils in a formal educational setting.

Starting from these three notions, i.e. SCh, Sg, St, Zylbersztajn attempts to provide 'a more complete picture of the transformations and interactions between different forms of knowledge in the context of secondary science education, (Zylbersztajn, 1983; Chapter 1, p.7) by introducing two additional expressions, viz. 'Curricular Science' (Scr) and 'Students' Science' (SSt). These he articulates within a generalised sequence of transformations for all five expressions and which he depicts by means of a diagram:
The nub of Zylbersztajn's written description of this sequence is as follows:

'In a first stage, "scientists" science (S_s) is transformed into "curricular science" (S_{CR}), in a process mediated by the action of curriculum planners (e.g. textbook writers; members of curricula development projects). Science curricula, either in their simplest forms (e.g. a textbook) or in their more refined versions (e.g. as an integration of printed material, AVA [Audio Visual Aids], and laboratory materials, plus teacher's guides) are here conceived of as structures representing versions of scientific knowledge.

The second stage of transformation occurs when a curriculum is implemented by a particular teacher, concerned with a particular group of pupils, in a particular school.
One of the assumptions of this study is that teachers interpret the structure of a curriculum in the light of their own conceptual structures and their perception of situations they are involved in. Therefore, what is conveyed by them to their pupils — "teacher's science" ($S_T$) — can be seen as a result of the interaction between teachers and "curricular science", in a specific context.

The third stage of transformation takes place in science courses, when pupils perceive, interpret and process what is presented to them, constructing their own personal meanings from the activities they are asked to perform. It is in that process that their previous knowledge — "children's science" ($S_{Ch}$) — seem to play an important role. Those activities are conceptualized in the framework as the interaction between "children's science" and "teacher's science", the result of which is named "students' science" ($S_{St}$)." (Zylbersztajn, 1983, Chapter 1, P.8: my emphasis).

Zylbersztajn is careful to stress that his conceptual framework represents only 'a simplified picture of the complex reality it is intended to represent' (1983, Chapter 1, p.9): he points out that, for example, teachers may complement the curricular materials they have selected for their lessons with information extracted from other sources. Similarly, pupils may interact directly with curricular and other sources of information and this is especially likely in the case of individualised learning schemes. These caveats notwithstanding, he contends in conclusion that his model provides a useful and distinctive first approximation of a means by which major transformations of knowledge occurring in the context of secondary school science education may be conceptualised (ibid.).
My additions to Zylbersztajn's model are, in turn, made in a dual attempt, on the one hand, to take better account of my focus of research interest and personal brand of constructivist perspective which are slightly different to his, and, on the other hand, to identify the broad continuity between his earlier investigation (and those of other colleagues) and mine. To explain, Zylbersztajn's investigation, as with most so far conducted within the Alternative Conceptions Movement (ACM), is primarily concerned with students' alternative conceptions in science (or, perhaps more accurately, in 'curricular science') - in Zylbersztajn's study, specifically, light and colour, force and movement. My investigation, by contrast, is primarily concerned with curricular philosophy and students' alternative conceptions of science - their "personal epistemologies of science".

I have tried to reflect these differences of interest and emphasis in my additions to both the components and dynamics of Zylbersztajn's model, as depicted in the following diagram:
Where

\[
\begin{align*}
\rightarrow & \quad = \text{strong influence} \\
\leftarrow & \quad = \text{weak influence}
\end{align*}
\]

\begin{align*}
S-C-P & = \text{Scientists' Constructive-Principles} \\
S_S & = \text{Scientists' Science} \\
P-C-P & = \text{Philosophers' Constructive-Principles} \\
P_P & = \text{Philosophers' Science} \\
C-C-P & = \text{Curricular Constructive-Principles} \\
C_C & = \text{Curricular Science} \\
T-C-P & = \text{Teachers' Constructive-Principles} \\
T_T & = \text{Teachers' Science} \\
S_{St} & = \text{Students' Constructive-Principles} \\
S_{St} & = \text{Students' Science} \\
S_{Ch} & = \text{Children's Constructive-Principles} \\
S_{Ch} & = \text{Children's Science}
\end{align*}

**Figure 2.2** A Model for the Transformation of Scientific Knowledge in Formal Educational Settings
My first modification to Zylbersztajn's model is to add another 'stage of transformation', identified by my expression 'Philosophers' Science' (Swift, 1981). I intend this term to represent the corpus of results of the efforts of 'philosophers of science' to identify and understand, by philosophical means, the aims, method(s) and defining characteristic(s) (if any) of scientific enquiry and of growth in scientific knowledge (cf. my description of 'philosophy of science' earlier in this chapter).

By 'philosophers of science' I mean not only professional philosophers of science but also scientists who speculate philosophically about the nature of science, often beyond (but including) their own area of specialisation. Amongst the latter, however, there are not only those working or retired scientists who write overtly philosophical works about science (e.g. Einstein, 1934; Medawar, 1969; Eccles, 1970; Capra, 1983), but also those scientists who are seconded from science in order to contribute to the development of a particular curriculum project (e.g. Professor Frank Halliwell for Nuffield 'O' level Chemistry, see Waring, 1979) or who turn their attention to issues of science pedagogy, however broadly they may be construed (e.g. Ziman, 1980).

Now I recognise that my addition of this extra stage of transformation to Zylbersztajn's sequence shall be judged by many to be either mistaken or extravagant, that I have articulated
the situation that I anticipate (and would like to see) developing. Yet I believe Philosophers' Science to be a prior and necessary condition for Curricular Science and not merely a desirable addition informing curriculum planners decisions - which latter I judge to be the more widely held view.

Now, I cannot attempt to present a compelling case for this view until later in this thesis for it first requires close reference to my detailed epistemological convictions (q.v. Chapters 3, 4, 5). Briefly, however, I do not believe that a science curriculum could ever be planned or implemented without recourse to (or implicit adoption of) criteria for the content of Scientists' Science - this prior to any selection from it (which might require different criteria) - and such criteria are at least partially constituted of what might be identified (if necessary by others) as 'epistemology of science', however broadly that be understood. Furthermore, if my earlier arguments for the link between theories of learning and theories of knowledge are recalled (q.v. section 2.3.2.2, above), then a case may be made for the view that Philosophers' Science functions (either in whole or in part) as a 'theory of science learning' in formal educational settings.

I contend that part of the problem of creating an "appropriate" curricular response to contemporary philosophy of science, i.e. along the lines suggested by Elkana (q.v. quotation, section 2.4, above) is achieving a general recognition by science
educationalists that plurality of traditions in philosophy of science does indeed create a problem for (theories of) science teaching. In later chapters I shall pursue my case that, rightly understood, the present situation in philosophy of science has both brought into sharp relief and rendered more problematic the necessary importance of philosophical assumptions and commitments, mainly of an epistemological kind, in mediating all teaching and learning in science. The present absence of orthodoxy in philosophy of science poses many novel problems for educationalists who are keen to respond. Such problems, which also herald many new and desirable possibilities, have mainly to do with issues of choice and emphasis. The pertinence and influence in the classroom of all these philosophical issues may be illustrated with an example described by S.P. Kanagy:

'In fact, philosophical assumptions and their implications permeate cosmology. In the area of quantum mechanics, it is difficult for students to comprehend why the indeterminancy principle is believed by many physicists without understanding at least the elementary ideas of such philosophical views on logical positivism. Should we teach only the Copenhagen interpretation of quantum mechanics? What about the "many worlds" interpretation? The "Hidden Variables" interpretation? In the discussion between Planck and Einstein concerning the reality of quanta, the two disagreed on what should be regarded as the "best" interpretation of statistical mechanics.'

(Kanagy, unpublished manuscript, 1981a, p.13 ; original emphasis)
It is, I think, sadly corroboratory of my recently expressed views that Kanagy has yet to succeed in getting this and other, similar, manuscripts, written in the early 1980's, accepted for publication (personal communication: letter dated 30th July 1986; see, however, Kanagy, 1981, for a tantalizingly brief published abstract of a paper in similar vein).

The need for conscious and conscientious consideration of epistemological issues in curriculum planning and in the classroom has been rendered all the more urgent by the fact, mentioned in Chapter 1, that the majority of the science curricula developed this century have included with increasing priority 'the process of scientific investigation and reasoning' (Fensham, 1983, p.5) amongst major teaching and learning objectives.

Kanagy's example of teaching quantum mechanics, however, does cast additional light upon the problem of formulating and implementing an appropriate curricular response to the present absence of orthodoxy in philosophy of science that I have claimed to exist.

To explain, whilst few physicists would now endorse the fundamental tenets of Logical Positivism (q.v. Chapter 4), still less its details, most would still subscribe to a highly related, albeit, usually more vaguely construed, instrumentalist interpretation of quantum theory. To make the same point another way, the many attempted micro-realistic interpretations of the quantum theory,
including constructivist ones (notably, Popper's, e.g. 1967, "propensity" interpretation) still only enjoy a sporadic and controversial acceptance amongst philosophers of science and scientists alike. The problem here is that notwithstanding the lack of orthodoxy in contemporary philosophy of science there is a prevailing rejection of Logical Positivism (or, more generally, of instrumentalism) within the community of philosophers of science.

I shall argue, then, in support of what I shall term a "strong" thesis of philosophy of science in science education which is only partially complementary to a "weak" thesis.

To clarify, by the "strong" thesis I hold not only that if science is considered to be a branch of knowledge, in at least one of its aspects, and if we choose to teach it, then epistemology of science is a necessary, though not a sufficient, condition for any theory of science teaching, but also that no learning of science, at least in the manner intended, can occur unless or until the learner's personal epistemology of science is compatible (congruent, commensurable) with that expressed, whether tacitly or explicitly, by the theory of science teaching and the science curriculum. Different epistemological commitments do have consequences for pedagogy and for learning, albeit not in any strict deterministic or formal deductive sense.
Supporters of the "weak" thesis of philosophy of science in science education, by contrast, hold that whilst philosophy of science ought to be taught explicitly within the science curriculum (e.g. by virtue of increasing science educands' interest in, and perceived relevance of, science), it is neither necessary nor sufficient for a theory of science teaching and for science learning to occur.

I support the view that philosophy of science ought to be taught explicitly but I reject the second clause of the "weak" thesis. In Chapter 4 I shall argue that any apparent plausibility of Zylbersztajn's exclusion of what I have termed 'philosophers' science' from his sequential model stems from, and is disguised by, his explicitly proclaimed sympathies with Kuhn's doctrines.

My second modification to Zylbersztajn's model is to add 'weak influences' in order to bring out the interactive, or two-way, character that I consider these transformations to have and to distinguish these from 'strong influences' (a modification which, incidentally, destroys the implicit left-to-right time line in Zylbersztajn's diagram).

Thus, for example, in a strong (influence or direction of) interaction, certain individual philosophers of science may be argued (or themselves claim) to draw particular inspiration from certain individual scientists, e.g. Popper from Einstein (e.g. Popper, 1978), Feyerabend from Galileo (e.g. Feyerabend, 1975).
Conversely, in a weak interaction, certain individual scientists may be argued (or themselves claim) to draw particular inspiration from individual philosophers of science, e.g. Darwin from Bacon (Darwin, 1887: quoted and discussed by Medawar, 1969, p.11), Eccles from Popper (Eccles, 1970).

Similarly, in a strong interaction, certain individual science educationalists may be argued (or themselves claim) to draw particular inspiration from certain individual philosophers of science, e.g. Zylberstajn from Kuhn (Zylberstajn, 1983), myself from Feyerabend (e.g. this thesis). Conversely, in a weak interaction certain individual philosophers of science may be argued (or themselves claim) to draw critical inspiration and/or corroboration from certain individual psychologists who are influential in curricular contexts, e.g. Capek, 1971, Feyerabend, 1975, p.227 re. Piaget; Maxwell, 1984, p.143 re. Kelly. It is also worth mentioning that some philosophers of science from amongst those who reject (or who equivocate as to) the 'objectivity' of their epistemology reorganise its pedagogic aspect, e.g.

'Sophisticated falsificationism adumbrates a new theory of learning.'

(Lakatos, 1970, p.121n.1: original emphasis).

I contend that the remaining 'weak interactions' are a presently un(der) recognised consequence of a constructivist perspective on education and I shall discuss them in later chapters.
My third modification to Zylbersztajn's model is to add 'constructive principles' to each component of the model.

By 'constructive principles' I mean basic and often implicit organizing ideas underlying and enabling the more specific, detailed and explicit ones characteristic of each component. Such principles might also (in various contexts) be called 'values', 'absolute postulates', 'fundamental assumptions' or 'metaphysics'.

Given the common interest of the components (science) and the interactive relations between them, it would seem reasonable to suggest that there is a corresponding sequence of overlapping clusters of constructive principles. Clearly some constructive principles shall reflect the specific interests and purposes of the group of persons comprising a component. The common interest in science, however, may be expected to result in constructive principles concerning the nature of the physical world to be of importance and interest to each and every component, albeit, perhaps to different extents. Appropriate expressions for such constructive principles might be Agassi's (e.g. 1964) 'scientific metaphysics' or Feyerabend's (e.g. 1978) 'cosmological assumptions'. Cosmological assumptions include such qualities or principles as 'symmetry', 'orderliness', 'simplicity' and 'non-randomness'. These inform more elaborated ontologies such as 'finite universe' (Aristotle), 'world consisting of a continuum of forces, all of them contact forces between parts of matter'
(Leibniz) and 'world consisting of separate particles, all with central forces acting at a distance between them' (Newton). (I shall discuss some possible roles for constructive principles in epistemology in Chapter 4).

Finally, notwithstanding the modification I have made to Zylbersztajn's model, I likewise urge that it still provides only a simplified picture of the complex reality it is intended to represent. I also assume that my modified version of this model may be held to apply more-or-less directly to the tertiary as well as to the secondary science education system. I shall use Osborne's (1980) expression 'School Science' to refer to the combination of Philosophers' Science, Curricular Science and Teachers' Science expressed in formal educational settings.
Chapter 3. Personal Construct Psychology as a Constructivist Theory of Knowledge.

'There are two ways of losing oneself: through fragmentation in the particular or dilution in the 'universal'.

Lettre à Maurice Thorez, Aimé Césaire. (1956)

3.1. Introduction.

My over-arching purpose in this chapter, and the two following it, is to argue for two main views: (1) that the Alternative Conceptions Movement (ACM) is a qualitatively different (and desirable) new approach within educational research; (2) that the ethos, research methods and constructivist epistemology of the ACM are both compatible with, and may best be developed by reference to, those of Personal Construct Psychology (PCP).

My broad strategy in this cause shall be to identify and articulate a number of fundamental similarities between PCP, ACM and recent trends and developments in philosophy of science and to contrast them with influential alternatives. With my remarks of Chapters 1 and 2 in mind, I shall concentrate in this chapter upon drawing similarities and contrasts between epistemological commitments.

3.2. A Taxonomy for Theories of Knowledge (Lakatos).

As a central framework for my arguments I have taken a classificatory scheme for epistemological traditions developed by Lakatos.
There is an important demarcation between 'passivist' and 'activist' theories of knowledge. 'Passivists' hold that true knowledge is Nature's imprint on a perfectly inert mind: mental activity can only result in bias and distortion. The most influential passivist school is classical empiricism. 'Activists' hold that we cannot read the book of Nature without mental activity, without interpreting them in the light of our theories. Now conservative 'activists' hold that we are born with our basic expectations; with them we turn the world into 'our world' but must then live forever in the prison of our world. The idea that we live and die in the prison of our 'conceptual framework' was developed primarily by Kant; pessimistic Kantians thought that the real world is for ever unknowable because of this prison, while optimistic Kantians thought that God created our conceptual framework to fit the world. But revolutionary activists believe that conceptual frameworks can be developed and also replaced by new, better ones; it is we who create our 'prisons' and we can also, critically, demolish them'.

(Lakatos, 1970, p.104: original emphasis).

Now, Lakatos does not develop meanings for the components of his taxonomy abstractly; rather, he concentrates upon articulating his 'Methodology of Scientific Research Programmes' (q.v. Appendix 1) apparently as an illustration of a 'revolutionary activist' theory of knowledge. Although I shall later argue that this classification is incompatible with Lakatos' (admittedly equivocal) epistemological claim to provide objective criteria for the growth of knowledge, there is, I think, something discernable in his brief explication of 'revolutionary activism', above, that suggests that an exhaustive, or even an extended, abstract formalisation of this particular meta-meta knowledge claim would somehow fail to refer to the class of epistemological traditions which it purports to elucidate. Accordingly, I shall follow Lakatos' example by providing illustrations-in-application of his taxonomy and shall only propose further abstract refinements to his meta-meta criteria in their contexts of application.
The constraints imposed by linear exposition mean that I shall have to advance details of my argumentative strategy in a "saltatory" manner, sometimes presenting material which may not appear to be strictly relevant or necessary at any given stage of the chapter. This should particularly be borne in mind during my account of the Kantian origins of constructivist epistemology which follows.

3.3. The Origin of Constructivist Theories of Knowledge (Kant).

Immanuel Kant may be said to have originated constructivist epistemology and introduced it to a large audience upon publication of the two editions of his Critique of Pure Reason (CPR) in 1781 and 1787. Where I quote from this work I shall refer only to Kemp-Smith's (1929) translation and without indicating from which edition I have drawn text (both are included in Kemp-Smith's rendering).

The Critique of Pure Reason is the first of a series of four Critiques through which Kant developed his ideas (the remaining three being of Practical Reason, of Aesthetic Judgement, and of Teleological Judgement). The resultant system of thought continues to draw the highest praise from eminent scholars: Medawar, for example, claims it to be '[] the greatest intellectual exploit in the history of philosophy []' (1969, p. 35). Popper similarly pays homage to the impact of Kant's ideas and to the nature of his achievement:

'By emphasizing the role played by the observer, the investigator, the theorist, Kant made an indelible impression not only upon philosophy but also upon physics and cosmology.' (Popper, 1972, p. 181).
The first, epistemological, Critique, is generally regarded as Kant's masterpiece. Whilst I have already drawn some links between epistemology and pedagogy in Chapter 2, let it not be thought that Kant lacked either involvement or respect for educational matters for, as Haack (1976, p. 159) points out, Kant interrupted his work on the Critique of Pure Reason in order to support Basedow's progressive school, the Philanthropin. In a work devoted to the subject, Kant declared that

'[...] the greatest and most difficult problem to which man can devote himself is the problem of education.'


I contend that at a time such as now when there is intense debate - and not a little confusion - amongst "constructivist" educationalists over fundamentals, an appraisal of the origins of constructivist epistemology cannot fail to be of use: indeed, many of Kant's ideas - and the controversies associated with them - may be seen, upon examination, to underlie or resemble those presently discussed, thereby helping to clarify them. My account of Kant's epistemology shall also serve both to justify and to develop Lakatos' 'conservative-activist' classification of it. This shall contribute an important point of reference in the broad strategy of my argument throughout this chapter.

Notwithstanding its acknowledged brilliance, the Critique of Pure Reason is complex, often subtle and riddled with ambiguities - or, as modern scholarship tends to have it, with confusions. In conscious and useful simplification, therefore, Brittan applies a well known framework for rationally reconstructing the genesis of constructivist epistemology and for discussing Kant's ideas:
Almost everyone follows Hegel in thinking that the history of modern philosophy has a nice symmetry about it. Rationalist thesis ("knowledge is based on reason") gives way to empiricist antithesis ("knowledge is based on sense experience"), which in turn gives way to Kantian synthesis ("knowledge is a product jointly of understanding and sensibility"). (Brittan, 1978, p. 3).

Even Kant himself summarises his "synthesis" fairly straightforwardly:

'Without sensibility no object would be given to us, without understanding no object would be thought. Thoughts without content are empty, intuitions without concepts are blind. [ ]. The understanding can intuit nothing, the senses can think nothing. Only through their union can knowledge arise.' (Kant, CPR, p. 93).

Now, the longstanding prevalence of the empiricist influenced "commonsense" theory of knowledge (q.v. Appendix I) means that, even nowadays, it is easy to mistake Kant's "knowledge thesis" for something less radical than it actually is when it is stated thus baldly. To help reduce this risk and to help me later to distinguish Kant's epistemology from other constructivist traditions, I shall elaborate considerably upon the basic points I have already made.

Central to Kant's purpose in elaborating his epistemology was a determination to present a viable alternative to each of the rival solutions offered by Leibnizian rationalists and Humean empiricists to the "problem of objectivity", viz. whether one can have knowledge of the world which is not just knowledge of one's own point of view. The Leibnizians argued for the attainability of objective knowledge by invoking dogmatically an extravagant and obscure metaphysics. As Beck comments
"[I] the Leibnizian epistemology [...] proceeded to solve even the simplest problems by an argument obscurum per obscurius. It was an epistemology which explained the simplest facts learned through sense experience by an appeal to preestablished harmony, and the theorems of physics by appeal to theodicy."

(Boek, 1978, p. 6: original emphasis).

With respect to the problem of objectivity, this did not seem helpful. As sceptical empiricists, the Humeans were in opposition to this tradition: they denied not only all metaphysics but also the attainability of objective knowledge, appealing instead to the pragmatic utilities to be gleaned from the habit of 'custom'. In the face of Newtonian physics, which was at once profoundly metaphysical and outstandingly empirically successful, this seemed tantamount to blasphemy.

Kant's response to this philosophical state of affairs was to propose as fundamental an epistemological problem (echoed in the previous Kant quotation) which was shocking, even nonsensical, to rationalists and empiricists alike for it seemed to juxtapose terms associated with each tradition and which had long been regarded as mutually exclusive, viz. 'How is synthetic a priori knowledge possible?'. To appreciate the radical nature of this proposition it may be useful to consider some meanings for the terms 'analytic' and 'synthetic', on the one hand, and 'a priori' and 'a posteriori' on the other.

The terms 'analytic' and 'synthetic' were coined by Kant to distinguish between kinds of proposition based upon the nature of the evidence required to establish their truth. An analytic truth is essentially a tautology: its truth is guaranteed by the meaning, discovered by analysis, of the terms used to express it. In Kant's
idiom, a proposition is analytic if, and only if, the concept of the "predicate" is included in the concept of the "subject". For example, the proposition "All men are biologically male" is an analytic truth: the predicate- "biologically male" - is already contained within the subject - "men". A synthetic truth, by contrast, is one whose truth is not derived by analysis but by other means and which affirms something in the predicate which is not already contained in the subject. For example, were the proposition "All men enjoy killing" to be true, then the predicate - "enjoy killing"- would tell us something non-trivial about the subject - "men". It would be a synthetic truth because it would not merely reiterate the definition of the term used to refer to men. (See Scruton, 1982, p. 18-19; Flew, 1979, p. 11).

The terms 'a priori' (from the Latin: "what comes before") and 'a posteriori' (from the Latin: "what comes after") are similarly used to refer to propositions upon the basis of how one may acquire knowledge of their truth. An a priori truth is a proposition that can be known to be true, or false, by exercise of reason (mind) alone, i.e. without reference to experience except in so far as experience is necessary for understanding its terms. An a posteriori truth is an empirical proposition whose truth or falsity can only be known by reference to how, as a matter of contingent fact, things have been, are, or will be. (See Scruton, 1982, p. 18-19; Flew, 1979, p. 15).

Thus the prima facie similarity between analytic and a priori truths, on the one hand, and synthetic and a posteriori truths, on the other, asserted by empiricists to be synonyms, explains the
The paradoxical quality of Kant's proposal for certain 'a priori' synthetic propositions.

Kant attempts to resolve this apparent contradiction by means of an ingenious argument (which he calls the 'Transcendental Deduction') for what must be presupposed in experience for experience to be possible at all. The synthetic aspect of what would otherwise be "a priori construction" may be discerned within Piaget's commentary upon this subject:

'[In Kant's view, intellect exhibits] priority in relation to experience: logical priority insofar as it is a necessary condition, as well as priority in part chronological (the a priori can only manifest itself at the moment of experience, and not before, but in all cases not afterward), and above all priority of level, insofar as the subject who experiences already possesses an underlying structure that determines his activities.'

(Piaget, 1965; Eng. trans. 1971, p. 57: original emphasis).

Thus Kant is suggesting neither that reason merely "organises" sense "data" after they have been "received" (which would be a version of empiricist knowledge thesis), nor that sense experience is merely "illustrative" in objective knowledge (which would be a version of rationalist knowledge thesis): his proposal is for a genuine synthesis from reason and experience in which both are necessary for (and constitutive of) objective knowledge. Here, however, much depends upon Kant's personal meaning of 'objective knowledge' ('objective truth') which differs from many modern uses - a point to which I shall return. In ad interim summary, then, for Kant the intellect alone (i.e. 'pure reason') cannot give content to knowledge; however, with (and only with) reference to sense experience it can supply form.
The themes of the Transcendental Deduction are developed and complemented throughout Kant's epistemology, which latter he named 'Transcendental Idealism'. The place of this argument within Kant's overall theory of knowledge, and the remainder of my description of it, may be assisted by reference to Losee's admirable schematization of Transcendental Idealism which he presents in both words and a diagram:

'[Kant] specified three stages in the cognitive organisation of experience. First, unstructured "sensations" are ordered with respect to Space and Time (the "Forms of the Sensibility"). Second, the "perceptions" thus ordered are related by means of such concepts as Unity, Substantiality, Causality, and Contingency (four of the twelve "Categories of the Understanding"). Third, the "judgements of experience" thus formed are organised into a single system of knowledge through application of "Regulative Principles of Reason".'


Figure 3.1. 'Kant's View of Cognitive Experience' (after Losee, 1980, p. 107).
Reading Iosee's diagram from right to left (which does not imply a
time line), when objects of the real world, described in Kant's
phrase as 'Things-in-Themselves' ('Ding-an-Sich') and in his
technical term as 'noumena', come within the purview of the sense
organs of the knowing subject they are unstructured sensations which
the 'forms of intuition', namely, space and time (sensibility),
allow to be intuited as perceptions - 'phenomena'.

Now, understanding (as opposed to intuition) requires judgements as
to the objective character and relatedness of perceptions. Such
judgements are effected by a set of innate 'forms of thought' to
which Kant also gave the title 'The Pure Concepts of the
Understanding, or Categories'. Kant's doctrines of both sensibility
and of understanding are essential components of his Transcendental
Deduction. In distinguishing their origins he also alludes to their
complementary functions and limits (cf. my first Kant quotation,
above):

'The categories are not, as regards their origin,
grounded in sensibility, like the forms of intuition,
space and time; and they seem, therefore, to allow of
an application extending beyond all objects of the
senses. As a matter of fact they are nothing but
forms of thought, which contain the merely logical
faculty of uniting a priori in one consciousness the
manifold given in intuition; and apart, therefore,
from the only intuition that is possible to us, they
have even less meaning than the pure sensible forms.
Through these forms an object is at least given,
whereas a mode of combining the manifold - a mode
peculiar to our understanding - by itself, in the
absence of that intuition wherein the manifold can
alone be given, signifies nothing at all. At the
same time, if we entitle certain objects, as
appearances, sensible entities (phenomena), then
since we thus distinguish the mode in which we intuit
them from the nature that belongs to them in themselves, it is implied in this distinction that we
place the latter, considered in their own nature,
although we do not so intuit them, or that we place
other possible things, which are not objects of our senses but are thought as objects merely through the understanding, in opposition to the former, and that in so doing we entitle them intelligible entities (noumena). The question then arises, whether our pure concepts of understanding have meaning in respect of these latter, and so can be a way of knowing them.'
(Kant, CPR, p. 266-267: original emphasis).

Notwithstanding Kant's misleading use of the term 'idealism' in the title of his epistemology (about which, more later), I contend that his distinction between noumena and phenomena demonstrates fairly clearly that he is a realist. That is to say, Kant subscribes to the commonsense belief that there is an independently existing "real world", a world which may meaningfully be distinguished from our personal and subjective thoughts about it. As Kant's final question in the last quotation suggests, however, he does not let the matter rest there: Transcendental Idealism purports to be a theory of objective knowledge (objective truth).

For Kant, objective knowledge requires both that there be a world and that it be intelligible. A critical examination of experience reveals the limits of possible experience and, by so doing, establishes the general form of any objectively possible world. This is enough to guarantee the reality (real existence) of the latter since a putative world which is beyond even the possibility of experience could not meaningfully be said to be real for it could never be known (be intelligible). The 'possibility of experience' is delineated by Kant's list of 12 categories (substance, causality etc.) - these prescribe what sort of a world the world must be in order that we might ever experience it. Categories are universal: they are innate, fundamental, forms of judgement to be found in all 'subjects of experience' (e.g. persons). Categories mediate,
indeed, are necessary for all 'possible experience' and, hence, for all possible knowledge. In Kant's words

'The possibility of experience is [ ] what gives objective reality to all our a priori modes of knowledge. [ ]. Experience depends [ ] upon a priori principles of its form, that is, upon universal rules of unity in the synthesis of appearances. Their objective reality, as necessary conditions of experience, and indeed of its very possibility, can always be shown in experience.'

(Kant, CPR, p. 193: original emphasis).

Kant thought that his set of categories was sufficient to solve all disputes of traditional metaphysics (or, at least, to supply the key to their solution) and hence to guarantee objective knowledge (or, at least, its eventual achievability).

Brittan (1978) expounds Kant's understanding of 'objectivity' by first discerning two rather different meanings of the term which enjoy currency in epistemology, viz. (1) the requirement of 'objects'; (2) the requirement of 'epistemological security' achieved by appeal to 'a certain kind of evidence' (p.12, 12n.) He then argues that Kant's epistemology can only be identified with the first of these meanings (Brittan, 1978, p. 12n. Ch. 5). I suggest that this "Kantian" brand of 'objectivity' - which henceforth I shall call 'realism' - contrasts with, on the one hand, instrumentalists (who confine themselves to the second meaning), and, on the other hand, classical empiricists and 'objectivist methodological constructivists' (who, I shall later argue, confound both these meanings by attempting to conflate them). I shall refer to Brittan's second meaning of 'objectivity' as 'objectivism'.

It is, no doubt, with something very like Brittan's first meaning of
'objectivity' in mind when Scruton comments that

'If valid, the transcendental deduction achieves a result of immense significance. It establishes the objectivity of my world while assuming no more than my point of view on it.'
(Scruton, 1982, p. 35).

The 'point of view' assumed denotes indifferently any being who can use the term 'I', any being who can identify itself as the "subject of experience". This single, subjective, premise of self-consciousness is the starting point for all of Kant's philosophy (Scruton, 1982, p. 24). The 'objectivity' established, however, transcends the point of view of any individual "subject of experience" for it is the point of view of 'possible experience' and denotes indifferently the "existence of objects". What Kant is claiming about objects and selves alike is thus both universal and necessary, not specific and idiosyncratic. His concern might be said to be jointly with elucidating the "universally knowable" features of the real world and with the "universal knowing subject". Finally, Kant's Transcendental Deduction purports to be a demonstration that neither the concept of 'objectivity' nor that of 'self' can be properly understood or upheld in isolation from the other.

Kant distinguishes 'reason' from understanding and sensibility (cf. Losee's remarks and diagram, above). The need for this distinction is because Kant considers reason to have a natural and irrepresible tendency to transgress the limits of possible experience set by the categories. Where this happens, however, we risk mistaking an 'idea', created by 'pure', 'speculative', reason (and which is outlawed from Kant's epistemology), for a concept corresponding to a
real object. 'If only' Kant says 'we can guard against [this]
misunderstanding and so can discover the proper direction of these
powers'.

(Kant, CPR, p. 532).

Kant's proposal involves a rather different meaning of 'reason':

'[] Reason has [] as its sole object, the
understanding and its effective application. Just as
the understanding unifies the manifold in the object
by means of concepts, so reason unifies the manifold
of concepts by means of ideas, positing a certain
collective unity as the goal of the activities of the
understanding, which otherwise are concerned solely
with distributive unity.'

(Kant, CPR, p. 533).

For Kant, '[Reason] does not [] create concepts (of objects) but
only orders them' (CPR, p. 532: original emphasis); the
transcendental deduction of all ideas of speculative reason
consists 'not as constitutive principles for the extension of our
knowledge to more objects than experience can give, but as
regulative principles of the systematic unity of the manifold of
empirical knowledge in general,[]' (CPR, p. 550: original emphasis).

The presence of an underlying regulative principle, such as 'order'
or 'totality', provides the criterion with which we may discern
'systematic unity' in 'ordinary knowledge' and so raise it to the
rank of 'science' - a task which Kant calls 'architectonic'. Kant's
notion of architectonic, and even the terms which he creates to
discuss it with, bear a striking similarity to those of
'structuralist' epistemologists in more recent times (especially
those of Piaget, cf. section 3.4.1., below):
'By an architectonic I understand the art of constructing systems. As systematic unity is what first raises ordinary knowledge to the rank of science, that is, makes a system out of a mere aggregate of knowledge, architectonic is the doctrine of the scientific in our knowledge, and therefore necessarily forms part of the doctrine of method. [] By a system I understand the unity of the manifold modes of knowledge under one idea. []. The whole is [] an organised unity (articulatio), and not an aggregate (coacervatio). It may grow from within (per intussusceptionem), but not by external addition (per appositionem). []. The idea requires for its realisation a schema, that is, a constituent manifold and an order of its parts, both of which must be determined a priori from the principle defined by its end. The schema, which is not devised in accordance with an idea, that is, in terms of the ultimate aim of reason, but empirically in accordance with purposes that are contingently occasioned (the number of which cannot be foreseen) yields technical unity; whereas the schema which originates from an idea (in which reason propounds the ends a priori, and does not wait for them to be empirically given) serves as the basis of architectonic unity.'

(Kant, CPR, p. 653-654: original emphasis).

The last part of this quotation indicates that Kant has two meanings for the word 'idea* ('idea of pure reason'): the first is an ad interim creation of the mind and effects 'technical unity'; the second is synonymous with 'regulative principle' and effects 'architectonic unity'.

The identification of a regulative principle provides both the criterion by which we differentiate one scientific discipline from another and the guarantee that we can relate each to every other: all sciences can be unified by virtue of having a definite place within a single, comprehensive, classification:

'Every science is a system on its own right; [] we must [] set to work architectonically with it as a separate and independent building. We must treat it as a self-subsisting whole, and not as a wing or section of another building - although we may subsequently make a passage to or from one part to another.'

- 3.15 -
The 'regulative employment' of the ideas of pure reason allows for the systematic relation of all knowledge to the essential ends of human reason. Whilst there is a sense in which the architectonic is incorrigible, it should not be forgotten that the systematic unity of knowledge is of a qualitatively different sort to the knowledge of the understanding (i.e. the categories). This is precisely because reason transgresses the limits of possible experience set by the categories with the result that

'[] I may have sufficient ground to assume something, in a relative sense (suppositio relativa), and yet have no right to assume it absolutely (suppositio absoluta). This distinction has to be reckoned within the case of a merely regulative principle. We recognise the necessity of the principle, but have no knowledge of the source of its necessity; and in assuming that it has a supreme ground, we do so solely in order to think its universality more determinately.'

(Kant, CPR, p. 553-4; original emphasis within brackets, mine thereafter).

I contend that this last quotation shows that regulative principles cannot be used to justify (in an absolute or objective sense) any particular system of empirical judgements such as Euclidian geometry or Newtonian physics - notwithstanding the immense admiration and respect Kant had for both of these systems (he contributed to the development of the latter: the so-called 'Kant-Laplace Hypothesis'). This high regard would seem to have had less to do with their long continued and wide ranging empirical verification (invoked by empiricists as evidence for inductive reasoning) than with their deductive structure as disciplines (i.e. comprised of a set of laws incorporated within an axiomatised hierarchy) - a
quality very much in harmony with Kant's criteria of the architectonic (see Losee, 1980, p. 108).

Construing Transcendental Idealism in light of Lakatos' taxonomy, I believe that I have now shown that, for Kant, "to know is to construct"—hence his proposal is, indeed, for an 'activist' theory of knowledge. We must, however, live and die within the limits that the categories (themselves held to be God-given/Kant-discovered) impose upon construction, so Kant's epistemology is, as Lakatos claims, 'conservative' in its activism.

3.3.1. The Legacy of Kant.

There exists a consensus amongst epistemologists today that whilst Kant's theory of knowledge repays close study it has been shown to be sufficiently flawed to have been overthrown and superseded (to greater and lesser extents) by other versions of constructivist epistemology. I suggest that Popper expresses lucidly the prevailing critical appraisal of Kant's epistemological legacy:

'When Kant said, 'Our intellect does not draw its laws from nature but imposes its laws upon nature', he was right. But in thinking that these laws are necessarily true, or that we necessarily succeed in imposing them upon nature, he was wrong'. (Popper, 1972, p. 48).

I, too, have no quarrel with this appraisal as stated above.

Popper, however, then elaborates upon his second clause:

'Kant believed that Newton's dynamics was a priori valid. (See his Metaphysical Foundations of Natural Science, published between the first and the second editions of the Critique of Pure Reason.) But if, as he thought, we can explain the validity of Newton's
theory by the fact that our intellect imposes its laws upon nature, it follows, I think, that our intellect must succeed in this; which makes it hard to understand why a priori knowledge such as Newton's should be so hard to come by. [] Nature very often resists quite successfully, forcing us to discard our laws as refuted; but if we live we may try again.' (Popper, 1972, p. 48 and n. 15 - I have combined part of the footnote and text; original emphasis).

I contend that Popper construes Kant's intention to 'explain the validity of Newton's theory' in terms of a demonstration or proof of validity due to his (Popper's) later conclusion that 'it follows, I think, that our intellect must succeed in this'. My inference is further strengthened by Popper's claim, made elsewhere in the same text, that 'Kant [] gives an a priori deduction of Newton's theory' (1972, p. 94: original emphasis).

Now, Brittan (1978, Ch. 5), although not citing Popper, provides ample documentary evidence to show that something very like this view is also held by most philosophers, viz. that (a) Kant intended Newtonian physics to follow as a natural and valid consequence of Transcendental Idealism and, hence, that (b) the "overthrow" of Newton's theory by that of Einstein represents a considerable cause for critical doubt over Kant's epistemology (this second part is implicit in Popper's last sentence in the previous quotation).

Against this "consensual interpretation" of Kant's intention, however, Brittan argues that Kant himself never explicitly characterises his purpose in that way (1978, p. 119) and that it would be more plausible to infer that he was not interested in defending the validity of Newton's theory but rather its possibility, viz. '[] that Newtonian physics applies to the world (i.e., that the subject terms of its propositions denote really
possible objects)' (1972, p. 123, n.12). Such a defence would allow for more than a 'minimal' (i.e. phenomenalistic) interpretation of Newton's results, viz. a realistic interpretation, but this should not be mistaken for the intention of Kant's argument which Brittan summarises in the following way:

'[P]ut in its simplest terms, [the Transcendental Deduction] comes down to this: unless we assume that those a priori principles that make a world of independent objects possible have application, from which follows the possibility of a realist interpretation of physics, then we will not have an adequate concept of the self.' (Brittan, 1978, p. 126; my emphasis under 'assume', elsewhere as per original).

Some of my earlier interpretations of Kant's ideas may now be seen to be sympathetic to those of Brittan. By way of a further gloss upon them, I suggest that Newton's theory is best thought of as playing an illustrative, rather than a constitutive, rôle in Kant's argument. There is, I think, overwhelming evidence that Kant believed Newton's theory to be a priori valid as suggested by Popper and the "consensualists" - but not for the(ir) further inference that Kant demonstrated it to be so. Kant, no doubt, intended his epistemology to complement Newton's theory and so it does. This is especially clear from Kant's Metaphysical Foundations of Natural Science of 1783 in which we find one 'Category' after another transformed via an 'Analogy of Experience' into a 'Principle of [Newtonian] Mechanics', e.g. (respectively), 'substance': 'conservation of substance': 'conservation of matter' (see Losee, 1980, p. 179). But close though this complementary relation may be, it stops short of an a priori deduction of Newton's theory as Popper and the consensualists claim it to be. Both parts of the consensual interpretation reflect an empiricist emphasis within
constructivist traditions of epistemology that has significantly lost influence in recent years (due to the rise of 'Post-empiricist philosophy of science', see Ch. 4). From a 'post-empiricist' point of view (and also, I believe, from that of Brittan), however, the attempt of Popper and the consensualists to construe Newtonian physics as an empirical enquiry into the adequacy of Kant's epistemology doubly misses the point for, as Hughes puts it in a general discussion,

'It is important to emphasise that ontological and epistemological questions are not to be answered by empirical enquiry since they are concerned with, among other things, the nature and significance of empirical inquiry. They are questions requiring philosophical argument and debate in which the very presuppositions of knowledge, as a general issue, are of concern.'

(Hughes, 1980, p. 7: my emphasis).

Now, there has, indeed, been considerable philosophical argument and debate over the presuppositions of Transcendental Idealism and from which Kant has not escaped unscathed. Certain of these discussions point the way to perennial controversies of constructivism which, if one claims to be a constructivist at all, must be considered.

Cherry (1981), for example, argues that Kant's claims to 'objectivity' and 'objective knowledge' (in the senses discussed above) are undermined by his metaphysical doctrine of noumena by virtue of the fact that he elaborates it in such a way as to deny all intellectual or experiential access to them, thereby rendering them 'vacuous' concepts. Thus Kant (quoted by Cherry) argues that the noumenon is an 'entirely indeterminate concept of an intelligible entity [; it is] a thing so far as it is not an object of our sensible intuition []' (CPR, p. 268: original emphasis). For Kant,
The concept of a noumenon is a merely limiting concept, the function of which is to curb the pretensions of sensibility, and it is therefore only of negative employment.\(^\text{1}\)

(Kant, CPR, p. 272: original emphasis; quoted by Cherry, 1981, p. 182).

Against this, Cherry argues that the set of noumena comprising an intelligible world ('Reality') behind the empirical world ('Appearance') can aspire to intelligibility only if an 'access condition' (Cherry, 1981, p. 183: my emphasis) is satisfied, viz.

'I\textit{t must be possible in principle for some subject to experience in some mode or another the intelligible world and its population. Unless this condition is satisfied the expression 'intelligible world' makes no conceivable reference; for it can make no sense to posit entities such that any and every form of access to them is logically barred.' (Cherry, 1981, p. 183: original emphasis).

Cherry claims this access condition to be necessary for discussions of noumena to be 'intelligible speculations' (1981, p. 183). He goes on to ask a rhetorical question:

'Whether or not [an access condition] is ever forthcoming, what form would knowledge of the intelligible world take?' (Cherry, 1981, p. 183).

In response he proposes two answers or interpretations of Kant which he develops with closely supporting references to the Critique of Pure Reason and which he later demolishes even-handedly, thereby demonstrating the insufficiency of an access condition.

I find Cherry's arguments convincing but further details of them are beyond the scope of this thesis. For reasons that shall become clear later in this chapter, however, I cannot accept Cherry's final
conclusion that the example of Kant's failure to allow access to the one ultimate or real world renders all talk about such a world 'seductive nonsense'.

As a parting shot at this issue, I would like briefly to suggest a reason for Kant's long winded obscurantism concerning access to the real world. This shall serve to point to precisely the sort of issues which cannot be tackled adequately within the tenets of empiricist traditions of epistemology (including objectivist methodological constructivist traditions). I believe that the key to Kant's obfuscation over the issue of an access condition resides in his famous statement that

'I have [ ] denied knowledge in order to make room for faith'.
(Kant, CPR, p. 29: original emphasis).

This sentence has been held by eminent Kant scholars (e.g. Beck, 1978, p. 16) to be the foundation of Kant's ethical theory, later to be elaborated in his second Critique. What I am suggesting, however, is that Kant's prevarication over articulating an access condition - a general theory of reference - is highly connected with, if not identical to, his refusal to provide a proof of the a priori validity of Newton's physics. To have done either, I believe, would have seemed to Kant to be a form of idolatory: he revered, even worshipped, the objects of Newtonian mechanics but, in his personal religiosity, could not allow that he (or any one else) was actually perceiving them directly for this would have amounted to staring God in the face. I contend then, that for Kant in his time, Newton's theory effects preeminent 'technical unity' amongst extant conceptual systems, it illustrates unsurpassed and perhaps
unsurpassable 'architectonic unity' of reason, but it cannot be held to be 'objective knowledge' per se for this is entirely prescribed by the forms of intuition and the categories. Whilst the overthrow of Newton's theory cannot be held to be a refutation of Kant's epistemology, Kant's refusal or inability clearly to articulate a theory of reference does undermine his claim to provide a realist epistemology.

What, then, remains? I contend that there are two, and only two, features of Kant's epistemology which remain common to all constructivist theories of knowledge and which consequently may be used as defining characteristics:

(1). The 'Assumption of Epistemological Realism' (AER):

"Reality is mind independent".

All constructivist epistemologies purport to be realist epistemologies in a sense which invokes something like Kant's doctrine of noumena. That is to say, all constructivists hold that however real the worlds of our ideas may seem, there is in fact only one ultimate, really existing, reality behind them all. Implicit in AER in the notion that whether or not reality itself is changing it remains unchanged by whatever we say or think about it. Our ideas about reality may come, go, even come back again (this has often happened), but reality itself is held to exist entirely independently from all such activities. Thus what AER assumes is that reality is "mind-independent". Of course, Kant was neither the first nor the last to assume epistemological realism and the assumption remains a necessary feature of all realist
Whilst I propose AER to be only a necessary, not a sufficient, condition for a realist epistemology, I clearly intend something more than a mere 'assumption of realism' (AR) - for which AER might reasonably be mistaken as I have characterized it so far. That something more is the further assumption, implicit to AER but not to AR, that not only is there a mind-independent reality but also that it may be known. Hence AER. This, in turn, carries with it the promise or responsibility to satisfy an access condition, or 'theory of reference', for failure to do so undermines any claim to demonstrate a requirement for objects, i.e. to propose a realist philosophy, leaving 'no more than an assertion of dogmatic faith belied by all the facts.' (Trigg, 1980, p. 36). This I consider to be the ultimate fate of Kant's epistemology, albeit unintended by him (cf. my account of Cherry's argument, above). But when the realism of a putative realist epistemology is cast in doubt the charge of 'idealism' is usually made and, indeed, was made against Kant even in his own time. This was no doubt facilitated by the doubly misleading name of 'Transcendental Idealism' which Kant gave to his epistemology and which he soon regretted (Popper, 1972, p. 179). Kant tried to make the emphasis on objectivity more compelling by not only entirely rewriting the Transcendental Deduction for the 2nd edition of the Critique of Pure Reason but also including a further passage specifically entitled 'The Refutation of Idealism' (CPR. p. 244) - all to no avail. Kant's theory is now widely regarded as being dogmatic in exactly the manner of those metaphysical tractions which he strove to transcend, viz. he ultimately presupposed the very doctrines which
Idealist theories of knowledge are founded upon the counter-part assumption to AER, namely, the 'Assumption of Epistemological Idealism' (AEI), viz. "Reality is mind-dependent". The most illustrious proponent of idealist epistemology was the early 18th century philosopher Bishop Berkeley and whose dictum was 'To be is to be perceived.' He argued (most now accept without success) that his system was saved from the charge of anthropocentric subjectivism by his postulate of an all(ways) seeing God. Such is not the case with solipsist epistemology and which may be regarded as the most extreme form of idealism. Amongst its few advocates is Bradley who, in 1897, characterized the view as follows:

'I cannot transcend experience, and experience is my experience. From this it follows that nothing beyond myself exists: for what is experience is its (the self's) states.'


Nowadays, hardly anyone explicitly embraces idealism, still less solipsistic idealism. They are, nevertheless, worth mentioning since all forms of constructivism are susceptible to some degree to the charge of idealism, or at least to the suspicion of it, and this is particularly true of the version which I wish to endorse. Yet I believe that a requirement for objects, i.e. objectivity or realism in the Kantian sense, is a requirement well worth defending and I shall later do so, albeit, by considerably less elaborate means than those of Kant. I state my commitment to realism unequivocally to delimit my otherwise considerable sympathy for a loose alliance of approaches going under the rubric of 'sociology of knowledge' and
including hermeneutics, phenomenology and systems theory.

Hesse comments

'The sociology of knowledge is a notorious black spot for fatal accidents both sociological and philosophical.'
(Hesse, 1980, p. 30).

There is a prima facie case for the view that one of the 'philosophical' accidents afflicting at least some sociological traditions is idealism, viz. by restricting all talk about reality to "social reality", and to our "social construction" of it, some sociological traditions risk (amongst other things) becoming idealist-by-default in manner analogous to the Logical Positivists who attempted to remain "agnostic" with respect to ultimate reality.

Now, I have earlier suggested that the locus of epistemological realism resides in the critical fulfillment of an access condition, a theory of reference. Lest this imply sympathies to "foundationalist", 'objectivist' (in Brittan's second sense, cf. discussion, above), epistemology which I do not hold, let me say now that if, as I have argued, the fatal flaw in Kant's system was its crucial vagueness with respect to access to reality, then I consider (and shall later argue) that the theories of reference proposed by the majority of post-Kantian traditions of constructivism have been intenably restrictive, viz. correspondence theories of truth linked inseparably to a requirement for "convergence" with reality. I shall argue, to the contrary, that realism and 'relativism' (or 'subjectivism') are not inimicable positions within an epistemology as is commonly supposed,
and in particular that complementary 'realist-relativist' construals of science and of science pedagogy are both possible and desirable.

Finally, whilst I accept that it may be useful to talk of the social construction of knowledge within a variety of traditions (possibly including the one I endorse - see Ch.10), I hope to have shown that it was Kant's intention (if not his achievement) to provide a realist epistemology and I suggest that since Kant also originated constructivism this establishes a historical precedent which should continue to be honoured within any approach claiming itself to be constructivist. Thus all constructivists are realists, though not all realists are constructivists.

(2). The 'Constructivist Knowledge Thesis' (CKT): "All observations are theory-laden".

Kant, as we have seen, was responsible for introducing the notion that experience is partially constituted by theoretical categories. This much, following the demise of positivistic theories of knowledge, is now generally accepted amongst epistemologists. What has not fared so well is Kant's view that certain theoretical categories are valid a priori (or, more accurately, that certain a priori synthetic propositions are valid). Instead, theoretical categories are now generally accepted to be conjectured by the creative imagination and that the mere act of creating them may, for any intent that matters in epistemology, be considered to be independent of experience. This acceptance is itself, however, independent or prior to any discussion of the validity, utility, etc. of conjectured theoretical categories and does require
reference to experience. Thus, what remains from Kant's original synthesis of reason and experience that is common to all constructivists, besides AER, is a weakened (because more vague) version of his original thesis that all knowledge is construction. This is summarised by the now familiar slogan: "All observations are theory-laden".

3.4. Personal Construct Psychology is 'Revolutionary Activist'

(Kelly).

To assist my further construal of Lakatos' notion of 'revolutionary activism', as instanced by the epistemological aspect of PCP, I shall both augment and systematically summarize some of the appraisative dimensions of epistemological traditions alluded to in the previous section.

For this I have modified and extended a framework originally of three dichotomous dimensions proposed by Kant in the Critique of Pure Reason for construing the scope and function of reason, viz.

(1) The Object of Knowledge: Realism - Idealism
(2) The Origin of Knowledge: Reason - Experience
(3) The Method of Knowledge: Rationalism - Empiricism

To these I add a fourth:

(4) The Claim of Knowledge: Objectivism - Relativism

My choice of the terms comprising this dichotomy was, in turn, partially inspired by Bernstein, with whose preliminary explication of them my own also coincides:
'By "objectivism", I mean the basic conviction that there is or must be some permanent, ahistorical matrix or framework to which we can ultimately appeal in determining the nature of rationality, knowledge, truth, reality, goodness, or rightness. An objectivist claims that there is (or must be) such a matrix and that the primary task of the philosopher is to discover what it is and to support his or her claims to have discovered such a matrix with the strongest possible reasons. Objectivism is closely related to foundationalism and the search for an Archimedean point. The objectivist maintains that unless we can ground philosophy, knowledge, or language in a rigorous manner we cannot avoid radical skepticism.

The relativist not only denies the positive claims of the objectivist but goes further. In its strongest form, relativism is the basic conviction that when we turn to the examination of those concepts that philosophers have taken to be the most fundamental — whether it is the concept of rationality, truth, reality, right, the good, or norms — we are forced to recognize that in the final analysis all such concepts must be understood as relative to a specific conceptual scheme, theoretical framework, paradigm, form of life, society, or culture. Since the relativist believes that there is (or can be) a nonreducible plurality of such conceptual schemes, he or she challenges the claim that these concepts can have a determinate and univocal significance. For the relativist, there is no substantive overarching framework or single metalanguage by which we can rationally adjudicate or univocally evaluate competing claims of alternative paradigms. Thus, for example, when we turn to something as fundamental as the issue of criteria or standards of rationality, the relativist claims that we can never escape from the predicament of speaking of "our" and "their" standards of rationality — standards that may be "radically incommensurable". It is an illusion to think that there is something that might properly be labeled "the standards of rationality", standards that are genuinely universal and that are not subject to historical or temporal change.'

(Bernstein, 1983, p.8 : original emphasis).

In due course, I shall develop in detail my personal meanings for these terms and which depart from Bernstein's further elaborations of them.

For now, I wish only to make two points in preliminary explication of this framework. Firstly, in discussing Kant's three dimensions, Beck argues that:
These three ways of dividing possible philosophies are logically independent of one another, but in fact we find certain family affiliations among some of them.\(^1\)

(L. W. Beck, 1978, p. 4)

I contend that both parts of this view hold good with respect not only to my modified version of Kant's three dimensions but also to the framework as augmented by my fourth dimension. The four dimensions may be represented diagrammatically by means of three orthogonal axes:

![Diagram Showing Four Dichotomous Dimensions for Construing Theories of Knowledge.](image)

**Figure 3.2** Diagram Showing Four Dichotomous Dimensions for Construing Theories of Knowledge.
Secondly, I draw attention to what might be called the "subjectivist" elements alluded to in Bernstein's definition of relativism, above. That is to say, what I shall term the "Relativistic Knowledge Thesis" (RKT) that all conceptual frameworks express and contain, albeit, to greater and lesser extents, person (or persons') specific commitments, i.e. "values".

In pursuit of this second point, and to relate it back to Lakatos' distinction between conservative activism and revolutionary activism, I recall my summary of the prevailing reading of the fatal flaw to Kant's epistemology, namely, that it was dogmatic, viz. it ultimately presupposed just those doctrines which it sought to demonstrate as objective (section 3.3.1., above).

I contend that most modern constructivist traditions have responded to this criticism of Kant by seeking to develop (and certainly claiming to have developed) epistemologies which yield objective knowledge but which are "critically fallibilistic". I shall argue, however, that such traditions cannot sustain these claims. Specifically, and for reasons which shall become clear later, I cite 'structuralist' traditions (e.g. that due to Piaget) by virtue of their claim to universal necessity, and so-called 'critical rationalist' traditions (e.g. that due to Popper) by virtue of their commitment to objectivism. These traditions, each of them 'constructivist' in the minimal sense defined in section 3.3.1., above, might both be classified as 'objectivist' - 'objectivist-constructivist' - epistemologies since I shall later argue that universal necessity and objectivism amount to the same thing.
Briefly stated, my argument against such objectivist constructivist traditions is that they retain certain features of traditional rationalism which are incompatible with revolutionary activism: notwithstanding their claims to the contrary, they are conservative activist.

To elaborate slightly upon the rationalist elements retained, I shall refer to Flew's characterization of 17th and 18th century rationalism as propounded by Descartes, Spinoza and Leibniz:

'The characteristics of this kind of rationalism are: (a) the belief that it is possible to obtain by reason alone a knowledge of the nature of what exists; (b) the view that knowledge forms a single system, which (c) is deductive in character; (d) the belief that everything is explicable, that is, that everything can in principle be brought under the single system.'

(Flew, 1979, p.278)

I contend, and shall later demonstrate in the cases of Piaget and Popper, that objectivist constructivist traditions retain '(b)' and '(c)' but yet this contradicts the demand for reflexivity (self-reference) of theories of knowledge required by Lakatos' characterization of revolutionary activism, viz. 'it is we who create our 'prisons' and we can also, critically, demolish them'.

And, further, that whilst reflexivity of a theory of knowledge is a minimal condition for both responsibility and relevance to be achieved, where a theory of knowledge is successfully self-applied, i.e. it meets its own criteria for (growth of) knowledge, this cannot be held to constitute a demonstration of its objectivity as some have tried to claim for this is ultimately to beg the question via an infinite regress (ironically, I shall argue that this is what Lakatos himself does, see section 3.4.2., below).
No, the case which I wish to make, and to exemplify with PCP, is that Lakatos' articulation of revolutionary activism requires and embodies a commitment to a "comprehensive" form of rationality similar to that propounded by Feyerabend and which objectivist constructivists reject:

'Science, commonsense, and even the refined commonsense of critical rationalism use certain fixed categories ('subject'; 'object'; 'reality'), in addition to the many changing views they contain. They are therefore not fully rational. Full rationality can be obtained by extending criticism to the stable parts also. This presupposes the invention of alternative categories and their application to the whole rich material at our disposal. The categories, and all other stable elements of our Knowledge, must be set in motion.'

(Feyerabend, 1981b, p.74 : my emphasis)

In an earlier elaboration of similar views made with respect to the conduct of science, Feyerabend urges that

'A scientist who wishes to maximize the empirical content of the views he holds and who wants to understand them as clearly as he possibly can must therefore introduce other views; that is, he must adopt a pluralistic methodology. He must compare ideas with other ideas rather than with 'experience' and he must try to improve rather than discard the views that have failed in the competition.'

(Feyerabend, 1975, p.30 : original emphasis)

Such comparison of ideas, however, may be carried out at different "levels". Hence, in Swift, Watts and Pope (1983) we distinguish between, on the one hand '(M)ethodological pluralism' (and which I shall here argue also entails a commitment to a 'pluralistic (M)ethodology') to indicate critical comparison of rival meta-theories, i.e. epistemologies, and, on the other hand, '(m)ethodological pluralism' (and which I shall here argue also entails a commitment to a 'pluralistic (m)ethodology') to refer to such comparison of theories. Viewed epistemologically, the former affords "inter-systemic" criticism, the latter "intra-systemic" criticism.
Now, comparison at the epistemological "level" - (M)ethodological pluralism - is precisely what is denied by objectivists due to their retention of the traditional rationalist commitment to epistemological monism (objectivism). For objectivists, criticism of their own "objective" standards and criteria can only be carried out by application of their own standards and criteria - assuming that they take up the option to do so, which is rare. Criticism by the alternative, incompatible, standards and criteria would necessarily be seen as introducing an element of subjectivism into the debate. Yet this, I contend, is exactly what a revolutionary activist theory of knowledge must allow.

In the remainder of this sub-section (prior to its further subdivisions), then, I shall concentrate upon advancing an initial case that Kelly's original articulation of PCP is revolutionary activist in its epistemological aspect by demonstrating that (M)ethodological pluralism - a pluralistic (M)ethodology - is consistent with, indeed, consequential of, the formal content of the theory and that this was anticipated and endorsed by Kelly. (I shall defer my consideration of the less radical (m)ethodological pluralism until section 3.4.4.).

To reiterate, (M)ethodological-pluralism implies the rejection of "Methodological-monism" - the view that everything is explicable and can in principle be brought under a single system. Kelly himself greatly enjoyed raising the ambiguity of categorical systems which sought to place his viewpoint within one framework or another. Thus he stated:

- 3.34 -
'Personal Construct Theory has also been categorised by responsible scholars as an emotional theory, a learning a psycho-analytic theory (Freudian, Adlerian, and Jungian – all three), a typically American theory, a Marxist theory, a Humanistic theory, a logical positivistic theory, a Zen Buddhistic theory, a Thomistic theory, a Behaviouristic theory, an Apollonian theory, a pragmatisitic theory, a reflective theory, and no theory at all. It has also been classified as nonsense, which indeed, by its own admission it will likely some day turn out to be.'

(Kelly, 1970, p.10)

From reading this quotation it would be wrong to think, however, that Kelly was merely rejecting these interpretations of his theory as "mistakes". On the contrary he was celebrating them as a vindication of his view that persons can be industrious inventors of ideas and of links between them. This interpretation of Kelly's comment is not at all inconsistent with his stated desire to explore a new analogy for man, namely, 'Man-the-Scientist' and that this desire was born of a dissatisfaction with many of those in his list. For Kelly, Methodological-monism would be unduly restrictive, indeed, pre-emptive.

Kelly explains the initial appeal to him of exploring his metaphor "person-as-scientist":

'To a large degree – though not entirely – the blueprint of human progress has been given the label of "science". Let us then, instead of occupying ourselves with man-the-biological organism or man-the-lucky guy, have a look at man-the-scientist.'

(Kelly, 1955, p.4 : original emphasis)

Now, as I shall later show, there is a good deal more of Kelly's personal thought behind this somewhat jocular summary of his reasons for choosing this metaphor than this quotation alone might suggest. For my immediate purposes, however, it is enough to point out that this remark, made early in his book, makes it clear that
Kelly regards his metaphor as one of many creative images of the person, in his view a potentially fruitful one to explore but which, at the time he was writing, had tended to be ignored by psychologists. He goes on, however, to add the first of many cautions and caveats:

'When we speak of man-the-scientist we are speaking of all mankind and not merely a particular class of men who have publicly attained the stature of "scientists". We are speaking of all mankind in its biological aspects or all mankind in its appetitive aspects.'

(Kelly, 1955, p.4 : original emphasis)

Kelly's metaphor is clearly intended to explore only an aspect of Man's personality. In a later comment he alerts us to his suspicion of 'complete' explanations of man, i.e. his wariness of Methodological-monism:

'No one has yet proved himself wise enough to propound a universal system of constructs. We can safely assume that it will be a long time before a satisfactorily unified system will be proposed. For the time being we shall have to content ourselves with a series of miniature systems, each with its own realm or limited range of convenience.'

(Kelly, 1955, p.10 : my emphasis)

and the temptation to over extend a locally useful system, such as his own:

'It is also important that we continue to recognize the limited ranges of convenience of our miniature systems. It is always tempting, once a miniature system has proved itself useful within a limited range of convenience, to try to extend its range of convenience.'

(Kelly, 1955, p.11)
Finally, Kelly identifies the range of convenience within which his exploration of the metaphor has provided a personal demonstration of utility, i.e. that of human personality and the problems of inter-personal relationships - a reflection of his concerns in his employment as a clinical psychologist - and then adds yet another caveat:

'Not only do systems, psychological and otherwise, tend to have limited ranges of convenience, but they also have foci of convenience. There are points within its realm of events where a system of a theory tends to work best. Usually these are the points which the author had in mind when he devised the system. For example, our own theory, we believe, tends to have its focus of convenience in the area of human readjustment to stress.'

(Kelly, 1955, p.11-12 : my emphasis)

It is this caution which has prompted me, as an educational researcher, to attempt to modify and extend his theory to render it (more) 'convenient' for use in formal educational settings.

Thus far, I hope only to have demonstrated Kelly's modesty in his proposals for PCP and his tolerance of (M)ethodological pluralism. Elsewhere, however, he makes the stronger claim that 'our theory is frankly designed to contribute effectively to its own eventual over-throw and displacement' (Kelly, 1969, p.66 : my emphasis).

This, I suggest, alludes to Kelly's commitment to (M)ethodological pluralism in its investigative aspect i.e. a pluralistic (M)ethodology. Demonstrating that this commitment is more than mere rhetoric, however, requires a selective consideration of the 'assumptive structure' (Kelly, 1955, p.58), or formal content, of PCP. This shall also serve to render meaningful later contrasts and comparisons that I shall make between PCP and alternative theories.
In a preliminary statement of his commitment to constructivism, Kelly proposes that

'Man looks at his world through transparent patterns or templates which he creates and then attempts to fit over the realities of which the world is composed. ( ). Let us give the name constructs to these patterns that are tentatively tried on for size. They are ways of construing the world.'

(Kelly, 1955, p.8-9 : original emphasis)

Now, Kelly gave a rather specific meaning to the term 'construct', viz.

'In its minimum context a construct is a way in which at least two elements are similar and contrast with a third.'

(Kelly, 1955, p.61)

Subsumed by this definition is Kelly's meaning of the term 'element':

'The things or events which are abstracted by a person's use of construct are called elements. In some systems they are called objects.'

(Kelly, 1955, p.562)

The construct represents Kelly's most important technical innovation and its influence is felt throughout his theory. The construct both initiates a qualitatively different and preferable mode of enquiry from that implied by the more familiar (though, I contend, poorly understood) "classical" notion of the 'concept' and, complementing this, adumbrates a radical new meaning for the notion of 'explanation'. I shall consider in detail these particular potentialities which the construct has to offer later in the thesis (section 3.4.2.; and Chapters 4 and 10, respectively). Meanwhile I shall discuss further the characteristic features and qualities of the construct in vitro as it were.
The quality of contrast, intrinsic to Kelly's meaning of the construct, has been discussed by Fransella and Bannister:

'When we say that Bill Bloggs is honest, we are not saying that Bill Bloggs is honest, he is not a chrysanthemum or a battle-ship or the square root of minus one. We are saying that Bill Bloggs is honest, he is not a crook.'

(Fransella and Bannister, 1977, p.5)

In the example, above, it should be clear that the contrast ('crook') is not any contrast, it is a relevant contrast and the relevancy is achieved through the dimensionality of the construct ('honest' vs 'crook').

Every construct has a relevant contrast as a proper part and Kelly terms the two ends of the dimension 'poles' (1955, p.137). The focus of the contrast, i.e. the contrast pole, is always, and in an important sense, the opposite of the other pole: it is relevant because it is opposite and, if construed as opposite, then it is relevant.

Now, exactly what this quality of "oppositeness", of relevancy in contrast, is cannot be universally formalised or specifically determined in advance for its meaning will depend upon, and vary according to, the particular purpose for which each construct has been created. (This is not to say, however, that Kelly's notion of opposite contrast cannot be developed beyond his original proposal. Cf. Chapter 4).
Constructs, then, are 'bi-polar' dimensions but the quality of oppositeness in each pole relative to the other renders the dimension they prescribe discontinuous or 'dichotomous'. As Kelly also, and less formally, puts it 'a construct is at heart a black and white affair, rather than a scale of greys' (Kelly, 1969, p.10: my emphasis). For Kelly, dichotomous dimensionality represents 'an essential feature of thinking itself' (Kelly, 1955, p.62) and affords 'universal utility' (Kelly, 1969, p.10) in the life of the person.

It is important now to stress, as Kelly does, that it is our constructs that are dichotomous, not the objects (elements) for which they provide an ad interim 'reference axis' (Kelly, 1969, p.10). This is because our constructs are, and can only be, our ideas about the world, of which the meta-theoretical notion of the construct is itself also one. With this accepted, then "greys" between the "absolute", "black and white", poles of a construct may be admitted:

'[A construct] may not be accurate, and it may not be stable from time to time, but as a construct, it has to be absolute. Still, by its successive application to events one may create a scale with a great number of points differentiated along its length. Now a person who likes greys can have them - as many as he likes.'

(Kelly, 1966, p.14: my emphasis)

Whilst it is true that any construct implies a kind of "conceptual closure" and is thus "absolute" or "deterministic" in some sense, we are always free to re-construe. Kelly claimed that events are subject to 'as great a variety of constructions as our wits would enable us to contrive' (Kelly, 1970, p.1), indeed, he urged that
'we must continually and adventurously hold all matters open to the possibility of fresh reconstruction.' (Kelly, 1966, p.5: my emphasis). In keeping with these sentiments, Kelly named the epistemological stance intrinsic to PCP as 'Constructive Alternativism' (Kelly, 1955, p.3: my emphasis). Now, in Kelly's view, constructs do not exist in isolation:

'Each person characteristically evolves, for his convenience in anticipating events, a construction system embracing ordinal relationships between constructs.'

(Kelly, 1955, p.56: my emphasis)

This statement constitutes one of 11 corollaries, namely, the 'Organization Corollary', which serve to articulate parameters and dynamics of the "constructive structure" of construction systems in augmentation of Kelly's 'Fundamental Postulate' of PCP, viz.

'A person's processes are psychologically channelized by the way in which he anticipates events.'

(Kelly, 1955, p.46)

The Organization Corollary implies that constructs are linked with each other in a 'more or less coherent and hierarchical manner' (Pope and Keen, 1981, p.36). (I have already discussed unannounced aspects of several of Kelly's corollaries, however, for a summary of the formal content of PCP, see Appendix 3).

Now, Kelly was both aware and approved of his theory being characterized as a 'meta-theory':

'Some have suggested that personal construct theory not be called a psychological theory at all, but a meta-theory. That is all right by me. It suggests that it is a theory about theories, and that is pretty much what I have in mind.'

(Kelly, 1966, p.9: my emphasis)
It may be useful to explore this view by considering the construct in light of Brodbeck's (1968) distinction between the 'content' and the 'form' of a theory:

- the 'content' (or in Kelly's idiom, 'range of convenience') of a theory comprises its 'descriptive terms' (or in Kelly's idiom, 'elements'), i.e. the names and characteristics of individual things abstracted by the theory, e.g. 'cool', 'spanner', 'manager'.

- The 'form' of a theory comprises its 'logical terms', i.e. the terms which specify connections and relations between the descriptive terms of the content, e.g. 'and', 'implies', 'or'.

When construed by means of this distinction, Kelly's formal definition of the construct may be seen as a radical proposal for the general form of theories (concepts, ideas etc.) since he held persons' construction systems to be 'composed entirely of constructs' (Kelly, 1955, p.61). As such it applies to Kelly's theory itself and if one is to remain true to the reflexivity implied by his exhortation to 'continually and adventurously hold all matters open to the possibility of fresh reconstruction' (quoted above), then one must attempt, at least periodically, to construe PCP itself in terms of a personal construct. The construct thus outlines a transcendental dialectic which is applicable to meta-theoretical elements, it constitutes a general meta-theory embodying a pluralistic (M)ethodology.

At this point I should like to consider Kelly's views on the 'utility', or worth, of constructs and construction systems since this also qualifies claims and recommendations I shall make in the remainder of this thesis.
Some comments by Bannister are pertinent to this subject:

'Construct theory accepts the notion of difference, but not the notion of superiority. Every construct system (which is to say every person) is valid in its own right. To argue that one construct system is superior to another is to accept the terms, purposes and criteria of one system as valid and to deny the validity and point of the "inferior" system.'

(Bannister, 1979, p.31)

This should not be understood as an endorsement of capricious solipsism (and nor does Bannister intend it as such), however, for Kelly does not dispute that

'Some of the alternative ways of construing are better than others.'

(Kelly, 1955, p.45 : my emphasis)

Rather, what is at issue in such comparisons is the relative personal utility of the constructs under consideration:

'Constructive Alternativism holds that man understands himself, his surroundings and his potentialities by devising constructions to place upon them and then testing the tentative utility [my emphasis] of these constructions against such ad interim criteria as the successful prediction and control of events.'

(Kelly, 1966, p.1 : original emphasis).

The caveats 'such' and 'ad interim' stem from Kelly's view that

'Men not only construe their alternatives, but they construe also criteria for choosing between them'

(Kelly, 1969, p.85)

- a view of vital pertinence to the subject of this thesis.
The personal utility of a construct may be understood to mean the degree of satisfaction of personally pre-established criteria by experience as mediated by that construct. Constructs are always created, used and appraised according to personal purposes and criteria for them with the consequence that constructs 'have no existence independent of the particular person whose thinking it characterizes' (Kelly, 1969, p.87).

There may thus be no "absolute" judgements of the utility of a construct or construction system made either within or between persons.

I return, at last, to Kelly's provocative statement that his theory is 'designed to contribute effectively to its own eventual over-throw and displacement' (quoted above). I hope by now to have demonstrated that Kelly's formal theory does, indeed, possess the technical ability to achieve this. I would, however, also like to argue for the urgent need that this "pluralistic (M)ethodological imperative" be fulfilled by appealing to a cultural aspect of the time we live in. This shall augment my already stated intention to extend the original range of convenience of Kelly's theory to encompass pedagogy.

Kelly argued that

'All thinking is based, in past, on prior convictions. A complete philosophical or scientific system attempts to make all these prior convictions explicit.'

(Kelly, 1955, p.6)
Consistent with his own advice, Kelly states that

'This theory of personality actually started with the combination of two simple notions: first, that man might be better understood if he were viewed in the perspective of the centuries rather than in the flicker of passing moments; and second, that each man contemplates in his own personal way the stream of events upon which he finds himself so swiftly borne. Perhaps within this interplay of the durable and the ephemeral we may discover ever more hopeful ways in which the individual man can restructure his life. The idea seems worth pursuing.'

(Kelly, 1955, p.3 : my emphasis).

Now, the formal content of Kelly's theory might be described as an elaboration of his "root" metaphor, namely, 'man-the-scientist'. Whilst I have already shown by means of a Kelly quotation that the consistently high, even pre-eminent status of science as a 'blueprint of human progress' afforded by Western societies constituted one aspect of the appeal to Kelly of his metaphor, his choice may be further understood by construing it to be based on his judgement that persons engaged in the conduct of science consistently achieve the most fruitful, dynamic, compromise between the 'durable' and the 'ephemeral'. Here we may also suppose that Kelly was influenced by his experience of his initial degree which was in physics and mathematics.

Now, "having a look" at the person-as-scientist requires developing an idea about what it is to be a scientist. This Kelly surely did by means of his Fundamental Postulate and its 11 corollaries - a remarkably complete and self-consistent theory, especially when compared with those of most other psychologists. Kelly, however, urged that

'One does not escape from his cultural controls (assuming that there is any reason to escape) simply by ignoring them - he must construe his way out'.

(Kelly, 1955, p.182 : original emphasis)
The point I am making is that Personal Construct Psychology became, in a sense, a 'cultural control' the instant Kelly publicly announced it. But having created PCP, Kelly himself did not fully respond to his own advocacy of pluralistic-(M)ethodology, indeed, he was not above a little pre-emptive construction of his own when it came to appraising the merits of rival psychological theories (Holland, 1970). We should not, however, let this overshadow the overall "spirit" of Kelly's proposal which was rigorously underscored by its formal content. Kelly, after all, was human and may be supposed to have had his hands full defending and promoting his own theory! Without offering more than a mild apology for Kelly, then, I suggest that there are two further reasons as to why Kelly did not fully implement a pluralistic-(M)ethodology and why it is increasingly urgent that this be done.

My first reason is that at the time Kelly was writing his seminal work, in the two decades prior to 1955, a unified view of science was widely held. As Vander Goot puts it

"Although nearly everyone acknowledged disciplinary differences, many scientists believed that their efforts were part of a unified enterprise held together by the necessities, limits, and possibilities of common reason which in turn were reflected in 'The Scientific Method'."

(Vander Goot, 1981, p.3 : my emphasis)

She goes on to explain how this view changed:

"It is interesting that one of the fundamental assumptions underlying the view from which Kelly drew his inspiration in the study of human personality is now, two decades later, widely questioned. Within the ranks of respected scientists there are many who claim that it is inaccurate to speak of "science" and prefer instead to refer only to various schools of scientific thought."

(Vander Goot, 1981, p.3 : my emphasis)
Exploring the metaphor "person-as-scientist" was thus less
problematic in this sense for Kelly in the mid-1950s than it is for
us today.

My second reason, highly related to the first, concerns developments
in the philosophy of science. In the 1950s, the shortcomings of the
traditional orthodoxy of Baconian empirical-inductivism were being
voiced by philosophers of science and scientists alike.

Now, as Fransella has pointed out

'(Kelly is unusually reticent on the subject of the nature of
science. He says such things as "It is customary to say that
the scientist's ultimate aim is to predict and control" (1955,
p.3) but little else. Later he talks particularly of the two
divisions he sees within science, accumulative fragmentalism
and constructive alternativism (Kelly, 1969)'

(Fransella, 1981, p.1: my emphasis)

- notwithstanding his detailed elaboration of "person-as-
scientist".

The nub of Kelly's principal characterization of 'accumulative
fragmentalism' may be construed to be synonymous with Baconian
empirical-inductivism:

'The [accumulative fragmentalist] view [is] that science makes
its progress step by step. This is usually taken to mean that
we discover nature a fragment at a time, that as each fragment
is verified it is fitted into place - much like a piece in a
jigsaw puzzle. Some day we'll get it all put together. [].
The man who has verified his hypothesis has, he supposes, a
little chunk of 24-carat truth to add to his inventory.
[Such a] man thinks he has captured an essence; []. Now what
happens? []. To the accumulative fragmentalist the next step
is to find another nugget of truth []. [F]or the accumulative
fragmentalist the only grounds for entertaining further
questions about the matter is evidence that he was wrong.
Since this kind of nuisance may pop up at any time he is
careful to replicate his experiments and make sure the answer
to his question is absolutely, positively, and irrevocably
right!'

(Kelly, 1969, p.125-6)
Now, the Assumption of Epistemological Realism (AER), intrinsic to both Baconian empirical inductivism and Kelly's notion if accumulative fragmentalism ('we discover nature', 'a little chunk of 24-carat truth') conflicts with the Assumption of Epistemological Idealism which is at the core of the doctrines of Logical Positivism and which, at the time Kelly was formulating his theory, had come to replace Bacon's epistemology as the new orthodoxy in philosophy of science. The Logical Positivists claimed corroboration for their epistemological idealism from the then new Quantum Theory and from epistemological views expressed by its leading exponents.\textsuperscript{11}

PCP is almost as diametrically opposed to Logical Positivism as it is to Baconian empirical inductivism (accumulative fragmentalism) - with respect to the former, not least because of Kelly's commitment quo constructivist epistemologist to AER. In 1955, however, Popper's constructivist critique of, and alternative to, Logical Positivism had still to make its monumental impact\textsuperscript{12}. Since Popper, however, there has been a gradually accelerating proliferation in rival constructivist traditions so that there is now no semblance of orthodoxy in philosophy of science, as I mentioned in Chapter 2.

Personal constructivists such as myself, who are anxious to develop and extend Kelly's theory and its range of convenience may, therefore, fruitfully tackle an even larger and more urgent question "Man-the-Scientist - but which?" by referring to contemporary philosophy of science. Explorations have already been made, for example, Kelly's personal scientist has been construed as a Kuhnian (e.g. Vander-Goot, 1981; Candy, 1982) and as a Lakatosian (e.g. Watts and Pope, 1982) and as Popperian, Kuhnian, Lakatosian and Feyerabendian respectively in Swift \textit{et al}, 1982.
In light of my preceding views I shall now attempt critically to further explore and to develop the epistemological aspect of PCP by employing a pluralist (M)ethodology as embodied by the construct applied to meta-theoretical elements, viz. having argued that the constructivist tradition due to Kelly is revolutionary-activist, in the remaining two sub-sections of this chapter, I shall argue that those due to Piaget and Popper both differ from Kelly's by virtue of their conservative-activism.

3.4.1. Relevant Contrast (1) : Structuralist Theories of Learning are 'Conservative-Activist' (e.g. Piaget)

In my discussion of Piaget's theory, which soon follows, I have made extensive use of two references, viz.


I justify my choice of both this tactic and these texts as follows:

Reasons common to my choice of these books include the fact that they were both compiled/written near the end of Piaget's life so I believe that they can be taken to provide an insight into his theory at, or at least near, its latest point of development. Also, each book has been widely read.
With respect to Piatelli-Palmarini's book, my choice was further influenced by the fact that it contains transcripts of Piaget (and Piagetians) actively arguing for his theory and defending it against alternatives: the book has been one fruitful source for arguments that I use in this paper. Where I quote Piaget (or another person) from this text, I shall indicate who, and reference the overall text 'P-P', e.g. (Piaget, in P-P, p.150).

Gruber and Voneche's book was highly thought of by Piaget himself: in his forward to it, he explicitly approved of both the selection of his works reprinted/excerpted within it, and the interpretative commentary supplied by the editors. I also hope that by using this book I shall make it easier for a reader to "check up" on, or to explore, the context from which I have quoted. Where I quote Piaget from this text I shall supply the date of the relevant work and reference the overall text as 'G.V.', e.g. (Piaget, 1952; in G.V., p.446).

To assist my later critique of Piaget's theory, which he termed 'Genetic Epistemology', I shall first articulate the relationship between Kant's theory and that of Piaget, as perceived by Piaget himself.

Thus, Piaget frequently, and explicitly, proclaims the influence of Kant's ideas on those of his own, e.g.

'I consider myself to be profoundly Kantian.'

(Piaget, in P-P, p.150)

The principal affinity that Piaget sees between himself and Kant is that both employ concepts - 'structures' and 'Categories', respectively -
which purport to universality and necessity. Piaget sometimes presents a concise approbation of these qualities of the other's theory, e.g.

'Kant ["s theory] comprises, as it should, universality and necessity ...'

(Piaget, 1971a, p.57)

Piaget alludes to universality and necessity in a characterisation of structuralist approaches:

'[There are] at least two aspects that are common to all varieties of structuralism: first, an ideal (perhaps a hope) of intrinsic intelligibility supported by the postulate that structures are self-sufficient and that, to grasp them, we do not need to make reference to all sorts of extraneous elements; second, certain insights — to the extent that one has succeeded in actually making out certain structures, their theoretical employment has shown that structures in general have, despite their diversity, certain common and perhaps necessary properties'.

(Piaget, 1971b, pp.4-6)

These qualities are, again, implicit when he goes on to give a sketch of his personal meaning of a structure:

'As a first approximation, we may say that a structure is a system of transformations. Inasmuch as it is a system and not a mere collection of elements and their properties, these transformations involve laws: the structure is preserved or enriched by the interplay of its transformation laws, which never yield results external to the system nor employ elements that are external to it. In short, the notion of structure is comprised of three key ideas: the idea of wholeness, the idea of transformation, and the idea of self-regulation.'

(Piaget, 1971b; p.5).

But they are explicit when, for example, he claims that

'The notion of law presents in the child, as indeed in the whole history of thought up to modern times, two complementary features — universality and necessity. Law is a constant and necessary relation'.

(Piaget 1927; in G.V., p.146 : my emphasis).
From the foregoing I hope to have demonstrated that Kant and Piaget share at least the intention of building universality and necessity into their respective theories. I also hope that these quotations provide an insight into Piaget's personal meaning of 'structure' and some of his anticipations and purposes for it.

It would be misleading to proceed further, however, without pointing out that where Piaget makes remarks about Kant, such as those I quoted earlier, he invariably does so as a prelude to elaborating upon the profound differences he perceives exist between their respective ideas.

Piaget's principal complaint is that Kant provides the neonate with too much:

'[ ], the construction characteristic of the epistemological subject, however rich it is in the Kantian perspective, is still too poor, since it is given completely at the start.'

(Piaget, 1971a; p.57)

Piaget considers himself to be

'[ ] of a Kantianism that is not static, that is, the categories are not there at the outset: it is rather a Kantianism that is dynamic, that is, with each category raising new possibilities, which is completely different.'

(Piaget, in P-P; p.150.)

Piaget himself denies that there are any innate cognitive structures:

'Nor do any a priori or innate cognitive structures exist in man; ....'

(Piaget, in P-P; p.23.)
'[ ] the functioning of intelligence alone is hereditary ...'

(Piaget, in P-P; p.23: my emphasis.)

By this Piaget means that a person inherits an ability to adapt to their environment: 'Intelligence is an adaptation' (Piaget, 1966a; p.3), indeed, '... intelligence .. is an extension and perfection of all adaptive processes' (Piaget, 1967; p.9). The functioning of intelligence consists of the process of 'equilibration' (or 'self-regulation'). Equilibration is comprised of the two complementary processes of 'assimilation' and 'accommodation':

'Intelligence is assimilation to the extent that it incorporates all the given data of experience within its framework. [ ]. There can be no doubt either that mental life is also accommodation to the environment. Assimilation can never be pure because by incorporating new elements into its earlier schemata the intelligence constantly modifies the latter in order to adjust them to new elements.'

(Piaget, 1966a, p.6-7 : my emphasis)

During an interaction with the environment, temporary dynamic balances, or equilibrium states, are found as a result of the operation of assimilation and accommodation. These equilibria are characterized by varying degrees of application to objects or events, their mobility or extent of transferability across data types, and stability or inclination to cope with presented demands without change.

Equilibration, then, is the mainstay of Piaget's general theory of cognitive function. This may be distinguished from his theory of the development of intelligence which is linked to his notion of stages.
The distinction I have just identified, however, is not a separation. This is because Piaget claims that equilibration not only provides the means by which we move through his postulated sequence of stages, but that it also

'[ ] creates structures through an organisation of successive actions performed on objects.'

(Piaget, in P-P; p.23 : my emphasis).

Thus, in Piaget's view, although the individual is not born with structures, they are born with the ability to create them (or rather, re-create them, as we shall see). The process of creating structures is begun by means of 'reflexes', a term which, on Piaget's idiosyncratic meaning, refers to the hereditary ability to perform certain unsophisticated motor-actions. Among the most important of these are sucking, eye-movements and grasping. Piagetian reflexes may be thought of as being only precursor 'operations' (N.B. For Piaget's definition of operation, discussion later).

Piaget argues that

'There is no structure apart from construction, either abstract or genetic.'

(Piaget, 1971b; p.140 : original emphasis).

The need for Piaget to defend this position - to argue that his postulated structures are created (constructed) - is precisely because his brand of structuralism would otherwise

'[ ] lapse into a theory of Husserlian essences, Platonic forms or Kantian apriori forms of synthesis.'

(Piaget, 1971b; p.9 : my emphasis).
In light of all the ideas so far expressed, it would seem that Piaget is committed to universality and necessity on the one hand, and to 'activism' (i.e. autonomous construction) on the other. He apparently perceives himself to be some sort of "dynamic Kantian", and thus it is quite clear to me that Piaget (were he alive) and Piagetians would claim their theory to be revolutionary-activist. But could they justify such a claim? I shall argue that they could not: their theory is not "dynamic" enough, it is conservative-activist!

Before I attempt this, however, I consider it necessary to provide relevant context by elaborating upon two further aspects of Piaget's meta-theory, viz., his notion of 'epistemic subject' and the relationship between his structuralist metaphysics and his developmental theory of stages. I shall consider them in order.

Piaget made a distinction between the "whole person", or 'psychological subject', and that aspect of the whole person — common to all people — which he considered to be the subject (or perhaps more accurately, object) of his attention, the 'epistemic subject'. Piaget explains the distinction:

'There is the 'psychological subject' centred in the conscious ego whose functional role is incontestable but which is not the origin of any structure of general knowledge but there is also the 'epistemic subject' or that which is common to all subjects at the same level of development, whose cognitive structures derive from the most general mechanisms for the co-ordination of actions.'

(Piaget, 1966b, p.308)

The psychological subject is the whole person and it therefore cannot be studied without reference to the activities of the particular, individuated and embodied person. The epistemic
subject, by contrast, represents an 'analytical abstraction' (Duveen, 1983; p.231) to enable study of human cognitive process purged of affect and individuation. As Mischel puts it:

'Talk about the "activities of the epistemic subject" does not refer to real activities performed by a subject: it is a way of talking about timeless, logico-mathematical relationships.'

(Mischel, 1979, p.101)

And Duveen characterizes Genetic-Epistemology as being a theory which is 'concerned with the development of forms of knowledge common to all individuals' (1983, p.231). He later identifies an important qualification to the theory:

'[the epistemic subject] does not deny the reality of other, non-cognitive processes. Piaget himself frequently stressed that every psychological action was both a cognitive and an affective action. In the individual, psychological subject, both aspects appear fused together. In seeking to deal with the former genetic epistemology has necessarily recognised its lack of comprehension of the latter. From this perspective indeed a recognition of the reality of affective processes is almost as far as its comprehension of them extends'.

(Duveen, 1983; p.238: original emphasis).

Piaget's notion of epistemic subject, although itself remaining a metaphysical or purely formal entity, identifies the "universe of discourse" of Piaget's structuralist metaphysics, viz., the problem of knowledge. Piaget's structuralism can be considered to be a metaphysical articulation of his commitment to universality and necessity, it underlies and informs development of all areas of his theory.

Piaget's theory of developmental stages is the most famous part of his theory. Stage-theory represents a formalisation of his
structuralist metaphysics and it is principally stage-theory that has import for activities in the real world: it purports to be theoretical.

Stage-theory has also been the most heavily criticised part of Genetic Epistemology. Criticisms of stage-theory have tended to be mainly empirical - I suspect for no better reason than that Piaget's empirical claims are somewhat less esoteric than their counterpart in his formalisms. Powerful and important though many of the empirical criticisms are I shall largely ignore them in this thesis. Instead I shall concentrate on advancing formal criticisms of stage-theory which complement certain of the empirical ones and which shall demonstrate the poverty of Piaget's approach, viz, his conservative-activism.

In its minimal context, a Piagetian stage could be defined as a systematised integration of 'operations'. As Piaget elaborates:

'Psychologically, operations are actions which are internalizable, reversible, and co-ordinated into systems characterized by laws which apply to the system as a whole... since operations do not exist in isolation they are connected in the form of structured wholes'.

(Piaget, 1952, in G.V., p.456)

A 'structured whole' ('structure d'ensemble'), however, is not to be understood as a mere aggregation of operations. This is because the term refers to a system of elements defined by a general set of laws: these laws define the system as a whole. Thus, a structure d'ensemble is to be distinguished from the individual operations themselves (translator's note, Piaget, 1952, in G.V.; p.456). For Piaget, a structure is a system of operations. Operations refer to the psychological comprehension of transformations underlying phenomena and, thereby, the recognition of invariances.
A Piagetian stage can be thought of as a level in cognitive development in two senses:

'[ ] A Stage consists of a level of preparation on the one hand, and a level of completion on the other.'

(Piaget, 1955, in G.V.; p.816 : original emphasis).

The 'level of preparation' consists of 'processes of formation or the genesis' (ibid.), whilst the 'level of completion' consists of 'forms of final equilibrium (in a relative sense)' (ibid.)

Piaget's first meaning of 'level', above, introduces an aspect of stage-theory that Piaget progressively emphasised during his academic life, viz.,

If we are to speak of stages, the order of succession of acquisitions must be constant. Not the timing but the order of succession.

(Piaget, 1955, in G.V. p.815 : original emphasis).

Piaget's use of the auxiliary verb "must", in the last quotation, signals that his idea of an invariant order of succession is much more than an empirical generalisation: it is necessarily invariant. For Piaget, operationality emerges in an invariant sequence as a matter of 'logical necessity' (Piaget, 1971c; p.316 : my emphasis). (Piaget's "neutralisation", of the term 'necessity', effected by his placing it within inverted commas wherever he uses the expression 'logical necessity', shall later turn out to be important.)

Now Piaget's logic, what he calls 'psycho-logic', differs from "standard" forms of propositional logic and for this he has been heavily criticised as I shall later discuss. Furthermore, the differences and difficulties are to some extent masked by his
selective use of standard notation. With this in mind we can
nevertheless say that Piaget sees the logical relationship obtaining
between a higher and a lower stage or structure to be that of

inclusion. Logically speaking, the sequence of stages is one of
"successive inclusion", or, as Piaget put it

' .. each structure becomes a subset of a richer structure ..
(Piaget, in P-P, p.150)

The attainments of an earlier stage are retained (included) within a
later stage. Attainment of an earlier stage is a pre-condition
(necessary, but not sufficient) for the construction of a later
stage - whence the idea that any stage may be partially construed as
a level of preparation for the next.

Now, Piaget (e.g. 1965, in G-V; 1971b) views intellectual
development as analogous to the biological notion of 'epigenesis':
equilibration is mediated by a sequential unfolding of "organs" of
regulation. Epigenesis has been construed to have four essential
features (Kitchener, 1978). First, the process of psychological
development involves a causal sequence of events, with successive
steps being dependent on those preceding. Second, the sequence
involves increased organization, differentiation and complexity,
these being a transformation from homogeneous to heterogeneous
status and from general to specific functions. Third, in the
process towards complexity, something new emerges at each step:
qualitatively different structures appear. Fourth, there is a
stepwise growth through a series of stages, each stage being marked
by qualitatively different emergent structures. In short, Piaget
sees intellectual development as proceeding through a series of
stages, each qualitatively different, in an invariant order.

Piaget's psycho-logic and his theory of stages serve to formalise
and to operationalize this analogy (cf. Gilbert and Swift, 1985, included in this thesis as Appendix 1).

In my critique of Piaget's theory, which soon follows, I shall refer to those researchers who claim some kind of allegiance to Piaget's ideas as "Piagetians" and members of the "Piagetian School". I shall refer to those Piagetians who consciously and/or conscientiously conduct their research in a manner which is strictly consistent with Piaget's structuralist meta-theory "strict-Piagetians". Amongst these latter, however, I include 'Piagetian technologists' (Gilbert and Swift, 1985), i.e. those who claim to maintain a "sceptical attitude" towards Piaget's structuralist meta-theory by virtue of their "empiricism" in applying Piaget's doctrines in formal educational settings (e.g. by deverbalizing elicitation techniques), and, thence, deriving recommendations for education policy. My contention is that such researchers end up as 'strict Piagetians' by default: their studies reflect 'logical necessity' as a built-in assumption of their research design. Piaget's structuralist meta-theory is largely immune to empirical criticism and development. Put another way, in a recent critical survey, White concluded that

'[ ] the bulk of Piagetian work has ossified into mass studies in which, ironically when one considers Piaget's concern for the individual, children are forced into labelled groups. Nearly all of the Piagetian work falls into four categories: development of group tests to replace Piaget's clinical interview techniques; assessment of the proportions of a population in each of Piaget's stages; measurement of the relation between stage membership and another variable, such as school performance; and attempts to promote attainment of stages.'

(White, 1983, p.5)
I shall argue that such concerns are all that the work of strict
Piagetians has reference and relevance to: such work tells us
nothing of substance for improving pedagogy and this is an
inevitable consequence of Piaget's structuralist meta-theory. It
needs hardly be said that it is against strict Piagetians (and
Piagetian technologists) that my arguments are principally directed:
I have nothing against Piagetian research conducted and confined
within an ivory tower.

Accordingly, my critique shall consist primarily of formal
arguments directed against those key aspects of Piaget's meta-theory
that I have already discussed. Although many of these criticisms
(the majority of which I have appropriated from others) complement
the burgeoning empirical criticisms of Piaget's stage theory I shall
refer to these latter only incidentally since they are already well
known. I shall begin justifying my contention that Piaget's theory
is conservative-activist by appealing to arguments due to Jerry
Fodor in (P-P).

Fodor argues that Piaget's demand for invariance of the sequence of
stage acquisition as a matter of logical necessity is incompatible
with his declared rejection of innateness, which, in this context,
may be understood to imply a priori construction.

Fodor's argument is a meta-logical, or more generally, a
meta-conceptual, critique of Piaget's psycho-logical underpinning of
his theory of stages. That is to say, Fodor does not advance
specific criticisms of Piaget's formalisation of stages. Rather,
he advances general arguments for the impossibility of acquiring
'more powerful' structures with only the basis of 'weaker' ones to
work from.
Fodor (in P-P) introduces the example he will employ:

'Suppose we have hypothetical organism for which, at the first stage, the form of logic instantiated is propositional logic. Suppose that at stage 2 the form of logic instantiated is first-order quantificational logic. The particular example does not matter in any respect, except that I want it to be clearly a case of a weaker system at stage 1 followed by a stronger system at stage 2. And, of course, every theorem of a propositional logic is a theorem of first-order quantificational logic, but not vice versa'.

(Fodor, in P-P, p.148 : my emphasis).

And then the crux of his argument:

'Now we are going to try to get from stage 1 to stage 2 by a process of learning, that is, by a process of hypothesis formation and confirmation. Patently, it can't be done. Why? Because to learn quantificational logic we are going to have to learn the truth conditions on such expressions as 

\[(X)Fx.\]

And, to learn those truth conditions, we are going to have to formulate, with the conceptual apparatus available at stage 1, some such hypotheses as 

\[(X)Fx \text{ is true if and only if} \]

... But of course, such a hypothesis can't be formulated with the conceptual apparatus available at stage 1, that is precisely the respect in which propositional logic is weaker than quantificational logic. Since there isn't any way of giving truth conditions on formulas such as all 

\[(X)Fx\]

in propositional logic, all you can do is say: they include \(F_a\) and \(F_b\) and \(F_c\), and so on.'

(Fodor, in P-P, p.148 : original emphasis).

Since there can be no rule governed symbol manipulation - no "computational procedure" - for a person at 'Stage 1' to acquire 'Stage 2', the whole system collapses upon itself.

Genetic-Epistemology does not allow that transition between stages can be effected by learning. Fodor concludes that in point of logical (though not empirical) necessity, Piaget must endorse a theory of stage transition which is the same as, or similar to, one of three that he proposes:

'[ ] God does it for you on Tuesdays, or you do it by falling on your head, or it is innate [ ]'

(Fodor in P-P; p.155)
For Fodor, innateness seems the most appropriate consequence from his reductio ad absurdum of Piagetian logical necessity since this is closest to his own views. I personally think that Piaget adopts a course most similar to the middle one, as I shall now try to show.

Piaget, as we have seen, claims the order of stage acquisition to be invariant as a matter of logical necessity.

Now, for logicians to seriously entertain any claim to logical necessity there must also be a demonstration (i.e. accompanying proof). Piaget has never advanced one: as Gruber and Voneche put it

'[ ] the demonstration that this sequence is logically necessary remains to be done'.

(Gruber and Voneche, 1980, p.xxiv)

Piaget's defence for his non-demonstration of logical necessity would appear to start with a distinction he has often claimed to exist between his meaning of a structure and its formalisation:

e.g. 'The discovery of structure may, either immediately or at a much later stage, give rise to formalisation. Such formalisation is, however, always the creature of the theoretician, whereas structure itself exists apart from him'.

(Piaget, 1971b; p.5: my emphasis.)

In the book from which this last quotation was taken, Piaget later elaborates both this distinction and defence in a section entitled 'The Limits of Formalization':

'In 1931 Kurt Gödel made a discovery which created a tremendous stir, because it undermined the then prevailing formalism, according to which mathematics was reducible to logic and logic could be exhaustively formalized. Gödel established definitively that the formalist program cannot be executed. In the first place, he showed that no consistent formal system sufficiently "rich" to contain
elementary arithmetic (for example, the system of Russell and Whitehead's *Principia Mathematica*), can, by its own principles of reasoning (a fortiori, by those of the relatively "weaker" systems) demonstrate its own consistency; second, that any such system allows for the generation of propositions which are "formally undecidable," or, to use yet another technical expression, that any logical system that might appear capable of serving as foundation for mathematics is "essentially incomplete." Though it was later discovered, by Gentzen, that consistency proofs of elementary arithmetic can be furnished by employing principles of reasoning "stronger" than those used within arithmetic, the consistency of these stronger rules of inference—roughly, those of Cantor's transfinite arithmetic—can only be demonstrated by appealing to a logical theory of yet a higher rank. In other words, since Gödel we know that the axiomatic method has certain inherent limitations, though these limits can be "shifted" by shifting systems.

[ ] From Gödel's conclusions there follow certain important insights as to the limits of formalization in general; in particular, it has been possible to show that there are, in addition to formalized levels of knowledge, distinct "semi-formal" or "semi-intuitive" levels, which wait their turn, so to say, for formalization. The limits of formalization are not laid down once and for all, like the walls of China, but, instead, are "moveable" or "vicarious."

(Piaget, 1971b, p.32-3; original emphasis)

Piaget apparently considers his invocation of Gödel's Incompleteness Theorem to constitute some kind of justification for his incomplete formalisation of stages, i.e. his non-demonstration of logical necessity. I see a number of problems with this.

First of all, Gödel devised his theorem for application to what might be called "standard" forms of propositional logic, where "standard" forms is understood for present purposes to mean that the axiom systems employ only general propositions, i.e. 'universal statements'. (The title of Gödel's famous 1931 paper was 'On Formally Undecidable Propositions of *Principia Mathematica* and Related Systems'—my emphasis).
Now Piaget's idiosyncratic brand of logic, his 'psycho-logic' differs from "standard" forms of propositional logic in that, for example, it includes propositional functions (i.e., specific instances). Psycho-logic is thus an empirical form of logic and, as such, exhibits a fundamental qualitative difference from the "standard" forms to which Gödel devised his theorem to apply.

Whether or not Gödel's Theorem can be applied to psycho-logic is a matter which Piaget neglects to consider - he simply assumes that psycho-logic is a 'related system'.

I conclude that until a case for this can be made, Piaget's appeal to Gödel's Theorem must be regarded as illegitimate, i.e., it provides no justification whatsoever for Piaget's incomplete formalisation of stages. (For reasons that I shall not enter into here (but see section 3.4.2., below), I happen to think Piaget was quite right to eschew "standard" forms of propositional logic as a language for formalising human cognition. Unfortunately, the arguments that I advance below suggest that psycho-logic is even more inadequate for this purpose).

Let us now assume that Gödel's Theorem does have application to psycho-logic as Piaget claims.

This lands Piaget in even greater trouble.

To demonstrate my contention, I shall assume that Piaget's description, below, of some Gödelian consequences for "standard" propositional logics applies equally well to psycho-logic:
'... [a] logical system, through a closed whole with respect to the theorems it demonstrates, is nevertheless only a relative whole: it remains open at the top with respect to those theorems which it does not demonstrate ..., and, since the primitive conceptions have all sorts of implicit elements, the system is open at the bottom as well.'

(Piaget, 1971b; p.30)

The situation can be analogously compared to a hyperboloid which is open at both ends. Gödel's Theorem rules out the possibility of an "exhaustive formalisation", a comprehensive demonstration (the open ends). Piaget seems to contend, however, that someone analogous to Gentzen, to whom he referred in the penultimate quotation, might yet come along and provide a "limited" demonstration, i.e., a demonstration of logical necessity between at least some structures (the hyperboloid). Presently unformalized (and possibly unformalizable) 'precursor operations', e.g. sucking, eye movements, grasping, and which are themselves probably preceded by some kind of "neuronal wiring", are at the bottom of the hyperboloid whilst presently formalized 'formal operations' are anticipated to be succeeded by "meta-formal operations" (cf. e.g. Arlin's, 1975, postulated fifth developmental stage - briefly discussed in Gilbert and Swift, 1985, p.688 : included in Appendix 1). My analogy may be represented by means of the following diagram:

![Diagram](image)

**Figure 3.3.** Diagram Showing Areas of Demonstrated and Un-Demonstrated Logical Necessity Within Piaget's Theory.
Because of this possibility we are apparently justified to go on claiming logical necessity. So be it. Now, what would happen to Piaget's claimed autonomy of the individual person in their act of (re) creating the next, 'stronger', structure? The answer is that it would disappear completely. Personal, individuated purposefulness of construction and learning would be denied a rôle: it would be sacrificed on the altar of Piaget's 'logical determinism' (Gruber and Vonèche's, 1977, p. xxxvii, apt descriptor of the central characteristic they see in Piaget's brand of constructivism).

Wilden, a systems theorist, concludes his own trenchant criticism of Piaget's use of Gödel's Theorem thus:

'Gödel's Proof is a double bind, and so is the square root of minus one. We are consequently led to remark that Piaget - like all of those outside the poetry of mathematics who use 'mathematics' as a tool to avoid having to think about what they are actually doing - does not understand that most, if not all, of the paradoxes of mathematics are the result of making the discourse of mathematics into a closed system BY EXCLUDING THE MATHEMATICIAN.

It is the mathematician (not the theory) who sees that the Gödelian sentence is 'true', and inconsistent with the axioms that produced it. But Piaget makes this into a property of the mathematical structure, not of the relation between the mathematician and his mathematics, ignoring once again his own model of the interaction between organism and environment.'

(Wilden, 1980, p.347 : my emphasis. I have combined a footnote with the main text).

Now is perhaps the time to recall that Piaget proposes equilibration to be his mechanism of construction, his process of learning. But what follows from arguments such as those recently discussed is that Piaget's claim for logical necessity is incompatible with equilibration: accommodation, one half of the equilibration
process, is redundant (this also effectively undermines any claim that Genetic-Epistemology is a realist theory of knowledge). Brown and Desforges comment:

'[Piaget's] account of learning is so circumscribed as to define it out of a role in transition'

(Brown and Desforges, 1979, p.161)

Piaget's notion of equilibration is both vague (see e.g. Bruner's 1968, criticisms) and limited: it contributes little to our understanding of transitions within stages and nothing to our understanding of transitions between stages. As Boden (1979) concludes:

'[ ] equilibration within a stage must be radically different from equilibration between stages, which latter should perhaps not be described in terms of equilibration at all.'

(Boden, 1979, p.139 : my emphasis)

So how does Piaget avoid drawing the same conclusion as Fodor when, for example, he elsewhere claims that;

'[ ] all learning presupposes a logic, and .. the learning of logical structures is itself based on logical or preliminary pre-logical structures, and this is an endless regress'

(Piaget, 1971a; p.55 : my emphasis)

The following, not un-typical, quotation represents the nub of his reply to Fodor:

'[ ] the previous structure already contained something of the subsequent one, containing it not as a structure, but as a possibility. What is this possibility; what is the set of all possibilities? I believe that the set of all possibilities is as antinomic a notion as the set of all sets. I believe that the "possible" is a process that progressively enriches itself : weak structure opens up only a few possibilities : a stronger structure opens up a large number of possibilities.'

(Piaget, in P-P; p.150 : original emphasis).
This is surely a case of *obscurum per obscurius*? Piaget's notion of what goes on in the 'levels of preparation' seems decidedly "semi-intuitive". As a non-Piagetian, and as a constructivist, I naturally welcome the individual being allowed this rare spell of freedom from the structuralist yoke. But this is a fatal concession for Piaget because he fails to present anything like a half-way decent *theory* at the very point most needed to sustain a claim to *revolutionary-activism*, viz., transition. The Piagetian individual is left stranded up a creek without a paddle. Piaget's allusions to "containment as a possibility" explain nothing: the process(es) of transition remain a complete mystery. We might just as well claim that we effect transition by falling on our heads! Piaget ultimately faces the ancient learning paradox of Plato:

'[
] a man cannot try to discover either what he knows or what he does not know [ ]. He would not seek what he knows, for since he knows it there is no need for the inquiry, nor what he does not know, for in that case he does not even know what he is to look for.'


In another sense, however, Piaget's liberal sounding talk of 'possibility' is downright disingenuous: he protests far too much. This is because whatever possibilities an individual may explore in the mysterious 'levels of preparation', Piaget's joint commitment to universality and necessity, which receives partial and formal expression in his doctrine of logical necessity, dictates that such an individual must always end up **re-creating the next, higher, structure as currently defined by Piaget(ians)**. Such logically deterministic "re-constructivism" is incompatible with revolutionary-activism; it is, however, of the essence of conservative-activism. Only when the individual has achieved formal operations can we take Piaget's liberal sounding remarks on 'possibility' seriously. But why should we wait until then?
A survey of the history of structuralism corroborates the conjecture that such doctrines have always been outsmarted by the collective, but individualated, ingenuity of that set of persons to which they have each been applied. Sooner or later, structuralist doctrines have to start fighting rear-guard actions. Genetic Epistemology is no exception, as the following example shall demonstrate.

Piaget was fond of quoting a little boy, aged 5 years 9 months, who when questioned about his apparent ability to conserve number replied:

'Once you know, you know for always'

(Piaget, 1964, p.184)

This piece of evidence fits in perfectly with Piaget's structuralist meta-theory and its formalisation: it is exactly the sort of empirical consequence one would expect from it, regardless of whether a "subject" is helpful enough to verbalise it in this straightforward way.

It was therefore unfortunate for Piaget and his colleagues that a substantial minority of their "subjects" did not "perform" quite so well. With a given task, individuals knew it on this occasion but not on that; or, with different tasks, but each purporting to identify the same structure, they always seemed to know it in this task but never in that.

This minority of "subjects" proved sufficiently recalcitrant that their responses had eventually to be recognised as being due to more than local insensitivities of the investigative methods. To avoid universality and necessity slipping through his fingers, Piaget introduced his "theory" of décalages way back in the mid 1950's.
Gruber and Voneche comment that Piaget tried to turn his "theory" of décalages to good account by using it

'[ ] as one explanatory principle for development: the very co-existence of the more highly developed and less highly developed structures generates disequilibrium or conflict that leads to further growth.'

(Gruber and Voneche, 1980, p.xxv)

But does it explain anything? Décalage merely brings into sharper focus the antagonism, which I have earlier discussed, between Piaget's demand for logical necessity and his notion of equilibration - to which latter décalage can be compared by analogy or extension. Co-existence of structures of different 'strengths' surely makes a nonsense of Piaget's formalisation of his structuralist metaphysic which requires a radical reconstruction of structures, i.e. a comprehensive re-construction of structures between 'levels of final equilibrium'? No amount of theoretical prevarication can save Piaget's "theory" of décalages from being identified as his most blatant ad hoc hypothesis.

It is tempting for me now to argue that Piaget's "theory" of décalages precludes the possibility of demonstrating logical necessity due to having swapped the logical relationship of 'inclusion' for 'overlap'. This, however, cannot be done straightforwardly because Piaget does not appropriate "standard" propositional logic straightforwardly, a fact to which I alluded earlier. I contend, however, that the differences between psycho-logic and "standard" propositional logic only delay the criticisms I have already discussed and, moreover, introduce new ones as well. I shall demonstrate this through consideration of the work of Robert Ennis, an author who acknowledges a debt to the pioneering formal criticalims of Piaget made by Parsons (1960).
In a paper entitled 'Children's Ability to Handle Piaget's Propositional Logic' (my emphasis), Ennis (1975) argues compellingly that, amongst other things, psycho-logic is both seriously flawed and normative.

Whilst Ennis' arguments are exemplary in their clarity, they tend to be long and technical. With respect to the flaws that he sees in psycho-logic I do not feel personally competent to summarise his arguments without undue risk of distorting them. I shall therefore summarise only certain of his conclusions to such arguments.

Ennis argues that, on one interpretation — an interpretation which he goes to considerable lengths to justify, there are many paradoxes and other odd, undesirable, results in psycho-logic. He argues, for example, that Piaget's brand of propositional logic both prohibits certain inferences one is entitled to make and endorses certain others which one is not entitled to make (entitled on grounds of both commonsense and "standard" propositional logic). I shall consider one example of the latter that Ennis gives.

Ennis argues that Piaget's formula for implication presupposes an isomorphism between observed and possible events, a presupposition which allows one to "logically" affirm such statements as

'... 'If \(X\) is a United States president, then \(X\) is male (for every \(X\)).' ...

(Ennis, 1975; p.21 : original emphasis).

He concludes that

'\([\ \text{the formula for implication ("p.q. v } \neg p.q. \lor p.q., and no cases of } \neg p.q.)\text{ appears to be an inductive guide for going from data to generalizations. It appears to be a formula for induction, yet to satisfy the formula one}\)'}
must already have performed the crucial induction [viz.,
one must already have judged, by means unspecified, that
the examined cases are typical]."

(Ennis, 1975; p.22 : my emphasis).

I point out that inductive logic is one defining characteristic of
Classical-Empiricism - which latter Lakatos regards as the most
influential school of passivist theories of knowledge (as we have
seen, section 3.2., above). Inductive logic is to be deplored less
for its formal inadequacies than for its empirically demonstrated
facility as mechanism by which persons become victims of their
biographies. This is especially likely when inductive logic is
enshrined in a system of thought purporting to be an epistemology.

I shall now turn to Ennis' contention that psycho-logic is
normative or, at least, has a normative dimension.

Ennis begins by citing evidence to suggest that Piaget is widely
considered to be attempting to offer a descriptive rather than a
normative system of logic where the latter is understood to be '... a
way of judging the validity of reasoning' (Ennis, 1975; p.37).

Against the descriptive interpretation, however, Ennis quotes many
passages from Piaget's work which suggest a normative application.
Some relevant words and phrases included within the passages Ennis
quotes from are:

'... inadequate for the solution of the problem we posed ..'
'... stage III subjects struggle against the temptation to
conclude too quickly from ...', '(Stage III B subjects)
know when they ... that it may be included in ...'

(Ennis, 1975; p.38 : Ennis' emphasis)
Ennis goes on to argue that this normative dimension renders the flaws he and others have found in psycho-logic even more problematic. I cannot agree too strongly on this point.

Unfortunately, this normative use of psycho-logic by Piaget amounts to his equating his metaphysical notion of a structure with his formalisation of it: far from being 'a creature of the theoretician' (Piaget quoted above), Piaget has "literalised" his formalisation. Although Piaget may plausibly be interpreted to have mooted his psycho-logic and theory of stages as 'architectonic', in Kant's sense, he does not appear to have heeded Kant's caution that 'in assuming [that the architectonic] has a supreme ground, we do so solely in order to think its universality more determinately' (Kant, quoted in section 3.3., above). The influence of this practice "feeds forward", as well as "feeds backward", on both the design of elicitation methods and the interpretation of responses. This commitment to "universal necessity" ultimately denies, or constrains, the reflexivity of Genetic-Epistemology; it is conservative-activist because its 'activism' is always within limits pre-scribed.

I believe that these last criticisms introduce a new charge which can be brought against Piaget in the course of demonstrating his conservative-activism, viz., Piaget's historicism.

The term 'historicism' has been used to designate a variety of methodological views relating to history and society (Flew, 1979; p.138) and it is not clear which sense(s) Piaget intended when he claimed that '[ ] structuralism tackle[s] historicism, [ ]' (Piaget, 1971b; p.4)
Notwithstanding such possible ambiguities, I shall argue that, on the contrary, Piaget's structuralist-underwritten theory can itself be construed as historicist, in Popper's (1979, 1980) sense of the term, and that historicist doctrines can support only conservative-activist theories of knowledge.

Popper (1980) defines the central tenet of historicism thus:

'the doctrine that history is controlled by specific historical or evolutionary laws whose discovery would enable us to prophesy the destiny of man.'

I contend that historicism is a species of structuralism. The doctrines of the former may be delineated within the latter by virtue of their characteristic means of formulating "structures", viz., by appeal to 'theoretical history' (Popper, 1979; p.39), as I shall now discuss.

For Popper, the historicist's method is informed by an anti-nominalistic theory that he calls 'essentialism' (Popper, 1979; p.27). Popper elaborates his meaning of this term:

'Essentialists deny that we first collect a group of single things and then label them 'white'; rather, they say, we call each single white thing 'white' on account of a certain intrinsic property that it shares with other white things, namely 'whiteness'. This property, denoted by the universal term, is regarded as an object which deserves investigation just as much as the individual things themselves ...'

(Popper, 1979; p.27-8)

The essentialist characteristically proceeds by asking "What is ... ?" questions, e.g. "What is force?", in the belief that it is only by answering such questions that the inquirer shall penetrate to the 'real' or 'essential' meaning of such terms and thereby the real or true nature of the essences denoted by them.
Popper calls those who use this approach 'methodological essentialists' (1979; p.28). (N.B. mere use of "what is ..." formulations is neither a necessary nor a sufficient condition for methodological essentialism).

The methodological essentialist accepts the Heraclitean argument that rational description cannot be applied to things in flux. This has the consequence that knowledge presupposes something that does not change, viz., an 'essence'. For the essentialist

'History, i.e. the description of change, and essence, i.e. that which remains unchanged during change, appear ... as correlative concepts.'

(Popper, 1979; p.33: original emphasis).

This however, is not the whole story for as Popper goes on to say

'But this correlation has yet another side: in a certain sense, an essence also presupposes change, and thereby history. For if that principle of a thing which remains identical or unchanged when the thing changes is its essence (or idea, or form, or nature, or substance), then the changes which the thing undergoes bring to light different sides or aspects of possibilities of the thing and therefore of its essence. The essence, accordingly, can be interpreted as the sum or source of the potentialities inherent in the thing, and the changes (or movements) can be interpreted as the realization or actualization of the hidden potentialities of its essence ... it follows that a thing, i.e. its unchanging essence, can be known only through its changes ..... (for example) the essence of a man - his personality - can only be known as it unfolds itself in his biography. Applying this principle to sociology we are led to the conclusion that the essence or the real character, of a social group can reveal itself, and be known, only through its history ..... the historicist claims that sociology is theoretical history.'

(Popper, 1979; p.33-39: original emphasis).

I hope that the reader has themself already gleaned from my rather lengthy exposition, above, something of the historicist character that I contend Genetic Epistemology to have. I wish, however, to argue for, and to elaborate, three main historicist aspects of Piaget's theory.
Firstly, I accuse strict-Piagetians of 'methodological essentialism', viz., I contend that when young persons articulate their views, strict-Piagetians ask "What is the structure at work here?" rather than, e.g., "Why do you believe this?" or "How is this view of use to you?" Of course, I do not deny that strict-Piagetians employ questions of the latter sort; what I am claiming, however, is that it is the first question that they are really asking and are interested in answering. As Gilbert and Swift put it:

'On re-reading the research literature on the [Piagetian School], which makes up a considerable proportion of published educational research, it is apparent that individuals' alternative conceptions have been sub-ordinated to their ascribed (Piagetian) stage of intellectual development: it is the stage level of intellectual development of individuals and populations which have been the primary focus of attention. Put another way, alternative-conceptions appear to have been used "diagnostically" - as merely a means of identifying or clarifying individuals' stage level of intellectual development. The existence of alternative-conceptions have often served only to demonstrate "stage (un)readiness" for specific concept learning'.

(Gilbert and Swift, 1985, p.693 : original emphasis)

These are issues which I shall elaborate upon later in this sub-section and in Chapter 5. The point I hope to have made however, is that Piagetian structures function as essences which inform the strict-Piagetian's aims and tactics in interactions with young persons.

Secondly, I contend that Piaget's interpretation, and subsequent use, of the history of mathematics represents a commitment to 'theoretical history'.

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Piaget's structuralistic interpretation of the history of mathematics constitutes one of the two, central, "analogies" that Piaget uses to assist him develop and communicate his theory (the other being epigenesis, discussed later):

'What I have tried to do ... is to show that cognitive development in the child - that is to say, the construction of successive structures - is analogous to what is found in history with the formation of mathematics, which is always a generalization from a weaker structure leading to a stronger structure, of which the first becomes a substructure, a subset ...'  

(Piaget, in P-P.; p.150 : my emphasis)

Now, were Piaget actually to use the history of mathematics analogically, or else to appeal to it as a source of phenomena to which his epistemology might legitimately be asked to apply (as e.g. Kuhn, 1970, p.9, bade us do with history of science), then my charge of historicism could not be made. But he does not.

My case hinges on Ennis' demonstration that there is a normative dimension to Piaget's theory and that this implies that Piaget equates his notion of structure with his formalisation of it, discussed earlier. This inference is further strengthened by reference to some direct statements that Piaget has made and which are especially pertinent to my present claim:

e.g. '[ ] a more precise analysis of the "reading off" of experience and of the mechanisms of learning as a function of experience teaches us that this "reading off" is always a function of a logico-mathematical framework, which plays a structuring role and not one of simple formulation, [ ]'

(Piaget, 1971a; p.55 : my emphasis).

and

'[ ] within a few years he [the child] spontaneously reconstructs operations and logic structures of a
logico-mathematical nature, without which he would understand nothing of what he will be taught in school. Thus [sic], after a lengthy pre-operative period during which he still lacks these cognitive instruments, he reinvents for himself, around his seventh year, the concepts of reversibility, [etc] - in other words, all the foundations of logic and mathematics.'

(Piaget, in P-P.; p. 26 : my emphasis).

"Spontaneous reinvention of all the foundations of mathematics" has become the criterion for developmental success. Thus Piaget's appeal to the history of mathematics is not analogical at all, it is historicist.

I mention in passing that the adequacy of Piaget's structuralist interpretation of the history of mathematics has not gone unchallenged. In brief, Piaget has let his structuralist admiration of mathematics run away with him: he has mistaken it for the real thing. As Lakatos points out (and curriculum planners, particularly science curriculum planners, please note):

'Unfortunately there is only one single word in most languages to denote history$\_1$ (the set of historical events) and history$\_2$ (a set of historical propositions). Any history is a theory and value-laden reconstruction of history$\_1$'

(Lakatos, 1974, p.218n.)

In remarks clearly directed at Piaget's history$\_2$ of mathematics, Boden (1979) cautions that

'[, Piaget's many suggestive comparisons between children's thinking and the history of physics and mathematics are inadequate in various ways. For instance, he ignores the influence of the social context in raising questions within these disciplines, and also misunderstands the nature and historical importance of mathematical proof.]

(Boden, 1979, p.100 : original emphasis).
I shall now argue for my view that historicist doctrines can only support conservative-activist theories of knowledge since this needs to be done and shall help clarify my later arguments for a third historicist aspect to Piaget's theory.

At first glance, historicist doctrines might appear to be revolutionary-activist. Such doctrines are, after all, committed to the notion of change and are not incompatible with political revolution. Moreover, historicist doctrines may 'have very marked tendencies toward activism' (Popper, 1979; p.49). All this notwithstanding, historicist doctrines can, at most, allow a conservative-activist theory of knowledge.

The conservative-activism of historicist doctrines is because the historicist individual is condemned forever to fulfill the promise (or threat) of the past. To explain, whilst historicism allows that there is, indeed must be, change, all change can be derived from historical laws. The historicist individual is not born with their "basic expectations", they are, however, born into an historical situation, a social context. This amounts to the same thing for whilst

'[Historicism] does not teach that nothing can be brought about; it ... predicts that neither your dreams nor what your reason constructs will ever be brought about according to plan [original emphasis]. Only such plans as fit in with the main current of history can be effective.'

(Popper, 1979; p.49 : my emphasis).

The 'main current of history' is, of course, that inexorable trend held to have been "discovered", but which has really been proposed, by the historicist. Historicist doctrines are thus conservative-activist.
With the above points in mind, I can now argue for my third historicist aspect of Piaget's theory, an aspect which has particular relevance for strict-Piagetian educationalists. It is this:

Popper identifies what he calls a 'moral law' (p.35) exhorted by disciples of historicist doctrines, viz., 'Help to bring about the inevitable!' (Popper, 1978, p.35: original emphasis).

I suggest that something very like this slogan is taken as an implicit mandate by strict-Piagetians, viz., if the individual continues to develop their intellect, then this development will be according to the historical laws - the theoretical history - of Genetic Epistemology. Hence the many studies on 'cognitive acceleration' (or "helping to bring about the inevitable"). Whilst such studies often beg the question, viz., the existence of structures, they are nevertheless to be applauded for they do at least help to ameliorate the pre-emptive and restrictive aspects of the problem strict-Piagetians have created for themselves (and their "subjects"), viz., the individual recapitulation of the social phenomenon of history of mathematics.

I would now like to summarise my criticisms of Piaget's logical meta-theory.

Piaget's logic has defied understanding by many logicians and certain amongst them have argued that it is faulty. There is every reason to believe that such flaws as have been identified in Piaget's logic will be perennial. Those who do claim to understand
Piaget's logic have yet to communicate their understanding in a generally understood and accepted way.

Now Piaget has claimed that

'.. there is a kind of intellectual dishonesty in making assertions in a domain concerned with facts, without a publicly verifiable method of testing, and in formal domains without a logistic one.'

(Piaget, 1971a, p.12 : my emphasis).

Yet "untestability" in either domain is the most common complaint amongst Piaget's critics (for further arguments and a catalogue of references, see, e.g. Brown and Desforges, 1978).

The opaque and incoherent conflation of empiricism and logicism that is psycho-logic renders Piaget guilty of both kinds of intellectual dishonesty: psycho-logic encourages a sort of "argumentative opportunism" in which Piaget (illegitimately) tries to have it both ways, viz., "standard" logical criticism is often stalled by appeals to the differences between psycho-logic and "standard" logic or to the empirical component, yet he often uses "standard" logic to further his case; empirical criticism is often diverted by invoking the logical part, yet he often cites empirical evidence to further his case.

In the remainder of this sub-section I shall make explicit contrasts with PC^P by briefly examining the different, principal, consequences each approach has for exploring the ideational words of individual persons.

One fundamental difference between the PCP and Genetic Epistemology concerns the nature of that which is each their principal concern,
viv., PCP admits the "whole person" or 'psychological subject':

'Our first consideration is the individual person rather than any part of the person, any group of persons, or any particular process manifested in the person's behaviour.'

(Kelly, 1955; p.47 : my emphasis)

This notion contrasts with strict-Piagetians who not only believe that a person's mental activities can be divided into 'cognitive' and 'affective' categories and studied separately but also that the cognitive category can be studied transindividually (cf. the epistemic subject, p. 57-8, above).

Kelly was emphatic that cognition and affect should not be separated within PCP:

'Particularly I hope that no one will think I am talking about "cognition", as contrasted with "affect". As a matter of fact, I can associate nothing with either of those terms that would justify treating it as a category rather than a dimension of appraisal - and one of doubtful utility at that'.

(Kelly, 1969 :p.9)

This aspect of the Personal Constructivists' approach derives ultimately from their commitment to reflexivity, for they, recognizing that their own experience of attempts to construe the world represents an 'active, creative, rational, emotional and pragmatic affair' (Pope, 1981; p.2), do not then pre-empt which experimental modalities or qualities shall be of pertinence to the persons whose constructive lives they investigate. As Kelly put it

'[ ] "hardening of the categories" [1], a common affliction amongst scientists, usually marks the end of the creative phase of a distinguished career.'

(Kelly, 1969, p.294)
Bannister has commented upon the reflexivity of PCP:

'Kelly proposed, as a primary requirement for any psychological theory, that it be reflexive — that is to say that the theory account, among other things, for its own construction, since its construction is a psychological act. This demand for reflexivity has to be met at many levels. It needs to be met by ensuring that we do not use one psychological language and set of assumptions in talking about our "subjects" and a different language and set of assumptions in talking about ourselves, "the scientists". Further, it requires us to regard our personal experience not as subjective, anecdotal nonsense, inadmissible in scientific discourse, but as a source of argument and a way of exploring the meaningfulness of the generalisations which we make. We ought not to proclaim publicly that which has no personal meaning for us.'

(Bannister, 1979, p.27-8)

Now, I hope that my earlier discussion (section 3.4.) has demonstrated that there is nothing in the formal content of PCP which would deny reflexivity — even to the "level" of (M)ethodological-pluralism! Strict Piagetians, by contrast, can extend, at most, only a constrained or "conservative" brand of reflexivity due to their commitment to 'universal necessity' (objectivism). To explain what I mean by this, I cite some remarks by Fodor in which he provides a most useful "Kantian perspective" of Piaget's theory:

'It seems to me that the following is at least one way of formulating the Piagetian view: Suppose you are Kant and are interested in writing the "First Critique," that is, you are interested in characterizing the computational capacities of the organism in terms of some very general constraints on the character of the concepts available to it. One way of reading the Piagetian position is to say that if you did that for several different time slices of the organism (instead of just considering the adult), what you would get is a fundamentally different galaxy of constraints on the organism's concepts.' (Fodor, in G-154,p.147)

I believe that the character of Fodor's 'fundamentally different
galaxy of constraints' may be further illuminated by reference to Bolton's (1977) application of R.G. Collingwood's definition of 'philosophical concepts':

'[ ], what characterises philosophical concepts is that differences of degree exist in combination with differences of kind in what Collingwood calls "a scale of forms". In a scale of forms there is both a difference in kind between the various forms which embody the essential element in the concept and a difference in the degree to which these forms embody it. The concept of intelligence viewed in a Piagetian perspective constitutes a scale of forms since intelligence is seen as existing at different levels which differ both in degree and in kind: thus, the concrete level represents a higher form of intelligence than the pre-operational level but it also differs in kind, being qualitatively different'.

(Bolton, 1977, p.39 : my emphasis)

By construing intelligence as a scale of forms, strict-Piagetians propose different psychologies (construction characteristics) for persons who have not demonstrated their "equivalence", i.e. formal operational status. This contrasts with Personal Constructivists who do not propose universal differences in kind between psychological processes of construction (the "construction characteristic" is held to be the same for all persons), notwithstanding possible differences in degree. This latter sort of difference, however, does not allow a normative reductionism for whilst Personal Constructivists accept the notion of difference between construction systems, they do not accept the notion of superiority of one construction system relative to another (i.e. on an inter-personal comparison), as we have seen (section 3.4.).

For the Personal Constructivist, the utility of another person's construct system is judged primarily in terms of the congruency
between that person's perceptions and their personal, and volitionally pre-established, reference value(s) or "test-criteria". As Kelly put it:

'If we reach an understanding of how a person behaves, we discover it in the manner in which he represents his circumstances to himself.'

(Kelly, 1955, p.16)

This has the methodological corollary that

'I am very sceptical of any piece of human research in which the subject's questions and contributions have not been elicited or have been ignored in the final analysis of results.'

(Kelly, 1969; p.132: my emphasis)

Personal Constructivists, then, strive not to make unidirectional inferences about the reasoning processes of other persons: they desire only negotiated inferences. In this way the meta-theory of PCP is not imposed on investigated persons but, rather, explored with them. Persons with and about whom PCP research is concerned are not referred to as "subjects" (a term which might just as well be "object" for it is usually understood in the senses of "persons having been subjected to" or "persons to be subjected to"...) but, rather, simply as "persons" (or, perhaps, "collaboratees": see Chapter 5).

The strict-Piagetian has the full sanction of their doctrine to initially consider their "subject" as "guilty of a lower intellectual stage until proved innocent". Only when a "subject" has shown themself to be capable of formal operations are their questions and contributions able to be taken as seriously as strict-Piagetians take those of themselves: full accreditation in
the research enterprise for a strict-Piagetian "subject" requires full operational competence to have been demonstrated as a prior and necessary condition. But it is my impression that it is precisely at this point that strict-Piagetians abandon their "subjects" in favour of discussion amongst themselves. At no level of intellectual development are their "subjects'" questions and contributions considered in the final analysis of results.

In his main paper on stages, Piaget cautions

'[ ] if we are speaking of stages, the order of succession of behaviours must be considered as constant, that is to say, a character will not appear before another in a certain number of subjects and after another in another group of subjects. Where we find such variations, the characters in question are not usable in establishing stages.'


This amounts to be a rationale for data-dumping.

Now it is possible that Piaget's postulated mental structures do indeed exist and that their formalisation only has application to the tasks he orginally set. This in itself would be an interesting finding worth pursuing, and, as such, I would have no objection to it, i.e. even if it involved data-dumping (I am grateful to Professor Jack Easley, personal communication, for pointing out this possibility to me - it informs my acceptance of "ivory tower Piagetianism", mentioned earlier). There is, however, an enormous research effort devoted to showing that Piaget's formalised structures have predictive validity across a very much wider range of tasks. Where there is the slightest suggestion made that such research might have educational import I be very worried that many
Idiosyncratic 'behaviours' by 'subjects' get regarded as 'extraneous elements' (cf Piaget quotation, p. 53 above) and ignored. When this occurs, and I believe that it occurs on a massive scale in the work of 'Piagetian technologists' and whose efforts constitute the bulk of 'Piagetian' research, then I regard any inferences made with respect to education as intrinsically unsound. Educational inferences made in strict accordance with Piaget's theory risk the "baby" being thrown out with the "bathwater"!

In PCP there is no "necessity" to be had outside the individual person's personal, and possibly idiosyncratic, purposes and anticipations (though there is good reason to believe that there is widespread commonality of such purposes and anticipations amongst persons). It is these that are of prime interest and importance to PCP and their investigation will not optimally be achieved, and may be undermined completely, if persons are approached through a 'scale of forms'. In PCP research there is no question of "testing a whole class at a time" or, indeed, of "testing" an individual person. As Kelly points out:

'It is of course, possible to restate many of the achievements of man in terms of the logic we have so far formalised. But this does not tell us much about how the achievements came about. Nor does it help at all in finding how to disengage ourselves from the logic by which these achievements are presently sustained so we can go on to greater ones.'

(Kelly, 1969, p.114 : my emphasis)

I suggest that "cognitive diagnosis" of students and curricula according to the canons of "cognitive health" implicit in Genetic Epistemology tell us very little that can be of educational use because persons' individuated purposes and anticipations are
ignored. Shayer and Adey's (1981) 'CAT' ('Curriculum Analysis Taxonomy'), for example, has a peculiarly Carrollian quality: there is a "grin" (operational competence as judged by Piaget's formalism), but no "body" (the individuated purposes and characteristics of the child). A comment by Kelly, made with reference to the Law of Excluded Middle in particular (q.v. section 3.4.2., below), and to "logicism" in general, may also be pertinent here:

'...we find a failure to take into account a psychological fact, the fact that human thought is essentially constructive in nature and that even the thinking of logicians and mathematicians is no exception.'

(Kelly, 1969, p.71: my emphasis).

The Personal Constructivist considers persons' reasoning to be highly context related. They exclude neither themselves nor teachers from their investigations (by contrast with strict-Piagetians who do).

Genetic Epistemology ultimately fails both formally and empirically - like all structuralist systems, so far proposed, it is struggling to survive as Person's audacities multiply. But it also fails in the far more important humanistic sense of "diluting" the individual person in the "universal" to the point of losing them altogether (cf. Césaire quotation at the beginning of this chapter). In response to this, however, it might be argued that Kelly's emphasis on the uniqueness of the individual person loses them through "fragmentation in the particular". That this does not happen within PCP is something that I shall argue in later chapters.
3.4.2. Relevant Contrast (2): Objectivist Theories of Knowledge are 'Conservative-Activist' (e.g. Popper).

In this sub-section I shall argue against Popper in a manner analogous to that which I have with Piaget, viz. Popper's theory, qua objectivist constructivist epistemology, is ultimately 'conservative activist' principally by virtue of his "logicism" (I shall develop a complementary case against his empiricism in Ch.4).

I shall also, and more importantly, however, first take this opportunity to begin to assert the complementary, positive, case for PCP, qua relativist constructivist epistemology, as an alternative to objectivist theories of knowledge in general and to Popper's in particular. I shall direct my arguments principally against those of Popper because, as I mentioned in Chapters 1 and 2, I consider him to be the most influential proponent of objectivist constructivism and because I consider the case against Baconian empirical inductivism and Logical Positivism to have been largely won (within the community of philosophers of science, though not within that of pedagogues) and not in need of detailed rehearsal here.

This I shall do by returning to conduct a preliminary exploration of my contention, introduced in section 3.4., above, that Kelly's notion of the 'construct' initiates a qualitatively different and preferable mode of enquiry from that implied by the more familiar, "classical", idea of the 'concept'. My initial assumption (which I shall later justify) is that, in any practical purpose or application all objectivist theories of knowledge endorse some notion indistinguishable from that of the

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"classical" concept. In this sub-section, I shall concentrate upon exploring the formal differences between the construct and the concept and shall pursue a Kellyan case that the former is preferable to the latter by virtue of its entailment of 'relevance' and 'responsibility' (understood in specialised senses) mainly in Chapter 4.

Kelly identifies the classificatory notion of concept as the 'classical' one and briefly and uncontroversially characterises it as being '... a property attributable to two or more objects which are otherwise distinguished from each other..' (Kelly, 1969, p.9). This "concept-of-concept" has also been named by (amongst others) Bolton as the 'traditional [ ] theory of abstraction' (1977, p.9). He argues that it can be traced back to Aristotle and that it has received sustained and influential expression through British empiricist philosophers, notably Locke and Hume.

Kelly's main criticism of the classical concept of concept is that it renders the current specified reference of any example '...indiscriminate and psychologically footless..' (Kelly, 1969, p.9), through '...lump[ing] together the contrasting and the irrelevant' (Kelly, 1955, p.63).

Underlying the classical concept-of-concept is a commitment to the so-called 'Law of the Excluded Middle'. Kelly summarises this law thus:

'What this law proposes is that for any proposition there is only one alternative. I call an object ['A'] a spade ['B']. There is only one alternative to calling it a spade - to call an object not a spade!'.

(Kelly, 1969, p.69).
In this law, 'A' is 'B', or 'A' is not 'B': each object ('A') either possesses or lacks any given property ('B'), there is no third ("middle") possibility. (N.B. "Middle" here has no connection with the middle term of the syllogism).

Now Kelly does not distinguish between different formulations of this Law but it shall be useful to me to do so now for my case against Popper, later. Thus, there is a 'strong formulation', i.e. "Every proposition is true or false", and a 'weaker formulation', i.e. 'Every proposition is true or not true' (Lacey, 1976, p.63).

Using his example of attributed "spade-hood", Kelly criticises the Law of the Excluded Middle by arguing that it denies alternatives – with the sole exception of the psychologically footless 'not a spade'. Kelly also suggests that the object in question may not usefully be called either a spade or not a spade (it is difficult, though not impossible, to imagine a context in which it would be useful to construe a cushion in terms of its spade-hood).

Kelly's principal objection to the Law of the Excluded Middle is that through it we collude uncritically with the 'dogmatism of [our] subject-predicate language structure [and] that it is often presented under the guise of objectivity', (Kelly, 1969, p.72: my emphasis) and that this has the undesirable result (or carries with it the unacceptable risk) that
we put the onus of choosing between the alternatives on the object itself. We disclaim responsibility for our propositions and try to make the objects we talk about hang themselves on the horns of the dilemmas we invent for dealing with them' (Kelly, 1969, p. 70: my emphasis).

To be sure, if someone asserts that an object is a spade, then we shall usually understand what they mean (we can suppose that this is especially likely to be the case if it is ourself that is asserting such!). But on closer inspection of this example, we can see that, under appropriate circumstances and prior to making the contrast explicit, there may still be plenty of scope for ambiguity in meaning, irresponsibility (wilful or otherwise) and general psychological footlooseness, cf.: "spade vs. fork", "spade vs. club", or even "spade vs. black person" - not to mention the difficulties that may arise from phonetic similarities or identities, e.g. "spayed vs. castrated"!

Any apparent triviality in Kelly's "spade" example should not obscure the importance of the principle at issue. Indeed, sometimes a person's life may depend upon a personal construct(ion of them), although we may not accept personal responsibility for such a consequence if we have placed it upon them in a psychologically footloose manner as Kelly demonstrates with an example:

'If a woman is accused of being a witch, she has to be either a witch or not a witch - it is up to her. The speaker disclaims all responsibility for the dilemma he has imposed upon her'.
(Kelly, 1969, p. 70: my emphasis).

Kelly argues, furthermore, that within PCP

'We consider the contrasting end of a construct to be both
relevant and necessary to the meaning of the construct'.
(Kelly, 1955, p.63: my emphasis).

For Kelly, meaning, relevance and responsibility thus fuse within his notion of the personal construct (I shall develop this point in later chapters).

Kelly claims that some persons argue against the Law of the Excluded Middle by proposing that an object may be appraised in terms of degrees of the single quality or attribute in question. For Kelly, this tactic misses his point:

'This is the notion of shades of grey that can be perceived between black and white. But this notion of 'reifying' the excluded middle by talking about grays is not what we are proposing. In fact we see this grey thinking as a form of concretism that merely equivocates and fails to get off the ground into the atmosphere of abstraction'.
(Kelly, 1969, p.72: my emphasis).

In Kelly's view, "greys" should be admitted, or re-admitted, only within the fully abstract notion of the construct (cf. my earlier discussion of "greys" in constructs, section 3.4). In this approach, for an idea to have 'human purpose' (Kelly, 1969, p.9: I have taken this to be his contrast pole for 'psychologically footless'), i.e. utility, it must exhibit dimensionality and relevant (which is to say, opposite) contrast - a "dimensionality of relevant contrast". In other words, it must be a construct. The dimensionality offered by the Law of the Excluded Middle is a "pseudo dimensionality" since there is no requirement in it for a relevant contrast.
Now, psychologically footless ways of talking about the world have a long and multi-faceted history: Kelly points out that the Law of the Excluded Middle has been accepted as a basic principle of logic for the past 2,400 years (Kelly, 1969, p.69), i.e. since the time of Aristotle.

Returning to my purpose for this sub-section, however, I suggest that such approaches have, if anything, received additional impetus particularly over the last 300 years or so and that this has been mainly due to the still prevailing tendency to construe the ideational fruits of certain of Personkind's activities, notably "science", as an achievement of objectivist epistemology ("objective method").

I shall try to render this view plausible by considering the specific case of Popper's objectivist constructivist theory of knowledge. In this cause, I shall argue that Popper's theory is psychologically footless through endorsing in effect, though not in logic, the Law of the Excluded Middle.

Turning now to Popper, I shall not labour the point that he considers himself to be a constructivist. Thus Popper, like Piaget, frequently acknowledges his intellectual debt to Kant (cf. e.g. Popper, 1972, p.48 - quoted in section 3.3.1.) and his commitment to constructivist epistemology could not be more explicit than where he entitles a section of one of his books 'All knowledge is Theory-Impregnated, Including Our Observations' (Popper, 1979, section 18: original emphasis) and then proceeds to argue for that proposition.
In his most famous work, *The Logic of Scientific Discovery* (Popper, [1934] 1980), however, Popper tells us of his opposition to the belief that there are propositions of science 'the analysis of [whose] relations compels us to introduce a special probabilistic logic which breaks the fetters of classical logic'. (p. 192). Popper's reference to 'a special probabilistic logic' is, of course, a critical allusion to attempts made by the (then influential) Logical Positivists to sophisticate the inductive logic originally favoured in the context of philosophy of science by Baconian empirical inductivists and other "Classical Empiricists".

Armed against such inductivist approaches with "Hume's Problem" (of induction), which Popper regards as an 'almost flawless gem' (1979, p.88), and further motivated by his dislike of the attitudes he associated with Marxists, Freidians and Adlerians - they all vaunted their views as a "scientific discipline" and all betrayed sympathies to inductive reasoning - and the undesirable human consequences he perceived to follow from their doctrines (see e.g. Popper, 1978, p.31 et seq.) Popper came to believe that all logical relations between the propositions of science can

'...be fully analysed in terms of the 'classical' logical relations of deducibility and contradiction'. (Popper, 1980, p.192: original emphasis).

More specifically, Popper states that

'My proposal is based upon an asymmetry between verifyability and falsifyability; an asymmetry which results from the logical form of universal statements. For these are never derivable from singular statements. Consequently it is possible by means of purely deductive..."
inferences (with the help of the modus tollens of classical logic) to argue from the truth of singular statements to the falsity of universal statements. Such an argument to the falsity of universal statements is the only strictly deductive kind of inference that proceeds, as it were, in the 'inductive direction'; that is, from singular to universal statements.
(Popper, 1980, p.41: original emphasis).

The substance of Popper's proposal may be further clarified through a brief consideration of Modus Tollens.

Modus Tollens is the valid, deductive, rule of inference which has the form

"If p, then q; not q; therefore not p"

(where, in Popper's application, p = a universal statement or proposition such as a scientific law, and q = a singular statement such as an observation statement or an actual observation).

For Popper, application of this logical rule of inference in matters epistemological means that, strictly speaking, we can never say that a knowledge claim is true, or even probably true, but we can, strictly speaking, sometimes say that one is false. Furthermore, Popper proposes 'falsifiability' as a 'criterion of demarcation' for science and pseudo-science and for science and non-science:

'[T]he falsifiability of a system is to be taken as a criterion of demarcation.[ ] it must be possible for an empirical scientific system to be refuted by experience'.
(Popper, 1980, p.40-41: original emphasis).

Popper repeatedly spells out the multiple synonymity which he extends to
"[The criterion of the scientific status of a theory is its falsifyability, or refutability, or testability]." (Popper, 1972, p.37: original emphasis).

For Popper, the hallmark of actual practising good science, as contrasted with philosophically attributing or judging it to be science, is to specify in advance 'potential falsifiers' (Popper, 1980, p.86: original emphasis) for one's own theory.

Now, as I have hitherto presented it, Popper's epistemology would be unworkable because it would not be able to meet even the most obvious and likely of problems. For example, how would we decide between two rival theories both of which are presently unfalsified but falsifiable? Again, a decisive empirical refutation — "conclusive disproof" — would seem to be impossible to achieve in either practice (experimental error) or principle (all observations are theory-laden)? Furthermore, an examination of history of science suggests that every non-trivial theory has had "anomalies" from the moment they have been proposed — what is to distinguish these from 'potential falsifiers' of such theories? Such an epistemology would be "naive" falsificationist, not least, because it could leave us with only falsified knowledge. Science itself would be falsified in the process.

Popper, however, is too astute to be a straightforward naive-falsificationist. He is aware of all the problems that I have mentioned and more besides. By means of ingenious and often complicated manoeuvres, Popper has sought to overcome the problems which beset
naive-falsificationism so as to provide a "methodological" brand of falsificationist epistemology, an empirically 'critical rationalist' theory of knowledge. For my purposes, Popper's theory might usefully be termed a species of 'objectivist methodological constructivism', as contrasted with the basically Kellyan species of 'relativist methodological constructivism' which I endorse and strive to develop.

Popper attempts to leave Modus Tollens recognisably intact within his epistemology, but yet, on the one hand, to weaken his methodological requirement for (possible) 'decisive refutation' and, on the other hand, to provide an alternative to a falsified theory (about which Modus Tollens says nothing).

Popper weakens his demand for falsifiability by allowing for degrees of falsifiability (i.e. methodological falsification): the more numerous, empirically knowable, states of affairs that a theory forbids, the more falsifiable it is (and the more "scientific" it is). The more states of affairs which have empirically refuted a theory, the more justified we are to attribute to its falsehood. Clearly, such "falsification by degree", which allows us to attribute "approximate falsity" to a proposition, remains a more likely result with those theories which, prior to testing, forbid more states of affairs than other theories. Hence, Popper exhorts us to make 'bold' conjectures.

In elaborating his alternative to a falsified theory, Popper attempts to avoid the pitfalls of both "absolute" truth and inductive notions of "probable" truth by providing a means for appraising the past (and only
the past) performance of a theory with respect to its tests and by which we may be justified to attribute to it its "approximate" truth status. This Popper does throughout his intimately related epistemological notions of 'corroboration' and 'verisimilitude' of a theory. Here it is enough to say Popper demands that rational theory-choice (which he understands to be synonymous with objective theory-choice) consists of always choosing that tested but unfalsified theory which has "excess corroboration" over its rival, viz. the better theory is the theory which accounts for all the facts ('content') which its rival accounts for and more. Hence, we should demand an "increase in content" - "excess corroboration" - from our later or preferred theories relative to our earlier or rival theories, and we should avoid making 'ad hoc' hypotheses. For Popper, an ad hoc hypothesis is an amendment to a theory which is made in the light of otherwise falsifying evidence but without also increasing the explanatory (empirical) content of the theory beyond that of the "anomalous" evidence. To avoid ad hoc-ness in such hypothetical amendments to theory, Popper demands that they also be 'independently testable' - as he puts it with a caution:

'For if it is admitted that a theory may be ad hoc if it is not independently testable by experiments of a new kind but merely explains all the explicanda, including the experiments which refuted its predecessors, then it is clear that the mere fact that the theory is also independently testable cannot as such ensure that it is not ad hoc. This becomes clear if we consider that it is always possible, by a trivial stratagem, to make ad hoc theory independently testable, if we do not also require that it should pass the independent tests in question; we merely have to connect it (conjunctively) in some way or other with any testable but not yet tested fantastic ad hoc prediction which may occur to us (or to some fiction writer)!' (Popper, 1972, p.244: original emphasis).
In Popper's epistemology, scientific knowledge is held to grow by an endless series of conjectures and refutations. The touchstone for progress in his prescription (which complements his demarcation criterion, cf. earlier quotation), is refutation, not verification as per Classical Empiricism and Logical Positivism:

'I can gladly admit that falsificationists like myself much prefer an attempt to solve an interesting problem by a bold conjecture, even (and especially) if it soon turns out to be false, to any recital of a sequence of irrelevant truisms. We prefer this because we believe that this is the way in which we can learn from our mistakes; and that in finding that our conjecture was false we shall have learnt much about the truth, and shall have got nearer to the truth.' (Popper, 1972, p.231: original emphasis).

For Popper, however, methodological falsification also implies

'[ ] the urgency of replacing a falsified hypothesis by a better one. In most cases [my emphasis] we have, before falsifying a hypothesis, another one up our sleeves; for the falsifying experiment is usually [my emphasis] a crucial experiment designed to decide between the two. That is to say, it is suggested by the fact that the two hypotheses differ in some respect; and it makes use of this difference to refute (at least) one of them.' (Popper, 1980, p.87 n.1: original emphasis).

Now, as far as I am presently concerned, the most important thing that I can discern in this last quotation (and elsewhere in Popper's writings) is that within his epistemology there is no necessary requirement for a rival or an alternative theory. Nor, indeed, could there be if Popper's claim to "objectivity" is to be sustained. The epistemological result to which I understand Popper to aspire is merely
"p is (approximately) true or p is (approximately) false"

(Where p = any empirical proposition).

I judge every aspect of Popper's attempt to achieve even this epistemological result to have been thoroughly and devastatingly criticised by others – notably, Kuhn and Feyerabend. Luckily, I do not need to discuss such criticisms here for they provide only indirect support for my present main case against Popper (but cf. Chapter 4). This is that even Popper's intention to provide a sophisticated methodological application of Modus Tollens is not worthwhile because it endorses a conceptualisation of knowledge claims which is indistinguishable from verificationist approaches such as Classical Empiricism and Logical Positivism, which are based upon the Law of the Excluded Middle in respect that neither require a relevant contrast. Hence I conclude that both these traditions are 'psychologically footless' in Kelly's sense. (N.B. construed epistemologically, Kelly's requirement for a relevant contrast in any construct may be understood to be a requirement for a rival or alternative theory to feature in any knowledge claim). This conclusion may seem more compelling if the result to Popperian testing, as I have recently characterised it, and as I here re-present in the form of a general assumption:

"Every proposition is (approximately) true or (approximately) false"
is compared with the "strong formulation" of the Law of the Excluded Middle (cf. my earlier description):

"Every proposition is true or false"

My views expressed above will now serve as a supporting context in which I shall advance an initial case (to be developed and generalised beyond Popper in Ch. 4) for Popper's conservative activism.

I shall begin by examining the assumption of authority implicit in Popper's belief, quoted earlier, that all logical relations between the propositions of science can 'be fully analysed in terms of the 'classical' logical relations of deductibility and contradiction'. Some remarks made by Feyerabend are pertinent here:

'Speaking as participants we [ ] often use [ ] standards [of criticism, rationality, science etc.] without any reference to their origin or to the wishes of those using them. We say 'theories ought to be falsifiable and contradiction free' and not 'I want theories to be falsifiable and contradiction free'. Now it is quite correct that the statements of the first kind (proposals, rules, standards) (a) contain no reference to the wishes of individual human beings or to the habits of a tribe and (b) cannot be derived from, or contradicted by, statements concerning such wishes, or habits, or any other facts. But that does not make them 'objective' and independent of traditions. [ ]. There are many statements that are formulated 'objectively' i.e. without reference to traditions or practices but are still meant to be understood in relation to a practice. Examples are dates, coordinates, statements concerning the value of a currency, statements of logic (after the discovery of alternative logics), statements of geometry (after the discovery of non-Euclidean geometries) and so on'.
(Feyerabend, 1978, p.22-23: original emphasis).
Now, the fact that Popper does relate his use of Modus Tollens to his own practice or tradition, namely 'critical rationalism', and that he does refer, as we have seen, to the origins of his ideas and his wishes and purposes for his theory of knowledge in, for example, his 'intellectual autobiography' (Popper, 1978), should not distract from Feyerabend's main point concerning objectivity - especially with respect to 'statements of logic' such as Modus Tollens. As Popper himself has ultimately to admit:

'[ ] critical rationalism [ ] recognises the fact that the fundamental rationalist attitude results from an (at least tentative) act of faith - from faith in reason. Accordingly, our choice is open. We may choose some form of irrationalism, even some radical or comprehensive form. But we are also free to choose a critical form of rationalism, one which frankly admits its origin in an irrational decision (and which, to that extent, admits a certain priority of irrationalism).

Popper does not try to justify the objectivity of his choice for (his brand of) objectivist epistemology (i.e. critical rationalism), for this, as he correctly goes on to argue, is impossible in principle. Of course we are free to choose between epistemological traditions. But Popper's admission of 'a certain priority of irrationalism' does nothing to counter what I have termed the 'Paradox of Objectivist Constructivism' (Swift, 1985a), viz. from a relativist methodological constructivist perspective, such as Kelly's, to propose a constructivist epistemology by which 'objective knowledge' is an attainable result, is hubris. This is because it denies to others the personal, albeit, sometimes highly sophisticated, act of construction, or 'psychological channelization' (Kelly, 1955, p.46), that created such an epistemology together with its
standards of objectivity in the first place. The creator of an
objectivist epistemology ends up by denying their own act of creation for
they can hardly claim to have created that which is objective. These
persons unwittingly cast themselves in the role of "discoverer" or
"revealer" or even "prophet" (cf. Lakatos' remarks about 'optimistic
Kantians'!) and, as such, they cannot account for the creation of their
own epistemology; at least, not in a way that is open to anyone else. As
Feyerabend comments

'[ ] the belief that some demands are 'objective' and
tradition-independent [ ] plays an important role in
rationalism which is a secularised belief in the power of
the word of God'.
(Feyerabend, 1978, p.20: original emphasis).

Moreover, Popper refers to all relativist epistemologies and even rival
objectivist ones (except, perhaps, where they are recognisable as
sub-species of his own) as 'irrationalist' – a sure case of lumping
together the contrasting with the irrelevant (a point I shall pursue in
Chapter 4)!

The matter does not rest there, however, for the 'Paradox of Objectivist
Constructivism' complements what I have termed the 'Tyranny of
Objectivism' (Swift, 1985a), viz. uncritical normativism and objectivism
are two sides of an epistemological coin. Whilst objectivist
epistemologies cannot themselves be objectively justified they are,
nevertheless, not "fully rational" in Feyerabend's sense, quoted in
section 3.4 above. Nor can they be. Lakatos is quite right to criticise
Popper in the following way:
'He does not answer the question: 'Under what conditions would you give up your demarcation criterion?' (Lakatos, 1978, p.144-145: original emphasis).

Popper simply assumes dogmatically the objectivity of his criterion for objective knowledge. Unfortunately, however, Lakatos' answer to the same question, notwithstanding his equivocation over objectivity (see Gilbert and Swift, 1985), and though an improvement upon Popper's mere assumption, still falls short of the full rationality which I have interpreted revolutionary activism to require.

Basically, what Lakatos does is to repeat his criterion for objectivity (his 'methodology of scientific research programmes') e.g. Lakatos, 1974, within his 'amended meta-criterion' (Lakatos, 1978, p.151: original emphasis)(his 'second order methodology of scientific research programmes': Lakatos, 1978, p.151). The reason for this is not difficult to find. In claiming to propose an objective method (if this is, indeed, what Lakatos is claiming – like Popper, he often uses the term 'rational' ambiguously), there is clearly no better method (epistemology) that such a proposer can conceive. To articulate a "better" meta-method to appraise the putative objective method would not only be contradictory in principle (could one have, say, a "more objective" meta-method?) but would also be, were it possible, self-defeating, for it would result in no one paying any further attention to the putative objective method (who would be satisfied with second-best?).
The tactic of meta-methodological appraisal invites an infinite regress for objectivists (e.g. "Under what conditions would you give up your meta-criterion? your meta-meta-criterion?"). Lakatos is aware of the problem but the apparent extent of his rationale for proposing only a meta-criterion is to assert that '[ ] one must always stop somewhere'. (Lakatos, 1978, p.153: my emphasis). Why? If objectivity of his meta-criterion is what Lakatos is claiming, then I conclude that, ironically, Lakatos himself was conservative activist. By eschewing pretentions to objectivity, Personal Constructivists, by contrast, may apply the construct self-transcendentally to PCP (a pluralistic methodology), thereby embracing a comprehensively critical rationalism (full rationality) but yet escaping charges of emptiness, nihilism and irrationalism.

Popper is only able to disguise his own conservative activism by misrepresenting Kant, as we have seen (section 3.3.1., above), and others. Popper eventually retreats into an obscurantist metaphysics himself, namely, 'simplicity'. (I shall discuss the role of metaphysics in epistemology, including objectivist traditions, in Chapter 4).

Like all objectivist epistemologists, especially those who have achieved a degree of success in getting their ideas accepted, Popper is only too willing in his less formal expositions to embrace personal responsibility for creating his "objective" theory of knowledge. In his intellectual autobiography, for example, Popper quotes Passmore who states 'Logical Positivism, then, is dead, or as dead as a philosophical movement ever becomes'. (Passmore, quoted by Popper, 1978, p.87), and then answers his
own rhetorical question, i.e. 'Who killed Logical Positivism?' (Popper, 1978, p.87), thus 'I fear that I must admit responsibility' (Popper, 1978, p.88). I fear indeed! Popper's pride in his achievement is overshadowed only by his fear that he shall not be attributed personal responsibility for it. It should be remembered, however, that the Logical Positivists' proudest boast was that they had enunciated an objective theory of knowledge, thereby "killing" speculative metaphysics.

Although I am cautious as to the extent to which I am prepared to follow their "sociological" perspective (see Ch.4), Barnes and Bloor have commented on the origins and nature of the "authority" of logic in a way which I endorse:

'Logic, as it is systematised in textbooks, monographs or research papers, is a learned body of scholarly lore, growing and varying over time. It is a mass of conventional routines, decisions, expedient restrictions, dicta, maxims, and ad hoc rules. The sheer lack of necessity in granting its assumptions or adopting its strange and elaborate definitions is the point that should strike any candid observer [ ]. As a body of conventions and esoteric traditions the compelling character of logic, such as it is, derives from certain narrowly defined purposes and from custom and institutionalised usage. Its authority is moral and social, and as such it is admirable material for sociological investigation and explanation. In particular the credibility of logical conventions, just like the everyday practices which deviate from them, will be of an entirely local character. The utility of granting or modifying a definition for the sake of formal symmetry; the expediency of ignoring the complexity of everyday discourse and everyday standards of reasoning so that a certain abstract generality can be achieved: these will be the kinds of justification that will be offered and accepted or disputed by specialists in the field. The point that emerges is that if any informal, intuitive reasoning dispositions are universally compelling, they are ipso facto without any reasoned justification. On the other hand, any part of logic which can be justified will not be universal but purely local in their credibility.
The rational goal of producing pieces of knowledge that are both universal in their credibility and justified in context-independent terms is unattainable'.
(Barnes and Bloor, 1982, p.45-46: original emphasis).

Their next comment is especially telling when compared with Popper's view concerning the priority of irrationalism in allegedly 'critical rationalist' theories of knowledge such as his own (i.e. Popper, 1966, p.231 - quoted earlier in this sub-section):

'There is, of course, a final move that the rationalist can make. He can fall back into dogmatism, saying of some selected inference or conclusion or procedure: this just is what it is to be rational, or, this just is a valid inference. It is at this point that the rationalist finally plucks victory out of defeat, for while the relativist can fight Reason, he is helpless against Faith. Just as Faith protects the Holy Trinity, or the Azande oracle, or the ancestral spirits of the Luba, so it can protect Reason'.
(Barnes and Bloor, 1982, p. 45-46: original emphasis).

I conclude that Popper's theory of knowledge has never got beyond the 'uncritical phase' (Popper, e.g. 1978, p.60: original emphasis) which he tells us 'necessarily precede[s]' a 'scientific or critical phase of thinking' (ibid); specifically, Popper's appropriation of Modus Tollens is historicist in his sense.

Finally, I suggest that Kelly's joint concern that the thinking of logicians, like all persons, should be regarded as essentially constructive in nature and that the possibility of restating many of personkind's achievements in the logic that we have so far formalised does not guarantee the utility of doing so (Kelly, 1969, p.71 quoted in section 3.4.1., above) is as pertinent in criticism of Popper as it is of Piaget.
n.1. I first suggested that Lakatos' taxonomy might help to delineate Kelly's brand of constructivism from that of Piaget and others in my PCKG seminar and accompanying paper (Swift, 1982). This chapter owes much to the constructive criticism which I received from colleagues both then and in a more developed version which I presented to a conference (Swift, 1984).

n.2. But to Kant, at least, not actually blasphemy: cf. my discussion of Kant's valuation of Newtonian physics in section 3.3.1, below.

n.3. This is my term to refer principally to Popper(ians) in section 3.4.2, below. It contrasts with the basically Kellyan approach which I support and which might be called 'relativist methodological constructivism', see Chapter 4.

n.4. I am not suggesting that Kant was altogether blithe as to the nature of the objects in his universe; he would seem to have had a marked preference for those occurring in Newtonian mechanics. This preference, however, is not enforced by his epistemology - cf. my discussion in section 3.3.1., below.

n.5. It should be borne in mind that Lakatos was once Popper's student. Under the influence of Popper, Lakatos may have identified Kant's epistemology with Newton's physics more closely than I would wish - cf. my discussion in section 3.3.1., below. This notwithstanding, Kant's epistemology must still be classified as 'conservative activist' according to Lakatos' taxonomy.

n.6. The same has been claimed for the development of the classical non-Euclidean geometries - see, e.g., Medawar's (1969, p.36) endorsement of others who have made this interpretation.

n.7. The nature and extent of my modifications to Kant's dichotomies may be discerned by reference to Beck's brief commentary on this part of the Critique of Pure Reason:

Kant sets forth three great perennial divisions in metaphysics as the theory of the scope and function of reason. He gives three dichotomies: between intellectualists and sensualists in regard to the object of knowledge, between empiricists and noologists (rationalists) in regard to the origin of knowledge, and between naturalists and scientists (users of the Scientificische Methode, i.e., systematic, "scholastic

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philosophers in regard to the methods of knowledge. Among the latter, Kant distinguishes two types: those who proceed dogmatically, like Wolff, and those who proceed skeptically, like Hume. (Beck, 1978, p.4: original emphasis).

n.8. Indeed, reflexivity applies at the meta-meta-theoretical level of 'revolutionary activism', viz. 'revolutionary activism' is itself an example of a 'prison' and it may yet turn out to be "absolutely wrong"!

n.9. As I shall hope to show, Feyerabend's recommendation in the last sentence complements perfectly Kelly's view that the person is themself 'a form of motion' (1955, p.48).

n.10. I shall discuss Kelly's views on "ontological" determinism in Chapter 4.

n.11. I suggest that Kelly was profoundly impressed with the Quantum Theory for he retained its fundamental formalism within PCP, namely, matrices. This, I believe, is at the core of complaints or suspicions that Kelly's theory is epistemologically idealist. For a discussion and a defence of Kelly on this issue, see Chapter 4.

n.12. This may account for the relatively small impact that Kelly's theory made when it was originally published in America. As Davisson comments

Personal construct theory seems to have staked out a territory that was neither scientific nor humanistic enough in the 1955 sense of the terms for Kelly's views to become a significant factor in American psychological thinking'. (Davisson, 1978, p.30: my emphasis).

n.13. Robey (1973) has distinguished two main "schools" or traditions of structuralism: the first, to which he contends Piaget belongs, is 'an interdisciplinary trend' (p.2); the second, represents a narrower 'acceptation of the term' as has been elaborated in the 'field of linguistics' (p.2). I intend my arguments to apply to all brands of structuralism, however, I shall explicitly consider only Piaget since he has been incomparably the most influential structuralist in a pedagogical context.

n.14. For my immediate purpose, an ad hoc hypothesis may be understood to be an amendment, an "auxiliary addition", to a theory that is made in order to accommodate new, unanticipated and otherwise contradictory or anomalous evidence. I shall discuss a more "sophisticated" formalism of ad hoc hypothesis, due to Popper, in section 3.4.2., below.
n.15. Interestingly, Piaget seems to have shared something very similar to Kelly's initial convictions whilst formulating the basis of his theory, viz. that persons should be construed in the 'perspective of centuries' and that there is an interplay between the 'durable' and the 'ephemeral' in the way that persons contemplate the stream of events. The model of person Piaget goes on to elaborate, however, might be described as "person-as-mathematician" (or perhaps, "person-as-analytical-philosopher") rather than "person-as-scientist". Cf. N16., below.

n.16. Piaget's exclusion of "affective" qualities from his study of cognition perpetuates the dichotomous distinction made by Logical Positivists between 'cognitive' and 'emotive' discourse. Such may, in turn, be construed as variants of the classical distinction, as enunciated by, e.g., Hume (see No. 22, below), between 'is' and 'ought' statements. These are also perpetuated by 'objectivist methodological constructivists' such as Popper (see Chapter 4).

Kelly's stance on this issue may help further to explain Davisson's view (see N. 12, above) that Kelly's theory was not scientific enough, in the 1955 sense of the term, for it to gain widespread acceptance in the U.S.A.

We may also suppose that Piaget, for his part, was more influenced by Kelly by analytical philosophy (the Logical Atomism of Russell and Whitehead and its heir, Logical Positivism) due to having laid the foundations of his theory slightly earlier than Kelly. Moreover, Piaget may be construed to have had strong personal reasons for wishing to debar consideration of the "affective domain" from his own investigation; although modern Piagetians are usually loath to admit it, Piaget greatly admired Freud's study of the affect and Piaget regarded it as complementary to his ideas (Sants, 1981, 1983). Also cf. N. 19, below.

n.17. Exactly complementary conclusions are made by, for example, Matthews (1980) whose investigations concern the development of philosophical reasoning in the young child (i.e. approximately age 4 to 6 years):

"Piaget proposes to validate his claims about developmental stages by finding the same patterns of responses in all children. Such a finding is to be considered a guarantee that the thinking of children really does develop in this fashion. The unusual response is discounted as an unreliable indicator of the ways in which children think: "The only valid criteria ... are based on multiplicity of results and on the comparison of individual reactions." [ ] But it is the deviant response that is the most likely to be philosophically interesting. The standard response is, in general, an unthinking and un-thought-out product of socialisation, whereas the nonconforming response is much more likely to be the fruit of honest reflection. Yet Piaget would have the nonconforming response discounted and eliminated on methodological grounds."
There is yet a further worry. Piaget aims to arrive at children's convictions. He distinguishes answers and comments that reveal convictions from those that constitute what he calls "mere romancing". Romancing, he explains is "inventing an answer in which [one] does not really believe, or in which [one] believes merely by force of saying it". Piaget makes clear in a variety of ways that he has little interest in, or appreciation for, romancing. "One would like to be able to rule out romancing," he says gravely, "with the same severity as [the answer intended simply to please the questioner]." [ ] (At this point the soft outline of the friendly Swiss psychologist, puffing reflectively on his curved-stem pipe, perceptibly hardens into the stern features of the no-nonsense schoolmaster and disciplinarian). It seems most likely that the philosophically interesting comments that a child makes will not so much express the child's settled convictions as explore a conceptual connection or make a conceptual joke. Thus, the most interesting and intriguing philosophical comments are likely to be counted by Piaget as mere romancing.' (Matthews, 1980, p.38-39: my emphasis).

n.18. Or, perhaps, a Schroedingerian CAT, viz. a creature which lives when viewed through a strict-Piagetian perspective, as mediated by strict-Piagetian investigative techniques, but which emerges as dead when viewed through a Kellyan (or ACM) perspective, as mediated by Kellyan (or ACM) investigative techniques. Cf. Chapter 5.

n.19. I suggest that there may be at least one exception which "proves" my rule, namely, Piaget himself! To explain, Piaget intended his theory to have universal application, as we have seen. He managed to initiate and maintain a sizable "school" devoted to exploring, developing and applying his ideas. The formal aspects of Genetic-Epistemology, however, remain very much Piaget's personal invention. Indeed, if we examine Piaget's personal, informal, purposes and anticipations for his theory (e.g. by reference to certain of his very early works and to his intellectual autobiography of 1952), then it can be construed both as a splendid "rational reconstruction" of his own intellectual development and as some kind of personal "life-solution" to certain very severe fears that he suffered from when young.

With respect to the latter construction (for which I have been greatly inspired by the historical researches of John Santos, 1981; 1983; personal communications) we find that, as a child and as a young man, Piaget harboured a tremendous fear of the irrational. More specifically, the young Piaget was terrified by 'autistic thought' (which he construed as domination of intellect by affect) and which nearly engulfed him as an adolescent.

Given that this was Piaget's preoccupative fear, it would seem quite
reasonable that he might want to seek out the 'universal' and the 'necessary'. These notions are common throughout his long, voluminous life's work, but if it is viewed as a whole, then it is, perhaps, not surprising that a long, "middle", phase was primarily concerned with empirical research and that only in his later years did he devote himself to developing his highly abstract logical meta-theory, (viz., the extensive empirical corroboration of his views, as interpreted by himself and others, might have convinced him that his abstract notion of structure was not itself a chimera, an autism).

I must stress that in placing this interpretation I am acting in a manner entirely consistent with the approach I endorse and that I do not intend them as an argument ad hominem.


n.21. Kelly's example may be construed to allude to one of the most important issues raised by post-empiricist philosophy of science, namely, sexual politics in and of epistemology. Cf. my discussion in Chapter 4.

n.22. Hume's problem of induction may be understood to have a logical and a psychological aspect (Popper, 1979, p.3 - 4). The logical aspect or problem, alluded to in my text, concerns the unattainability of a logical justification for making an "inductive leap" from "some" to "all", viz. logical justification or proof for reasoning from (repeated) instances of which we have experience to other instances (conclusions) of which we have no experience (I am here paraphrasing Popper's formulation).

"Hume's problem" should not be confused with "Hume's Law" (again, to use the most popular ascribed title) and which may be summarised by the slogan "You cannot derive an 'ought' from an 'is'".

Popper, like all objectivists, would seem also to endorse Hume's Law, by contrast with post-empiricists who (to varying degrees) do not (see Chapter 4).

n.23. Which is not to say that he succeeds in solving or transcending all or even any of them as his growing number of critics attest (see Chapter 4).

n.24. I use the term 'objectivist methodological constructivism' to refer to those traditions in which 'objectivity' in Brittan's (1978) second sense (see Section 3.3, above) is exhorted in combination with 'objectivism' in Bernstein's (1983) sense (see Section 3.4, above), thereby (as I shall argue in Chapter 4) confounding Brittan's first and second senses of 'objectivity'. (I interpret 'method' always to imply some role for empirical evidence in theory choice as opposed to reasoning alone which is the meaning Kant articulated for method in CPR,
n.25. Chalmers (1982) makes the useful point that sophisticated falsificationists' proposals for how knowledge can be said to grow (known to have progressed) are often misrepresented by their critics and commentators who pay exclusive attention to their demand for falsifiability and to falsifying instances. He provides an account of the sophisticated falsificationists' stance with respect to this matter which, in my judgement, is both lucid and accurate:

'Significant advances will be marked by the confirmation [Popper would say 'corroboration'] of bold conjectures or the falsification of cautious conjectures'.
(Chalmers, 1982, p. 54: original emphasis).

The problem arises for sophisticated falsificationists, however, when this notion of the growth of knowledge is related to their meaning of "rational" (i.e. objective) theory-choice, viz. the requirement for excess empirical content. As Black comments

'[ ] induction seems to creep in by the back door in Popper's theory of "corroboration", that is, of the criteria by which we discriminate between the relative strengths of hypotheses, none of which are falsified by known observational facts'. (Black, 1973, p. 159: my emphasis).

Black does not develop this criticism of Popper, though others do (e.g. Lakatos, 1978, Chapter 3). (N.B. in certain rare cases, such as when the empirical contents of rival theories are exactly the same, the sophisticated falsificationists' rationale for theory-choice is less straightforward, however, the methodological principle of content increase is essentially upheld).

I shall not consider in detail such criticisms of Popper and other objectivist methodological constructivists since their function is more-or-less limited to reductiones ad absurdum and my principal purpose in this thesis is to endorse and develop Kelly's theory as a viable and desirable alternative.

n.26. Albeit, of a special, viz. incommensurable, sort (see Chapter 4).

n.27. Interestingly, Maxwell (1984), who articulates an otherwise uncompromising critique of, and alternative to, (especially) Popper (discussed briefly in Chapter 4), does not put this point so strongly:

'A minor point of criticism (alongside the major criticisms) is that an element of authoritarianism lingers on in Popper's conception of reason, or method. For Popper's method, ideally, determines for us, in a fallible
way, the best choice. It chooses for us, as it were. The view developed here is that putting into practice the heuristic methods of reason enhances our capacity to choose as we really desire: it enhances desirable spontaneity, creativity, freedom, and does not reduce freedom to the one decision to proceed in accordance with the methods of reason. (Maxwell, 1984, p. 88 N.13: original emphasis).

n.28. Popper has also been claimed to misrepresent, for example, Hume (Store, 1982) and Bacon (Urbach, 1982) — not to mention relativist traditions of constructivism where, at least, some of the original protagonists are alive and have responded vigorously: see, e.g., Lakatos and Musgrave (eds.) (1970).

n.29. Toulmin makes an analogous point:

'[ ] In the neurophysiology of the higher mental functions, the problem of reasons and causes reaches — depending on your viewpoint — the ultimate point of acuteness, or of absurdity. Many neuroscientists believe that we are at last within sight of explaining, in neurophysiological terms, all the basic causal interconnections and influences involved in the operation of the brain and the central nervous system. And when that day finally arrives, — as Charles Townes likes to remind us — the scientists concerned will certainly wish to take credit for their intellectual feat. 'Take credit for what intellectual feat?', we may ask: 'For the scientific discovery that strictly causal brain-mechanisms underlie all rational thought-processes — including the scientific discovery that strictly causal brain-mechanisms underlie all rational thought-processes'. (Toulmin, 1970, p. : original emphasis).

The fact that I happen to agree with Popper that he, more than anyone else, was responsible for "killing" Logical Positivism is quite irrelevant to the point which I (and I believe Toulmin) am making.
Chapter 4. Personal Construct Psychology and Post-Empiricist Philosophy of Science

'The history of science as a history of truth is quite unrealisable. The conception itself is internally contradictory.'

4.1 Introduction

My purpose in this chapter is to articulate similarities and contrasts between Personal Construct Psychology (PCP) and influential traditions in philosophy of science as manifested especially by their varying constructions of the 'structure of a scientific theory' and the consequences these have for construing the 'growth of scientific knowledge.' I shall begin, however, by briefly articulating the pedagogic pertinence of doing this.

'The function of theories', we are often (and, I believe, quite rightly) told, 'is to explain' (e.g. Harre, 1972, p.168: my emphasis). But what, then, might be said to constitute or be meant by an "explanation"? The need to answer this question becomes acute when we recall that the prime concern of epistemologists is to articulate how knowledge may be said to grow (cf. Chapter 2) - a concern which implies that 'theories' (explanations) must also grow or, perhaps, must periodically be replaced.

In pedagogy essentially the same question arises through the prevalent aim to promote and achieve "meaningful learning" (this actual expression is most associated with Ausubel). Thus, one

- 4.1 -
author has similarly argued that lecturing involves explaining and that an explanation consists of 'giving an understanding to somebody else' (Brown, 1978, p. : my emphasis). Again, I have argued in Chapter 2 that learning shares with epistemology a preoccupative concern with the growth of knowledge. Here, the implication is that understandings (explanations) must also grow or be replaced.

Now, in this chapter I shall argue that objectivist epistemologies can, by virtue of their objectivism, only articulate impoverished and untenable meanings of 'explanation'. I contend that the prevalence of objectivist epistemology has led to impoverished explanations being promulgated through theories of science teaching, to the detriment of science learning. In this chapter I shall also argue, however, that certain amongst relativist traditions in contemporary philosophy of science treat the notions of explanation and growth of knowledge in a manner which complements that of PCP, indeed, the notion of the construct may be developed by incorporating one of their philosophical innovations within it. This I contend shall be to the benefit of epistemology of science and, thence, of science pedagogy and ultimately of society.

As both a broad framework for, and an introduction to, the sorts of comparisons and contrasts that I shall make within and between objectivist and relativist traditions in this chapter, I shall use Hesse's (1980) distinction between 'empiricist' and 'post-empiricist' accounts of science, respectively. These comparisons and contrasts shall complement and extend many of those begun in Chapter 3. Hesse's distinction is particularly appropriate for my purpose, bearing in mind that Kelly's theory is (or began as) a
means of a five part dichotomy of traditional contrasts that have
been drawn between natural science and the human sciences:

1. In natural science experience is taken to be objective, testable, and independent of theoretical explanation. In human science data are not detachable from theory, for what count as data are determined in the light of some theoretical interpretation, and the facts themselves have to be reconstructed in the light of interpretation.

2. In natural science theories are artificial constructions or models, yielding explanation in the sense of logic of hypothetico-deduction: if external nature were of such a kind, then data and experience would be as we find them. In human science theories are mimetic reconstructions of the facts themselves, and the criterion of a good theory in understanding of meanings and intentions rather than deductive explanation.

3. In natural science the lawlike relations asserted of experience are external, both to the objects connected and to the investigator, since they are merely correlational. In human science the relations asserted are internal, both because the objects studied are essentially constituted by their interrelations with one another, and also because the relations are mental, in the sense of being created by human categories of understanding recognized (or imposed?) by the investigator.

4. The language of natural science is exact, formalizable, and literal; therefore meanings are univocal, and a problem of meaning arises only in the application of universal categories to particulars. The language of human science is irreducibly equivocal and continually adapts itself to particulars.

5. Meanings in natural science are separate from facts. Meanings in human science are what constitute facts, for data consists of documents, inscriptions, intentional behaviour, social rules, human artefacts, and the like, and these are inseparable from their meanings for agents.'

(Hesse, 1980, p.170: original emphasis).

By way of elaboration she adds:

'It follows, so it is held, that in natural science a one-way logic and method of interpretation is appropriate, since theory is dependent on self-subsistent facts, and testable by them. In
human science, on the other hand, the 'logic' of interpretation is irreducibly circular: part cannot be understood without whole, which itself depends on the relation of its parts; data and concepts cannot be understood without theory and context, which themselves depend on relations of data and concepts.' (Hesse, 1980, p.173).

Hesse then presents a summary of the new post-empiricist account of natural science which parallels the five points of the dichotomy:

1. In natural science data is not detachable from theory, for what count as data are determined in the light of some theoretical interpretation, and the facts themselves have to be reconstructed in the light of interpretation.
2. In natural science theories are not models externally compared to nature in a hypothetico-deductive schema, they are the way the facts themselves are seen.
3. In natural science the lawlike relations asserted of experience are internal, because what count as facts are constituted by what the theory says about their interrelations with one another.
4. The language of natural science is irreducibly metaphorical and inexact, and formalizable only at the cost of distortion of the historical dynamics of scientific development and of the imaginative constructions in terms of which nature is interpreted by science.
5. Meanings in natural science are determined by theory; they are understood by theoretical coherence rather than by correspondence with facts.' (Hesse, 1980, p.172-173).

Hesse, again by way of elaboration, adds:

'It follows, so it is held, that the logic of science is necessarily circular: data are interpreted and sometimes corrected by coherence with theory, and, at least in less extreme versions of the account, theory is also somehow constrained by empirical data. The resemblances between this account and the hermeneutic analysis of the human sciences seems so close that, among the more extreme post-empiricists, Feyerabend at least has drawn the explicit conclusion that scientific theories and arguments are closely analogous to the circular reinforcement of beliefs, doctrines, documents, and conditioned experience that
may be found in some religious groups, and in political party lines and their associated techniques of propaganda."
(Hesse, 1980, p.173).

In fairness to Hesse it is necessary to point out, as one of her commentators has done (Bernstein, 1983, p. 33), that she is not saying (and nor does it follow from what she says) that there are no important differences between natural science and human science; rather, what she is asserting is that the standard ways of making the dichotomy are suspect.

4.2 Theory 'Reduction' vs Theory 'Incommensurability' in the Growth of Knowledge

Hesse's claims that the five characteristics of the natural sciences 'presuppose a traditional empiricist view [ ] that is almost universally discredited' (1980, p. 172). This, I suggest, is an exaggeration. Whilst it is true that her account resembles most closely the doctrines of Logical Positivism and its heirs, discussed below, there are certain important features which are retained by objectivist-methodological-constructivist traditions, such as that due to Popper as I shall show, and these traditions manifestly have not been universally discredited – especially in the minds of science educationalists.

To help me to explain why these features were originally created, why they have been retained within objectivist-methodological-constructivist traditions and why they should be abandoned, I shall
briefly examine their origin in the context of developments in 17th century, and especially early 20th century, science and philosophy of science.

From 1620, when Bacon announced his doctrines of empirical-inductivism, history of science presented a fundamental problem, viz. science could only be portrayed as an accumulation of factually true knowledge, i.e. theories induced from, and entirely circumscribed by, factually true observations, if a high degree of selectivity was exercised. In the 17th century, this was particularly so if empiricist-inductivist historians and philosophers of science extended their scope backwards, i.e. prior to their so-called scientific revolution in method. In the 17th and 18th centuries it was at least plausible, if not entirely unproblematic, to endorse and maintain an essentially Baconian image of science due to the relative stability and longevity of the central principles in science. Then, indeed, this image was officially embraced (in England) by the Royal Society under the influence of Boyle (Elkana, 1970, p.16). This notwithstanding, revolutionary changes in the construal of aspects of reality, e.g. the fundamental nature of matter, continued to occur and required Baconian historians repeatedly to re-write their 'true histories'. This "Orwellian" or "Whiggish" practice gradually undermined the plausibility of the Baconian notion of an incursion of 'prejudice' as an explanation for "erroneous" scientific knowledge claims.

Hesse comments that

'The empiricist response to this instability of theory has been the positivist or instrumentalist view of
science as constituted essentially by accumulating knowledge of phenomena of observables, rather than of the fundamental but hidden nature of things. This is the kind of knowledge that issues in technical application, the cumulative character of which cannot be in doubt.'
(Hesse, 1980, p.174: original emphasis).

The claim of science for what I shall term ontological truth thus gave way to a claim for merely phenomenal truth. As Popper (1972, p.107) has said, instrumentalism has the dual attractions of being both modest and simple as judged relative to 'essentialist' doctrines such as Bacon's (not to mention rationalist traditions, such as that due to Descartes) - but yet, I would add, preserves the claim of science to be preeminent in, or even co-extensive with, the attainment of objective knowledge.

An early proponent of instrumentalism in science was Ernst Mach whose most influential philosophical work was published in the last quarter of the 19th century (he died in 1916 still unconvinced of the reality of the atom). Suppe (1978, p.9) classifies Mach's doctrines, and the school which developed them, as 'neo-positivism' because they drew partial inspiration from the social scientist and philosopher August Compte - 'Comptean positivism' (1978, p.8).

Suppe's commentary on Mach's neo-positivism helps to explain the later emergence of Logical Positivism:

'[In the instrumentalist's view] scientific statements must be empirically verifiable, which is to say that all empirical statements occurring in a scientific theory must be capable of being reduced to statements about sensations. [ ] Mach [ ] trie[d], rather unsuccessfully, to develop this approach into an analysis which construes the principles of science as nothing but abbreviated descriptions of sensations.
His lack of success in carrying out this program stems partially from the fact that abbreviated descriptions of sensations cannot account for the fact that scientific principles contain mathematical relationships not reducible to sensations alone. (Suppe, 1978, p.10: my emphasis).

This notwithstanding, the status and appeal of instrumentalism in science might have remained equivocal, with 'mechanistic materialism' and 'neo-Kantianism' as the main competitors at the turn of this century (Suppe, 1978, p.10), were it not for the advent of the special theory of relativity and the quantum theory. Within less than three decades (by which time the general theory of relativity had entailed the special theory and the quantum theory had been superseded by quantum mechanics) these advances were judged by the community of scientists to have vanquished the "classical" physics which had been dominated by Newton for over two centuries.

Instrumentalists were to claim fresh inspiration and corroboration especially from quantum mechanics (as construed by the so-called 'Copenhagen Interpretation'). Amongst the epistemological pronouncements and asides made by its leading contributors and seized upon by instrumentalists in support of their case, perhaps the most notorious is a remark attributed to Niels Bohr:

'There is no quantum world. There is only an abstract quantum mechanical description. It is wrong to think that the task of physics is to find out how nature is. Physics concerns what we can say about nature.'

(Bohr, oral comment attributed by Petersen, 1963, p.12: original emphasis).

Instrumentalism thus became more than a doctrine for disappointed realists, indeed, its basic tenets were taken up and developed by the Logical Positivists, led by Moritz Schlick and his Vienna Circle, into possibly the most ambitious form of intellectual (not
to say, cultural) imperialism ever attempted. For the Logical
Positivists, scientific knowledge was held to comprise all
knowledge; all else was held to be 'metaphysics' and, as such, was
deemed to be not only false but also meaningless. This stems from
their view that only the language of science (what they termed
'cognitive discourse') could meet the stringent conditions of their
'verifiability Principle', enunciated by Schlick, i.e. 'The meaning
of a proposition is the method of its verification' (Schlick, 1936,
p. ).

For my purposes, however, the most important innovation or addition
that the Logical Positivists made to the neo-positivism of Mach was,
as their title suggests, their admittance of "pure", i.e.
"unsensationalized", mathematics to science and of mathematical
logic to their epistemology for this rekindled interest in, and
allowed for a greater role of, deductive reasoning in epistemology
of science. Kraft provides an admirable summary of the
complementary roles and statuses of logic and empiricism in Logical
Positivism:

'In the Vienna Circle two points of view were
fundamental: The special position of logic and
mathematics and the empirical ground of the knowledge
of reality. Logic and mathematics are valid not
because they are concerned with the laws of reality,
neither in its most general laws nor the natural
necessities of thinking, but because they establish
the rules of our language. This is why both are
valid independent of experience. Knowledge of
reality, by contrast, is dependent on experience; in
it lies the ground of its validity.'
(Kraft, 1974, p.187).

The language of science was construed to be comprised of a hierarchy
of levels, with statements that record instrument readings at the
base, and their theories at the apex:
### Language Levels in Science

<table>
<thead>
<tr>
<th>Level</th>
<th>Content</th>
<th>E.g.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Theories</td>
<td>Deductive systems in which laws are theorems</td>
<td>Kinetic molecular theory</td>
</tr>
<tr>
<td>Laws</td>
<td>Invariant (or statistical) relations among scientific concepts</td>
<td>Boyle's Law ('P \propto 1/V')</td>
</tr>
<tr>
<td>Values of concepts</td>
<td>Statements that assign values to scientific concepts</td>
<td>'P = 2.0 atm.' 'V = 1.5 lit.'</td>
</tr>
<tr>
<td>Primary experimental data</td>
<td>Statements about pointer readings, menisci, counter clicks, et al.</td>
<td>'Pointer (\rho) is on 3.5.'</td>
</tr>
</tbody>
</table>

Figure 4.1 Table Showing the 'Logical Reconstructionist'

**Hierarchy of Language Levels in Science (after Losee, 1980, p.175)**

Logical Positivism in its original extreme form, which embraced both the Verifiability Principle of meaning and an epistemologically idealistic rejection of metaphysics, soon gave way to burgeoning formal philosophical criticism and from complementary historiographic examinations of the conduct of science itself.

Chalmers has summarized what remains as perhaps the greatest defect of instrumentalism:

"The fact that theories can lead to novel predictions is an embarrassment for instrumentalists. It must seem a strange kind of accident to them that theories, that are supposed to be mere calculating devices, can lead to the discovery of new kinds of observable phenomena by way of concepts that are theoretical fictions."

(Chalmers, 1982, p.149).
As one example in support of this criticism, Chalmers discusses Kekule's theory concerning the molecular structure of certain organic compounds, notably, Benzene, which were later "observed" by electron microscopes. This example is, perhaps, a particularly compelling example because, as Chalmers points out, Kekule himself had a somewhat instrumentalist attitude towards his own theory: he regarded his ring structures as useful theoretical fictions. Although instrumentalism remained most plausible with respect to micro-physics it did not prove to be immune to similar criticism from even that area. It became accepted that in most contexts scientists not only thought realistically (in an ontological sense) but also that it was empirically plausible, even necessary, that they should do so.

As I have earlier implied, Logical Positivists responded to the critical onslaught by weakening their extreme original position. Thus the untenability of an inductive logic of discovery led Reichenbach (1938, p. 6-7) to introduce a categorical distinction between the 'context of discovery' and the 'context of justification'. On this view, the problems and importance of the context of discovery for the actual conduct of science were not denied, but they were excluded from the concerns of epistemology. The context of discovery was held to fall within the province of psychology and history; epistemology, by contrast, was deemed to be exclusively concerned with, and exclusively competent to deal with, the context of justification. Epistemologists initially attempted critically to maintain ontological "agnosticism". In response to logical criticism (Hume's Problem) they strove to preserve their commitment to verificationism by requiring only probabilistic inferences: they developed 'inductive probabililistic' forms of
logic. By the middle of this century, however, most empiricist philosophers of science had come to accept that theoretical terms neither could, nor should, be precisely definable in observation language (Papineau, 1979, p.10).

Carnap (1956, p.47) introduced his so-called 'double language model' consisting of two, semi-autonomous, languages, namely, an 'observational language' and a 'theoretical language' - the latter containing a postulate system.

Suppe lists the following as paradigm examples of observational and theoretical terms:

<table>
<thead>
<tr>
<th>Observation Terms</th>
<th>Theoretical Terms</th>
</tr>
</thead>
<tbody>
<tr>
<td>red</td>
<td>electric field</td>
</tr>
<tr>
<td>warm</td>
<td>electron</td>
</tr>
<tr>
<td>left of</td>
<td>atom</td>
</tr>
<tr>
<td>touches</td>
<td>molecule</td>
</tr>
<tr>
<td>longer than</td>
<td>wave function</td>
</tr>
<tr>
<td>hard</td>
<td>charge</td>
</tr>
<tr>
<td>stick</td>
<td>cell nucleus</td>
</tr>
<tr>
<td>volume</td>
<td>mass</td>
</tr>
<tr>
<td>floats</td>
<td>electric resistance</td>
</tr>
<tr>
<td>wood</td>
<td>temperature</td>
</tr>
<tr>
<td>water</td>
<td>gene</td>
</tr>
<tr>
<td>iron</td>
<td>virus</td>
</tr>
<tr>
<td>weight</td>
<td>ego</td>
</tr>
</tbody>
</table>

Figure 4.2 Table Showing Paradigm Examples of Observational and Theoretical Terms (Suppe, 1974, p. 80)

Scientific theories were construed to be axiomatic calculi in which theoretical terms and statements could now only be given a partial observational interpretation by means of 'correspondence rules.'

Clusters of the modified doctrines which emerged are referred to variously as 'Logical Empiricisms', the 'Orthodoxy' and the 'Received View'. All of them however, continued to embrace the
three doctrines of truth by verification, inductive reasoning and "brute data" i.e. data which has not been subjected to interpretation, judgement etc. Since it is often difficult to delineate traditions or sub-traditions in the move away from Logical Positivism, particularly as regards the "quality" of truth endorsed (i.e. phenomenal or ontological), I shall use the title 'empiricist-verificationist' to refer to all traditions which subscribe to the three doctrines above, irrespective of the way truth is understood. I shall use the title 'orthodox account' to refer to all post-Logical Positivist traditions of empirical-verificationism.

Deductive reasoning was given full reign in what Feyerabend ([1962] 1981a, p. 91) has called the 'orthodox theory of reduction and explanation'.

Simply stated, the orthodox theory of scientific explanation, as propounded most influentially by Hempel and Oppenheim (1948), consists of the view that the facts to be explained (the 'explanandum') must be a logical consequence of the discipline which functions as the basis of explanation (the 'explanans'), i.e. the explanandum must be logically deducible from the information contained in the explanans. If not, then 'the explanans would not constitute adequate grounds for the explanation.' (Hempel and Oppenheim, 1948, p. 321).

On this orthodox, deductive, theory of explanation there are two principal ways by which growth in knowledge might be (said to have been) achieved. The first of these is by "confirmation erosion". This is where a theory is widely accepted to be highly confirmed but subsequent developments, such as technological advances which
substantially improve the accuracy of observations and measurements, undermine the degree of confirmation thereby allowing for a replacement theory. As an explanation for the growth of knowledge, confirmation erosion complements in many ways the Baconian notion of an "incursion of prejudice". The overthrow of Ptolemy's geocentric cosmology by the heliocentric one of Copernicus is a frequently claimed example of this. The second way, namely, "theory extension", is where a theory which, again, is widely held to be highly confirmed, comes to be "extended" from its original range of convenience to encompass a larger class of phenomena. An often proposed example of this is the development of the theory of mechanics which was originally held only to describe the motions of point masses but which was later extended to apply to the motions of rigid bodies.

Highly complementary to the orthodox theory of explanation is the view that to explain a theory is to show that it follows as a logical (deductive) consequence of (an)other theories. It is not unduly misleading to characterize this view as simply the orthodox theory of explanation "grown large".

The most influential proponent of this idea is Nagel (1949, 1953, 1961) who argues for "growth by incorporation" or, as he more formally terms it, growth by theory 'reduction'. He summarizes the character and purpose of his theory in the following way:

'The objective of the reduction is to show that the laws or general principles of the secondary science are simply logical consequences of the assumptions of the primary science (the reducing science). However, if these laws contain expressions that do not occur in the assumptions of the primary science, a logical derivation is the explicit formulation of suitable
relations between such expressions in the secondary science and the expressions occurring in the premises of the primary discipline.' (Nagel, 1953, p.541).

And elsewhere Nagel asserts that

'Reduction, in the sense in which the work is here employed, is the explanation of a theory or a set of experimental laws established in one area of enquiry, by a theory usually though not invariably formulated for some other domain. (Nagel, 1961, p. 338).

This is a view which Feyerabend argues' [ ] implies that the conditions for explanation and the conditions for reduction coincide for Nagel.' (Feyerabend, 1965, p. 169 n.102).

Paradigm examples of such growth in knowledge include the reduction of Galileo's terrestrial physics by Newtonian dynamics and the reduction of Newtonian mechanics by General Relativity Theory.

Returning now to the problems of instrumentalism and which were not fully resolved by orthodox accounts, a climate of intellectual opinion gradually emerged which sought an ontologically realistic but yet corrigible theory of knowledge.

Now, realist epistemology begins with the joint assumption of a reality to be known (the Assumptions of Realism or AR) and that a means of knowing it may be forthcoming (reality is comprensible: the Assumptions of Epistemological Realism or AER).
The classical constructivism of Kant certainly met these criteria but, one way or another, his articulation of the Constructivist Knowledge Thesis (CKT) had been judged to be dogmatic, i.e. incorrigible. Nor, for essentially the same reasons, could the doctrines of the so-called 'neo-Kantians', who had been influential at the turn of this century and who had been led by Cohen and his Marburg School, be seriously considered. This was because their notion of 'structures' or 'forms' of phenomena endorsed a 'Platonic sort of absoluteness' (Suppe, 1974, p.9) which virtually precluded acceptance of both relativity theory and quantum theory.

A solution attempted by Popper was to render constructivism corrigible by means of an injection of empiricism. The constructivist foundation of AER and CKT was now augmented by what one author has termed *standard empiricism* (Maxwell, 1984, p. 12 n. 2: original emphasis), a view which enjoins as a central tenet of epistemology *the principle of empiricism* which asserts that in science, only observation and experiment may decide upon the acceptance or rejection of scientific statements, including laws and theories.' (Popper, 1972, p.54; original emphasis: quoted by Maxwell). Epistemological realism, however, was no longer to be construed and pursued as an absolute, "global", affair a la Kant but, rather, was to be comprehended piecemeal. At the same time, such piecemeal comprehension could not be understood in terms of successive verification a la empirical verificationism due to the instability or revolutionary character displayed so palpably by developments in 20th century science. Popper, as we have seen responded by opting for falsifiability in his brand of objectivist-methodological-constructivism. By this manoeuvre he hoped to fuse ontological realism with corrigible objectivity of knowledge claims.
Principally under Popper's influence, knowledge came to be understood by philosophers of science to grow by successive approximation to reality: each later approximation closer than the last. Reality, then, was held to be comprehended (and only comprehensible) by piecemeal approximation.

Laudan has termed such a view 'convergent epistemological realism' (CER) and he characterizes it by elaborating five complementary theses, at least the first four of which I suggest are endorsed by Popper:

'R1) Scientific theories (at least in the 'mature' sciences) are typically approximately true and more recent theories are closer to the truth than older theories in the same domain;

R2) The observational and theoretical terms within the theories of a mature science genuinely refer (roughly, there are substances in the world that correspond to the ontologies presumed by our best theories);

R3) Successive theories in any mature science will be such that they 'preserve' the theoretical relations and the apparent referents of earlier theories (i.e. earlier theories will be 'limiting cases' of later theories).

R4) Acceptable new theories do and should explain why their predecessors were successful insofar as they were successful.

To these semantic, methodological and epistemic theses is conjoined an important meta-philosophical claim about how realism is to be evaluated and assessed. Specifically, it is maintained that:

R5) Theses (R1)-(R4) entail the ('mature') scientific theories should be successful; indeed theses constitute the best, if not the only, explanation for the success of science. The empirical success of science (in the sense of giving detailed explanations and accurate predictions) accordingly provides striking empirical confirmation for realism. 6

Now, Laudan's first thesis, R1, identifies something that shall later be of crucial importance in distinguishing objectivist-
methodological-constructivist traditions from post-empiricist ones,
viz. the idea of closer approximations to reality re-admits the idea
of ontological truth into epistemology after its long exile from
mainstream philosophy of science due to the prevalence of
instrumentalist and orthodox accounts (cf. Popper, 1972, p.231,
quoted in Section 3.4.2). As Laudan remarks:

'\[ \text{I take it that a realist would never want to} \]
\[ \text{say that a theory was approximately true if its} \]
\[ \text{central theoretical terms failed to refer.} \]\n\[ \text{(An instrumentalist, of course, could content the} \]
\[ \text{weaker claim that a theory was approximately true so} \]
\[ \text{long as its directly testable consequences were close} \]
\[ \text{to the observable values. But \{} \text{the realist must} \]
\[ \text{take claims about approximate truth to refer alike to} \]
\[ \text{the observable and the deep-structural dimensions of} \]
\[ \text{a theory [i.e. ontological commitments].} \]'\n

Notwithstanding his admission of an earlier diffidence, Popper is
enthusiastic in proclaiming that when Tarski's (1944) 'semantic'
conception of truth involving the notion of a 'meta-language' is
incorporated into his epistemology it re-establishes the 'intuitive
idea' that truth is 'correspondence to the facts':

'Tarski's greatest achievement, and the real
significance of his theory for the philosophy of the
empirical sciences, is that he rehabilitated the
correspondence theory of absolute or objective truth
which had become suspect. He vindicated the free use
of the intuitive idea of truth as correspondence to
the facts.\[ .\]. Thanks to Tarski's work, the idea of
objective or absolute truth - that is truth as
correspondence to the facts - appears to be accepted
today with confidence by all who understand it.'
I mention that whilst this last quotation demonstrates unequivocally Popper's commitment to a correspondence theory of truth, his appropriation of Tarski's theory has been severely, and in my opinion, fatally, criticized (for an especially clear and compelling example, see Haack, 1976).

Unaccepting of such criticisms, however, Popper (in common with all objectivists) identified truth to be the aim of science. In his intellectual autobiography, Popper comments pertinently on this issue when he articulates the differences which he perceives to exist between his brand of objectivist-methodological-constructivism (or, as he himself terms it, 'critical rationalism') and the 'traditional philosophy':

'Traditional philosophy linked the ideal of rationality with final, demonstrable knowledge (either proreligious or anti-religious: religion was the main issue) while I linked it with the growth of conjectural knowledge. This itself I linked with the idea of a better and better approximation to truth, or of increasing truthlikeness or verisimilitude. According to this view, finding theories which are better approximations to truth is what the scientist aims at; the aim of science is knowing more and more. This involves the growth of the content of our theories, the growth of our knowledge of the world.' (Popper, 1978, pp.149-150: original emphasis).

For Popper, scientists and critical rationalists are 'guided by the idea of truth as a regulative principle' (as Kant [!] or Pierce might have said); [ ].' (Popper, 1972, p.226: original emphasis). Knowledge, if unfalsified, is forever conjectural but we are always free to re-construe it and, assuming at least some empirical corroboration is forthcoming, thereby knowingly advance towards the
true. Popper summarises these ontologically realistic yet fallible and approximate qualities he sees as characteristic of scientific knowledge by means of a striking metaphor:

'The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.' (Popper, 1980, p. 111).

Now, two Personal Constructivists, Pope and Keen (1981, p. 29), have appropriated the widely used expression 'relativity of knowledge' to refer to epistemological stances which reject accumulative fragmentalism and the attainability of "final, demonstrable, knowledge" but which accept persons' perennial freedom to reconstrue knowledge claims. Pope and Keen argue that the epistemological aspect of PCP is commensurate with the relativity of knowledge and they cite a number of other philosophers and social scientists who similarly endorse this thesis thus understood.

Whilst I accept that this commitment is indeed shared by those authors whom Pope and Keen instance, their characterization of the 'relativity of knowledge' and their inclusion of Popper amongst those whom they cite in support of it should not be taken to imply further commonality of commitments concerning the quality and mode of evaluating constructs (theories, ideas).
To clarify, whilst Popper and objectivist-methodological-constructivists endorse the relativity of knowledge, they argue that we can and do have "sufficiently good reasons" for knowing that our later or preferred theories are more true as judged relative to our earlier or rival theories. This contrasts with Kelly and post-empiricists who, whilst also endorsing the relativity of knowledge, go on to argue that we can and do have "sufficiently good reasons" for knowing "only" that our later or preferred theories are more useful as judged relative to our earlier or rival theories. Convergent epistemological realism and correspondence theories of truth play no role whatsoever in these latter traditions and from their perspective these objectivistic commitments undermine or preclude a desirable and defensible treatment of meaning - an issue of central importance to educationalists. I shall devote the remainder of this chapter to elaborating upon, exemplifying and justifying these views.

Popper has taken considerable pains to delineate his approach from those of other epistemological traditions, especially empiricist-verificationist ones. Thus, he claims to be a realist but yet a fallibalist; he argues for falsifiability rather than verifiability as a demarcation criterion for scientific knowledge and he rejects the double-language model for theoretical and observational terms.

Popper has been widely interpreted to have been successful in demonstrating these differences. Widely, but not comprehensively: we have already seen that some critics have argued that Popper's notion of corroboration allows inductive reasoning to "creep in by the back door" (cf. Chapter 3, n. 25). I point out that Popper,
like empiricist-verificationists, resolutely excludes metaphysics from his demarcation criterion for scientific knowledge — notwithstanding his claim to have rehabilitated a role for it in science. Again, he would seem to endorse and perpetuate Reichenbach's distinction between a context of discovery and context of justification (not to mention Hume's Law, which may be argued to complement it in many respects). Most importantly, however, Popper's objectivism requires him (like all objectivist-methodological-constructivists) to treat explanation and issues of meaning in a manner which is virtually indistinguishable from that of orthodox accounts, indeed, Feyerabend([1962] 1981, p. 48) has argued that the orthodox theory of explanation 'may be regarded as an elaboration of suggestions that were first made, in a less definite form, by Popper' (cf. Popper, 1980, section 12).

I shall not attempt to present a detailed demonstration of Popper's orthodoxy concerning explanation and reduction; for my purposes, only brief reference to certain of his remarks and asides should be sufficient.

Thus, in his intellectual autobiography, Popper enthusiastically cites the formative influence that a remark made by Einstein in 1917 exerted upon the development of his epistemology:

'There could be no fairer destiny for any physical theory than that it should point the way to a more comprehensive theory, in which it lives on as a limiting case.'
(Einstein, quoted by Popper, 1978, p. 38).
Popper goes on to approve Havas (1964) as having provided a 'clear' demonstration that Newton's theory may, indeed, be regarded as a limiting case of Einstein's theory of gravitation (achieved by formulating the former in a 'general relativistic' or 'covariant' way, viz. 'by taking the velocity of light as infinite.' (Popper, 1978, p. 38 n. 32).

Popper's notion of empirical corroboration of theories, necessary to render useable his requirement of falsifiability of theories, explains his sympathy for theory reduction for a theory may be regarded as falsified if it is entailed by (is reduced by) another theory. Notwithstanding the fact that Popper stops short of a straightforward requirement of strict deductive relations between all our earlier or rival theories and all our later or preferred theories (as all orthodox theorists ultimately do), the spell that deductivism holds over him is, I think, particularly apparent in the following commentary which he provides on the nature and development of his ideas:

'I have discussed the question of the degrees of independence of tests in various places; it is an interesting problem, and it is connected with the problems of simplicity and depth. Since then I have also stressed the need to refer it or relativize it to the problem of explanation which we are engaged in solving, and to the problem situations under discussion, because all these ideas bear on the degrees of 'goodness' of the competing theories. Moreover, the degree of boldness of a theory also depends on its relation to predecessors. The main point of interest is, I think, that for very high degrees of boldness or non-adhocness I have been able to give an objective criterion. It is that the new theory, although it has to explain what the old theory explained, corrects the old theory, so that it actually contradicts the old theory: it contains the old theory, but only as an approximation. Thus I pointed out that Newton's theory contradicts both Kepler's and Galileo's theories - although it explains
For Popper, theory reduction might be said to be an "endorsed side effect" of his epistemological prescriptions: his main emphasis remains on falsifiability as both a demarcation criterion for scientific knowledge and as a requirement for its growth. He appeals to theory reduction only in his rational reconstruction of the history of the growth of scientific knowledge: it helps him strengthen his claims both to have provided an ontologically realistic method ('objective method') and that his method has actually been used. As Laudan (1981, p. 31 n. 9) has pointed out, however, Popper, unlike some of his disciples and intellectual descendents, is generally careful not to assert that actual historical theories exhibit ever increasing truth content (an exception which Laudan cites occurs in Popper, 1972, p. 220). And theory reduction, which involves two theories, plays no role in Popper's methodology per se. This is why I earlier referred to it as only an "endorsed side effect" of Popper's epistemology. Lakatos (1970, p. 129) is hence quite correct when he refers to 'deductive model[s]' of scientific explanation and change - amongst which he explicitly includes Popper's - as 'mono-theoretical' (my emphasis).

Popper's quest for, and belief in the attainability of, objective knowledge but which is forever conjectural and fallible leads him to construe all questions of meaning in essentialist terms. In his notorious table of 'ideas', Popper relegates such questions to a category of 'unimportant' ('my thesis is that the left side of this table is unimportant, as compared to the right side [ ]' Popper, 1979, p. 123: original emphasis):
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<td>MEANINGFUL</td>
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<tr>
<td>to that of</td>
<td>PRIMITIVE PROPOSITIONS</td>
</tr>
<tr>
<td>UNDEFINED CONCEPTS</td>
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the attempt to establish (rather than reduce) by these means their MEANING leads to an infinite regress

Figure 4.3 Table of 'Ideas' (after Popper, e.g. 1979, p. 124).

Popper originally proposed this table in a lecture he delivered in 1960 and which was entitled 'On the Sources of Knowledge and Ignorance'. The implication that Popper made then, as in his later uses of this table, was that questions of meaning were not only 'unimportant' but also a "source of ignorance". Popper argues that instead '[] what should concern us are theories; truth; argument.' (Popper, 1979, p. 123).

Notwithstanding the similarities I have drawn between Popper's epistemology (as prime representative of objectivist-methodological-constructivism) and orthodox "hypothetico-deductive" accounts, the extent of these similarities must remain controversial and somewhat obscure owing to Popper's extreme caution whenever the chips really are down. I suggest that what is ultimately clear, however, is that for all its multi-facetedness post-empiricist philosophy of science purports to provide an even-handed critique and alternative to them.
Now, my discussion of the post-empiricist critique and alternative shall be both selective and incomplete in many ways. This stems partly from its multi-facetedness as I have recently mentioned — post-empiricism is presently constituted of only a loose alliance of views and traditions — and partly from my desire to include and promote PCP amongst them.

The original and essential characteristics of the critical alternative of post-empiricism to orthodox accounts and to Popper and objectivist-methodological-constructivism may, however, usefully be summarised as a two pronged assault, each of which ultimately converge.

Thus, firstly, Popper's persistence with a deductive account of explanation, whereby '[ ]' meaning' in natural science presupposes an account of the empirical reference of terms and of their intensional connotations within a scientific theory' (Hesse, 1980, p. 171), renders him susceptible of formal criticisms and of the charge of instrumentalism — thereby undermining any claim to articulate an objectivist-methodological-constructivist epistemology. Post-empiricists, by contrast, embrace a "richer" meaning of 'meaning' (scientific explanation) since they do not divorce 'data' from their human (and possibly culture, tradition or even person specific) origin and purpose and thereby 'go beyond an external semantics of language' (Hesse, 1980, p. 171). For post-empiricists, questions of meaning are far from being 'unimportant' and amongst 'sources of ignorance': to the contrary, they are considered to be of pre-eminent importance and interest (the brand of post-empiricism which I endorse does, however, reject essentialism). This difference of approach is reflected in the
post-empiricists' treatment of the history of science, viz. they include 'external' or 'social' factors as well as 'internal' or 'rational' ones. I suggest that in at least some branches of post-empiricism, history of science (both internal and external) may also be said to be - 'constitutive' as opposed to merely 'illustrative' of philosophy of science.

Secondly, Popper's endorsement of CKT and of the perennial fallibility of knowledge claims has been used to undermine his demand for (empirical) falsifiability - 'all knowledge is theory-impregnated, including our observations' - and hence any claim to articulate an objectivist-methodological-constructivist epistemology. Post-empiricists, by contrast, extend the notion of rational theory-choice beyond the demand of 'standard empiricism', discussed earlier, to include also as relevant evidence compatibility of theories with the metaphysical assumptions and commitments informing them.


The idea that non-trivial theories in the history, and, in Feyerabend's account, even in the contemporary practice, of science might be incommensurable has variously inspired, baffled and dismayed philosophers of science and of social science. These widely differing reactions stem partly from ambiguities in the original writings of Kuhn and Feyerabend (compounded by many of their commentators) and partly from the nature of the thesis itself.
which was never intended by its authors to be "monolithic" and which, moreover, critically allows for an inevitable residue of "vagueness" in any generalisable account of theory-choice and change. These two factors - ambiguity in original presentation and critical endorsement of a degree of vagueness - have often been confounded by admirers and detractors of incommensurability alike.

I am particularly keen to demonstrate that the typical objectivist response to incommensurability, which claims that it represents an open invitation to extreme subjectivism, irrationalism and nihilism, is inappropriate and to incorporate this demonstration within a clear, positive, case for its potentialities.

In his seminal paper on incommensurability, Feyerabend summarizes two basic assumptions of the orthodox account which he rejects and transcends:

'The first assumption was that the explanandum is derivable from the explanans. The second assumption was that meanings are invariant with respect to the process of reduction and explanation.'

Now, I suggest that the critical alternative offered by the incommensurability thesis to these two assumptions of the orthodox account find their respective loci within each of the prongs of the post-empiricist assault (as I have earlier characterized it). The demise of the first assumption is achieved by demonstrating logical incompatibility between rival theories and may be said to follow as a trivial and less radical consequence of the rejection of the second assumption which is effected by demonstrating ontological
incommensurability between rival theories, i.e. incommensurability proper. The rejection of the first assumption gains its importance only by virtue of its temporal relation to the relative chronological impact of epistemological traditions, viz. orthodox accounts followed by Popper and objectivist-methodological-constructivism (be they variants of, or alternatives to, orthodoxy). Accordingly, I shall discuss the arguments against the first assumption in a somewhat abstract manner and leave contextual (i.e. historiographic) support until after my consideration of the arguments against the second assumption of orthodox accounts.

Thus, Feyerabend prefaces his arguments for incommensurability by making it clear that they apply only to 'general' or 'universal' or 'comprehensive' or 'non-instantial' theories such as 'the Aristotelian theory of motion, the impetus theory, Newton's celestial mechanics, Maxwell's electrodynamics, the theory of relativity, and the quantum theory.' (Feyerabend, [1962], 1981, p. 44) - in other words, examples of the sort with which orthodox theorists have been primarily concerned. For my present purposes, Feyerabend's meaning of 'non-instantial' theories may be considered to be synonymous with Hempel's meaning of 'explanans' and Kuhn's meaning of 'paradigm'. Since it is only with these sorts of theories that I shall be concerned I shall refer to them simply as 'theories'.

Feyerabend allows that the orthodox theory of explanation and reduction 'fairly adequately represents the relation between sentences of the 'All-ravens-are-black' type, which abound in the more pedestrian parts of the scientific enterprise'. (Feyerabend, [1962], 1981a, p. 44). This qualification to the applicability of
the incommensurability thesis does not constitute any kind of concession to the orthodox theory and follows from the different way in which empirical generalizations are tested in comparison with theories, as Feyerabend explains:

'In what follows, the usual distinction will be drawn between empirical generalizations, on the one side, and theories, on the other. Empirical generalizations are statements, such as "All A's are B's" (the A's and B's are not necessarily observational entities), which are tested by inspection of instances (the A's). Universal theories, such as Newton's theory of gravitation, are not tested in this manner. Roughly speaking their test consists of two steps: (1) derivation, with the help of suitable boundary conditions, of empirical generalizations and (2) tests, in the manner indicated above, of these generalizations. One should not be misled by the fact that universal theories too can be (and usually are) put in the form 'All A's are B's'; for whereas in the case of generalizations this form reflects the test procedures in a very direct way, such an immediate relation between the form and the test procedure does not obtain in the case of theories. Many thinkers have been seduced by the similarity of form into thinking that the test procedures will be the same in both cases.' (Feyerabend, [1962], 1981a, p. 44 n. 1: original emphasis).

Logical incompatibility of theories which are "in the same domain" or, as Feyerabend puts it, situation where rival theories, 'deductively disjoint' (Feyerabend, 1978, p. 67), use concepts which cannot be brought into the usual logical relations of inclusion, exclusion and overlap.

Now, it has long been accepted by orthodox theorists that rarely in fact does a strict deductive relation obtain between scientific explanans and explanation (and between earlier or rival theories and later or preferred theories) but only a relation of "approximate fit". And, further, that what counts as sufficiently approximate
fit cannot be decided deductively but involves a 'complicated function of coherence with the rest of a theoretical system, general empirical acceptability throughout the domain of the explanandum, and many other factors' (Hesse, 1980, p. 121). Putnam (1965) actually suggested that Nagel's theory of reduction could be protected against Feyerabend's charge of logical incompatibility by specifying that it is only a suitable approximation of the earlier theory that is deducible from the later one.

This orthodox manoeuvre, however, omits or glosses an explanation (and between earlier or rival theories and later or preferred theories) but only relations of approximate fit, indeed, Putnam (1965) suggested that if it was actually specified that it is only a suitable approximation of the earlier theory that is deducible from the later one, then Nagel's theory of reduction could be protected against Feyerabend's criticism.

Feyerabend ([1965] 1981a) was quick to respond, however, that this orthodox manoeuvre effectively undermined their claim to provide an account of actual cases of theory replacement since the transformation of the earlier theory into a suitable approximation of the later theory requires that "grievous ontological violence" be committed against the former. The epistemological account which emerges thereafter, however and moreover, disguises the element of (often passionate) disagreement over rival theories which Feyerabend sees as essential to the development of scientific knowledge.

Now, Feyerabend would not deny the practical utility of sometimes construing an earlier theory as an approximation of a later theory and then deriving deductive relations between them. The earlier
theory might, for example, offer easier means of calculation which are accurate within the limits required (Newtonian rather than relativistic mechanics was sufficient to send rockets to the moon). But the epistemological force of the orthodox account is lost in the process of transforming the earlier theory in order to achieve an approximate fit with the later theory for as Hesse points out:

'[ ] what counts as sufficient approximate fit cannot be decided deductively, but is a complicated function of coherence with the rest of a theoretical system, general empirical acceptability throughout the domain of the explanandum, and many other factors.'
(Hesse, 1980, p. 121: my emphasis).

The reason why logical incompatibility is less radical than ontological incompatibility is because the former amounts only to a reduction ad absurdum of the orthodox account: for the sake of argument, it grants that there is a common framework - a logical framework - within which two theories may be demonstrated to be incompatible. This crucial assumption, however, is precisely what ontological incompatibility, i.e. incommensurability proper, denies as I shall now try to show.

In articulating the incommensurability thesis, Kuhn and Feyerabend would seem to have drawn partial inspiration and support from the ideas of Duhem, Quine and Hanson and so to that extent their thesis is compatible and complementary to the earlier work of these authors, specifically, Quine’s stress on the ‘undetermination’ of theory by data and the ‘indeterminacy of translation’ between rival theories. Quine’s epistemology has, in turn, some intellectual antecedents in the historical researches of Duhem. Their interaction has led to the so-called ‘Duhem-Quine Thesis’ which,
briefly stated, 'holds that any theory can be permanently saved from refutation by some suitable adjustment to the background knowledge in which it is embedded' (Lakatos, 1970, p. 184). Hanson's influence stems from his emphasis on the perceptual discontinuities which he argues are a consequence of theory-change.

Feyerabend's development of the incommensurability thesis may be said to have begun with criticisms of Carnap's double-language model based upon his commitment to CKT.

Thus, for example, in an early paper he argues for the thesis that

'[the interpretation of an observation-language is determined by the theories which we use to explain what we observe, and it changes as soon as those theories change.]

One of the consequences of this thesis is that the observation term - theoretical term distinction is context dependent. This also implies, however, that a neutral, tradition independent, language or rationale for appraising rival theories is not possible. As Feyerabend elsewhere puts it:

'Science has always been a matter of context-dependent plausibility and not of a context-independent 'organon of thought'.
(Feyerabend, 1981b, p. 18).

Feyerabend states clearly the difference he sees between the orthodox account of theory change and his own:
'What happens here when a transition is made from a theory $T'$ to a wider theory $T$ (which, we shall assume, is capable of covering all the phenomena that have been covered by $T'$) is something much more radical than incorporation of the unchanged theory $T'$ (unchanged, that is, with respect to the meanings of its main descriptive terms as well as to the meanings of the terms of its observation language) into the context of $T$. What does happen is, rather, a replacement of the ontology (and perhaps even of the formalism) of $T'$ by the ontology (and the formalism) of $T$, and a corresponding change of the meanings of the descriptive elements of the formalism of $T'$ (provided these elements and this formalism are still used). This replacement affects not only the theoretical terms of $T'$ but also at least some of the observational terms which occurred in its test statements. That is, not only will description of things and processes in the domain in which $T'$ has been applied be infiltrated, either with the formalism and the terms of $T$, or if the terms of $T'$ are still in use, with the meanings of the terms of $T$, but the sentences expressing what is accessible to direct observation inside this domain will now mean something different. In short, introducing a new theory involves changes of outlook both with respect to the observable and with respect to the unobservable features of the world, and corresponding changes in the meanings of even the most 'fundamental' terms of the language employed.' (Feyerabend [1962] 1981a, pp. 44-45: original emphasis).

Hence Feyerabend argues that one may tentatively identify a theory as incommensurable with another theory 'if its ontological consequences are incompatible with the ontological consequences of the latter' (Feyerabend, 1981a, p. xi), mere difference of concepts does not suffice: the conditions of concept formation in one theory must be shown actually to forbid the formation of the basic concepts of the other theory (Feyerabend, 1978, pp. 67-68 n. 118).

Feyerabend's notion of incommensurability, as so far I have excerpted it from his writings, does noM differ in any important way from Kuhn's. Their fully elaborated accounts, however, display many
differences of emphasis and of detail and, for reasons that I shall make clear in section 4.4, below, these are responsible for my preference for Feyerabend's approach. One such difference that may be timely to mention is that without compromising his critique of the orthodox theory Feyerabend places less emphasis upon uni-directionality (i.e. earlier to later theories) and totality of incommensurable relations:

'Of course, theories may be interpreted in different ways, they may be incommensurable in some interpretations, not incommensurable in others. Still, there are pairs of theories which in their customary interpretation turn out to be incommensurable in the [ontological] sense at issue here. Examples are classical physics and quantum theory; general relativity and classical mechanics; Homeric Aggregate physics and the substance physics of the Presocratics.'

Intrinsic to his ideas on incommensurability, Feyerabend (1965, p. 180) embraces what might be called a 'theoretical-context account of meaning' whereby the meaning of every term we use depends upon the theoretical context in which it occurs and cannot be understood in isolation from it. As a consequence of this, where there are 'two contexts with basic principles that either contradict each other or lead to inconsistent consequences in certain domains, it is to be expected that some terms of the first context will not occur in the second with exactly the same meanings' (Feyerabend, 1965, p. 180: my emphasis).

This also implies that there can be no 'crucial experiments', at least, none as understood in any objective sense (Feyerabend, 1965, p. 214). This is not because the experimental device for such an
experiment would be prohibitively expensive or complicated; rather, it would be impossible in principle: the absence of overlap of experiences between rival theories means that there could be no universally accepted statement (or, more generally, language) capable of expressing whatever emerges from the observation.

For exactly these reasons, the incommensurability thesis shows that certain forms of epistemological realism are too narrow and in conflict with the actual conduct of science (see e.g. Feyerabend, 1978, pp. 69-70). Amongst influential views "rejected" are the ideas that realism must be interpreted as a particular theory or that realism is a necessary presupposition of science (and of knowledge in general):

'If theories are commensurable, then no problem arises - we simply have an addition to knowledge. It is different with incommensurable theories. For we certainly cannot assume that two incommensurable theories deal with one and the same objective state of affairs (to make the assumption we would have to assume that both at least refer to the same objective situation. But how can we assert that 'they both' refer to the same situation when 'they both' never make sense together? Besides, statements about what does and what does not refer can be checked only if the things referred to are described properly, but then our problem arises again with renewed force).' (Feyerabend, 1978, p. 70: original emphasis).

Now, Feyerabend does not abandon or deny a role for empirical evidence in the methodology of science; rather, he reconstrues its status - downwards - so that it can no longer be held to be the sole, universal, arbiter in matters of theory-choice and in criteria for the growth of knowledge. Here he abandons and rejects only the claim of standard empiricism. Nor can it straightforwardly be assumed that the incommensurability thesis denies AER (as
Feyerbend's recent comment might seem to imply as I shall try to argue in section 4.4, below (my 'transformational theory of reference').

For Feyerabend, then, theories cannot be understood or tested independently from the metaphysical and other extra-empirical commitments of the tradition or school of thought in which they occur. Feyerabend construes these latter as instances of 'ideology' and in its broad social context he takes the whole of science itself also to be just one tradition or ideology which is distinguishable but not separable from the others (see, especially, Feyerabend, 1975a). The fruits of Feyerabend's "anarchistic interpretation" of objectivist epistemology might be described as underwriting 'radical relativity of knowledge' for even the claim of CER is abandoned:

'Knowledge so conceived is not a series of self-consistent theories that converges towards an ideal view; it is not a gradual approach to the truth. It is rather an ever increasing ocean of mutually incompatible (and perhaps even incommensurable) alternatives, each single theory, each fairy tale, each myth that is part of the collection forcing the others into greater articulation and all of them contributing, via this process of competition, to the development of our consciousness. Nothing is ever settled, no view can ever be omitted from a comprehensive account.' (Feyerabend, 1975b, p. 30: original emphasis).

From the perspective Feyerabend provides, the orthodox account (and all its objectivist variants) perpetrates

'[
] an interesting epistemological illusion: the imagined content of the earlier theories (which is the intersection of the remembered consequences of these theories with the newly recognized domain of problems and facts) shrinks and may decrease to such an extent that it becomes smaller than the imagined content of the new ideologies (which are the actual
consequences of these ideologies plus all those 'facts', laws, principles which are tied to them by ad hoc hypotheses, ad hoc approximations or by the say-so of some influential physicist or philosopher of science - and which properly belong to the predecessor). Comparing the old and the new it thus appears that the relation of empirical contents is like this

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or, perhaps, like this

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while in actual fact it is much more like this

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| domain D |
| representing the problems and facts of the old theory which are still remembered and which have been distorted so as to fit into the new framework. It is this illusion which is responsible for the persistent survival of the demand for increased content. |
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(Feyerabend, 1975b, pp. 177-178: original emphasis).
Amongst many examples which Feyerabend uses to demonstrate incommensurability are the alleged overthrow by 'reduction' of Galilean terrestrial physics to Newtonian dynamics and of Newtonian mechanics to General Relativity Theory. These I shall now briefly discuss.

Thus, Feyerabend ([1962] 1981a) argues that a basic assumption underlying Galileo's law concerning the motion of material objects (e.g. falling stones, penduli, balls on an inclined plane) in free fall near the surface of the earth is that their vertical acceleration over any finite (vertical) interval is constant. This contrasts with Newton's theory where vertical acceleration in free fall of such bodies are inversely proportional to the distance of the earth due to the mutual attractive force exerted by gravity. If the ratio distance of fall were equal to zero, then Galileo's law could, indeed, be derived from Newtonian laws, but in cases of free fall this is never the case. The two laws are thus logically disjunct and so, strictly speaking, the Galilean relation cannot be derived from Newton's law, notwithstanding their experimental indistinguishability under ordinary standards of measurement.

In the case of the supposed reduction in Newtonian mechanics to General Relativity Theory, Feyerabend (e.g. 1965) likewise does not deny that under certain limiting conditions the equations of the latter yield values similar to those of the former but he challenges the orthodox assumption that the meanings of shared, fundamental, terms or concepts remain invariant when this is done. 'Meaning invariance' of such terms is necessary for the orthodox requirement of 'logical connectability' to be fulfilled. Feyerabend's critical
comparison of the meanings of classical and relativistic 'length' and 'mass', however, show that in this particular case the requirement of connectability cannot be met: they are incommensurable notions.

To elaborate upon the example of 'mass', in classical mechanics the mass of a particle is assumed to be constant and conserved in all reactions in a closed system. In relativity theory, by contrast, the mass of a particle is proportional to its velocity relative to a co-ordinate system (frame of reference) in which the observations are made. Accordingly, each theory contains a different set of equations about mass. Feyerabend argues that these sets of equations are forever incompatible because they reflect — and may only be understood in terms of — mutually exclusive ontologies.

Whilst 'mass' in the classical theory is an \textit{attribute of the object itself} and is unaffected by its behaviour in co-ordinate systems, 'mass' in relativity theory is a \textit{relation involving relative velocities between an object and a co-ordinate system}. Although various manoeuvres may effect a numerical or quantitative reconciliation between the two theories (e.g. identifying the classical mass with the relativistic rest mass), any claim for an objective account of the growth of knowledge is undermined because the two "masses" cannot be represented by the same \textit{concept}.

Now, objectivist critics of the incommensurability thesis often fail to appreciate Kuhn's and Feyerabend's \textit{purpose} for introducing the notion (which is not to suggest that compelling \textit{arguments} against the thesis might not be forthcoming). Thus, Papineau (1979, p. 14) comments that a number of critics have 'pointed out' that Kuhn's and Feyerabend's views make an 'extreme relativism quite
inescapable' and this they have 'understandably' found 'impossible to stomach'. On the other hand, he (they) concedes that decisive, generally accepted, arguments against Kuhn and Feyerabend have yet to be advanced. Papineau offers the following characterization of the critical state of play that has resulted:

'The situation may be characterized as the 'paradox of meaning variance'. There do seem to be good arguments for moving away from a traditional empiricist conception of meaning towards a theoretical context account. But then it follows that with every change of theory there will be a change of meaning (the 'meaning variance thesis'). And with this there seems no way of escaping the unpalatable consequences that objective choices between scientific theories are impossible'.

(Papineau, 1979, p. 41).

The first point that I would like to make here is that only an objectivist would construe this situation as a paradox! The possibility of objective choices between theories is precisely what Kuhn and Feyerabend reject but, far from seeing this as heralding 'unpalatable consequences', Feyerabend, at least, has emphasized that this offers new possibilities on an unprecedented scale for a socially pertinent and responsible conduct of science. (I shall consider this latter claim in section 4.4., below).

Many critics still persist, however, in asserting that Kuhn and Feyerabend argue for irrationality, extreme relativism etc, in science but this is just not so. In response to such inappropriate construals of their intentions, Kuhn has responded thus:

'[ ] to describe [Feyerbend's] argument as a defense of irrationality in science seems to me not only absurd but vaguely obscene. I would describe it, together with my own, as an attempt to show that existing theories of rationality are not quite right
and that we must readjust or change them to explain why science works as it does.' (Kuhn, 1970b, p. 264).

Much of the confusion concerning Kuhn's and Feyerabend's intentions or purpose for their thesis would seem to derive from the still widely held myth that they argue that incommensurable theories are radically incomparable. Neither Kuhn nor Feyerabend, however, have ever argued that incommensurable theories are comparable except in a fully comprehensive or objective sense. What they reject as an illusory and unworthy goal, then, is objectivism.

It is easy to see where the myth of incomparability originated. The stress which Kuhn and Feyerabend each place upon the theory-ladenness of all observations (CKT) and, thence, upon the incommensurability of certain theories, has led to an over identification of their views with those of Hanson (e.g. 1958) who argued that theory-change necessarily resulted in perceptual changes also - thereby rendering rival theories radically incomparable.

Now, although Kuhn has claimed in a series of notorious passages that scientific revolutions result in a 'shift of vision' (Kuhn, [1962] 1970a, p. 119) whereby scientists thereafter 'work in a different world' (Kuhn, [1962] 1970a, p. 135), close reading even of his original text shows that he exercised considerable circumspection over any claim for perceptual discontinuities in the development of scientific knowledge. Thus, he heavily qualifies his endorsement of Hanson's appropriation of notions from certain experiments in perception psychology (Kuhn, [1962] 1970a, pp. 113-114). Kuhn argues that schools guided by different paradigms are always 'slightly at cross-purposes' (Kuhn, [1962] 1970a, p. 112:  

- 4.42 -
my emphasis) and although he is critical of the double-language model which preserves in all essentials the traditional empiricist notion that theories are no more that person-made interpretations of 'given data', he claims that

"In the absence of a developed alternative, I find it impossible to relinquish entirely that viewpoint."

Kuhn is apparently modest enough not to consider his own doctrines to constitute such an alternative.

Feyerabend's stance on this issue is somewhat easier to discern than Kuhn's. Thus, Feyerabend (1978, pp. 67-68 n. 118) straightforwardly admits that he believed for 'some time' that conceptual differences would always be accompanied by perceptual differences but, in his 'Reply to Criticism' (Feyerabend, [1965] 1981a), he abandoned his commitment to the claim of universality of this consequence due to its incompatibility with the 'results of psychological research'. To the charge that he inferred (comprehensive) incomparability of incommensurable theories, Feyerabend has responded

'Quite the contrary, I tried to find means of comparing such theories. Comparison by content, or verisimilitude was of course out. But there certainly remained other methods. There are formal criteria: a linear theory is preferable to a non-linear one because solutions can be obtained more easily. This was one of the main arguments against the non-linear electrodynamics of Mie, Born and Infeld. The argument was also used against the general theory of relativity until the development of high speed computers simplified numerical calculations. Or a 'coherent' theory is preferable to a non-coherent one (this was one of Einstein's reasons for preferring general relativity to other accounts). A theory using many and daring approximations to reach 'its facts' may be less likeable than a theory that uses only few, and safe
approximations. Number of facts predicted may be another criterion. Nonformal criteria usually demand conformity with basic theory (relativistic invariance; agreement with basic quantum laws) or with metaphysical principles (such as Einstein's 'principle of reality').

(Feyerabend, 1978, p. 68: original emphasis - I have combined Feyerabend's text with his footnote, n. 119).

Thus, it is only comparison of rival theories via translation ("commensuration") that Feyerabend denies with the incommensurability thesis.

For Kuhn and Feyerabend, the nature of what is actually involved when we do compare rival theories is the reason why they introduce the notion of incommensurability at all. That incommensurable theories are comparable is also clear from the method of presentation which Kuhn and Feyerabend use to demonstrate instances of the relation as Bernstein's excellent commentary on Feyerabend's discussion of the incommensurability of archaic Greek art with traditional Western art elucidates:

'In [Feyerabend's] example of the Greek archaic style, he does not, as some critics have claimed, tell us that because this style (and the world view that it embodies) it incommensurable with later styles we must dumbly contemplate it. We are not confronted with forms of life that are so self-contained that we cannot compare them. If this were really the case, the appropriate response would be silence. On the contrary, he attempts to understand what is distinctive about this style - and the procedure for bringing out what is distinctive depends on a skillful use of comparison and contrast. The basic presupposition here is that we can understand what is distinctive about this incommensurable style and form of life - and we do not do this by jumping out of our own skins (and language) and transforming ourselves, by some sort of mystical intuition or empathy, into archaic Greeks. Rather, the analysis proceeds by a careful attention to detail - to the various "building blocks" - working back and forth in order
to appreciate and highlight similarities with and differences from other styles and forms of life.' (Bernstein, 1980, p. 90: original emphasis).

Feyerabend (though not Kuhn) additionally argues that the generation of inconsistent and incommensurable theories constitutes an important part of scientific method (see discussion in section 4.3, below).

From my foregoing discussion I hope that the reader has already experienced many "resonances" between the incommensurability thesis and the formal content of PCP. In the remainder of this part of the chapter, however, I shall try to make some of these explicit and to explore their interaction. I shall here concentrate upon drawing similarities of "constructive structure" ("products") and leave the drawing of similarities of "constructive dynamics" ("process") until sections 4.3 and 4.4. Specifically, I shall argue that the relation of opposite contrast, which is intrinsic to the notion of the construct, may usefully, and non-preemptively develop by construing it as a relation of incommensurability. I shall begin, however, by contrasting certain formal, epistemological, aspects of PCP from those of orthodox accounts.

Kelly's 11 Corollaries to his Fundamental Postulate may be said to formalise both the constructive structure and the formative dynamics of personal constructs (and, more broadly, of construction systems). Prior to presenting them, however, Kelly cautions us that although he has termed them 'corollaries', 'logically, they involve somewhat more than what is minimally implied by the exact wording of the [fundamental] postulate.' (Kelly, 1955, p. 50). And earlier, in a complementary manner, he advises us that
'The [fundamental] postulate we formulate will not necessarily provide a statement from which everyone will make the same deductions. The system built upon the postulate will therefore not be completely logic-tight. Rather, we shall strive to make our theoretical position provocative, and hence fertile, rather than legalistic.' (Kelly, 1955, p. 46).

Thus, right from the start we see a departure from the aim demanded by orthodox theorists for formal-deductive relations to obtain between components of construction systems. One of my principal tasks ahead shall be to demonstrate how Kelly manages to avoid formal-deductive relations in construction systems (and, by so doing, also departs from the orthodox theory of explanation and reduction), but yet to allow for 'super-ordination' between constructs and elements. (Kelly's notion of super-ordination often strikes the unwary as being formal-deductive by another name).

For Kelly, elements cannot be derived from constructs and consequently the meaning (or even a meaning) of a construct cannot depend upon this being done. A personal construct implies a meaning which is, at least in part, also personal (unique). This, in turn, shall depend upon the personal purposes for which that construct was created and this may only be gleaned by reference to both the dimension of opposite contrast which it embodies and the elements which are held by the construer to come within its purview.

In PCP, then, a meaningful explanation, whether "scientific" or otherwise, implies contrast, and dimensionality provides grounds for relevance (cf. my discussion in section 3.4). With my intention to explicate similarities between the construct and the incommensurability thesis in mind, the following summary of meaning in PCP by Kelly would seem to be particularly appropriate:
'Whatever one says about any event gathers its meaning from what contrasting things could otherwise have been said about it, as much as from the other events of which the same might have been said or those occurrences of which a contrasting statement might have been made. Since every perception we have, as well as every statement we make, is no less a denial than it is an assertion, it becomes important to note what is perceptually negated as well as what is verbally affirmed.'

(Kelly, 1969, p. 11: my emphasis).

Now, as we have seen, the incommensurability thesis high-lights intellectual disagreement and conflict in science; it is concerned only with rival theories. As we have also seen, however rival or incommensurable, theories are always, in some sense, alternative theories. Accordingly, I shall pursue my demonstration of the pertinence and applicability of the incommensurability thesis to PCP by considering the notion of an "alternative construct".

Here it might be useful first to distinguish a "fundamentally different" construct from a "genuinely alternative" construct.

A fundamentally different construct is one which is created for a completely different purpose than another construct. At the methodological level, we can recognize such a construct by virtue of it abstracting an entirely different set of elements, i.e. there is no overlap of their respective ranges of convenience.

A genuinely alternative construct, by contrast, may be said to be such by virtue of its abstracting the same set of elements but in a different way. At the methodological level, this means that a
genuinely alternative construct must be shown to exhibit at least one difference, viz. different ordering of the abstracted elements, and one similarity, viz. overlap between the classes of elements abstracted by the constructs within the respective ranges of convenience. With respect to this similarity, however, I think it reasonable to be more specific and suppose that there must be overlap of the 'contexts' of the two constructs and, further, that it is here that the locus of choice – the judgement of relative personal utility – resides, notwithstanding that the two constructs' respective ranges of convenience (and, hence, utilities) may differ elsewhere.

A genuinely alternative construct may also exhibit one further difference, namely, different pole or element names(s). It is important to note, however, that a construct which displayed only this difference (i.e. whilst maintaining the same ordering of elements) would not be a genuinely alternative construct; it would be only a "trivially different" (or "trivially alternative") construct. Such a difference might be described as "merely semantic" (one of the very few legitimate uses of the phrase). As Kelly cautioned: 'Construing is not to be confounded with verbal formulation.' (Kelly, 1955, p. 51).

Now, it is also useful to point out that our experienced commitment to constructs which have proven useful to us is normally only to one pole – the 'emergent' pole; indeed, this would seem to be so except in situations of psychological distress or disturbance, e.g. as part of the process of re-adjusting to (coping with) stress "caused" by problems in interpersonal relationships or an ongoing "scientific revolution" (the two may coincide). Contrast poles, then, may,
and usually do, remain implicit: certainly this is the way in which knowledge claims have traditionally been linguistically formulated and expressed (the subject-predicate mode of speech exerts a virtual mono-pole-y!). There may often be advantages to be had from such a practice - perhaps most obviously, that of "linguistic economy" - provided that it is recognized for what it is, i.e. mode of linguistic formulation and expression. But in PCP, meaningful criticisms and learning of existing knowledge claims may only be said to occur to the extent that dimensionality and opposite contrast are made explicit. Our psychological processes of creating meaningful new constructs are similarly understood to be enhanced by explicitly creating opposite contrasts. (cf. my discussion in section 4.3, below).

An interesting (and from an objectivist point of view, paradoxical) case of sociality of construction occurs in the most alternative of all genuinely alternative constructs, i.e. where a construct is common to two persons in all respects except that the poles are reversed. This shows the highly complementary and social nature that "diametrically opposed" views may have. Dimensionality in knowledge claims is thus not necessarily a recipe for personal isolation and alienation.
Having recently introduced the notion of choice, this is as far as I can go in my consideration of the 'alternative construct' without invoking certain of Kelly's corollaries to his Fundamental Postulate which have to do with this since any particular personal construct is fundamentally as much a thinking process as it is an epistemic product or claim.

Three corollaries in particular emphasize the qualities of choice and change in a construction system yet remain compatible with the 'Organization Corollary' (discussed in section 3.4), namely,

(1) 'Choice Corollary: A person chooses for himself that alternative in a dichotomized construct through which he anticipates the greater possibility for extension and definition of his system.' (Kelly, 1955, p. 64: my emphasis - capital letters used throughout original).

(2) 'Modulation Corollary: The variation in a person's construction system is limited by the permeability of the constructs within whose ranges of convenience the variants lie.' (Kelly, 1955, p. 77: my emphasis - capital letters used throughout original).

(3) 'Fragmentation Corollary: A person may successively employ a variety of construction subsystems which are inferentially incompatible with each other.' (Kelly, 1955, p. 83: my emphasis - capital letters used throughout original).

Now, I interpret the Modulation and Fragmentation corollaries to be a more-or-less deliberate attempt by Kelly to suggest, on the one hand, part of a scheme for "rationally integrating" the contents
and, thence, of construing the overall form of construction systems,
but yet, on the other hand, to undermine any attempt to construe the
Choice Corollary in terms of a universally applicable
epistemological demand for 'excess empirical corroboration' as
formally mediated and appraised by deductive relations between
explananda and explanation.

I shall elaborate upon this interpretation through a very brief
consideration of Hinkle's (1965) 'laddering technique'. Hinklean
'laddering' is a technique for moving between levels within a
construction system, e.g. between 'peripheral' constructs and 'core'
constructs. To help a person to "ladder up" from a particular
construct, the researcher asks them which pole of that construct
they prefer and why. In answering such questions, the person
usually offers a higher level superordinate construct. Conversely,
one may "ladder down" by asking what or how questions, e.g. "what is
it that you perceive in both elements X and Y and which makes them
different from element Z?".

Now, when "systematic" epistemologists (e.g. orthodox theorists,
Popperians, Piagetians) attempt to interpret Kelly's ideas in their
own terms they soon face a dilemma for, if one "ladders up" within a
construction system, then these would seem to be two possible
outcomes to the process, viz. either an overarching superordinate
construct or two 'inferentially incompatible' (i.e. fragmented)
construction sub-systems shall emerge. This situation may
semi-humorously be described as 'The "Systematic" Epistemologist's
Dilemma of the "Tight Tops" vs "Stocking Tops" Outcome to Construct
Laddering' and represented by the following diagram:
For the Constructive Alternativist, this possibility of two outcomes to construct laddering presents no problem or dilemma: both are possible, albeit, not simultaneously, and which shall depend upon the perceived purpose of laddering. Just as theories may be interpreted in different ways, emerging as incommensurable in some interpretations and not incommensurable in others, so too may constructs (contrast poles may be iterative). In PCP, whilst the Choice Corollary assumes that a person shall make the 'elaborative choice' (Kelly, 1955, p. 65: original emphasis) this cannot be interpreted as a universal test criterion or demand for "excess empirical content" for this would prescribe also personal purpose and salience. Of course, the range or context of a particular construct, $C_1$, may be construed to entail that of another construct, $C_2$, and if, and only if, "comprehensivity" (i.e. "excess content") is pre-eminent amongst the construer's test-criteria for each of
these constructs, then this situation (entailment) would constitute sufficient good reason for preferring C1 to C2. PCP thus rejects a universal "arrow of development" in cognition or knowledge claims.

Turning now to the Modulation Corollary, its most important aspect may be said to lie in Kelly's notion of 'permeability' and which he defines (in part) thus:

'A construct is permeable if it will admit to its range of convenience new elements which are not yet construed within its framework.' (Kelly, 1955, p. 79).

Kelly makes it clear that his notion of permeability has increasing utility according to the degree to which constructs are superordinate.

Kelly tells us that he considers his Fragmentation Corollary to be, in part, a 'derivative' of his Modulation Corollary (Kelly, 1955, p. 87) and that it follows 'as an explicit statement of the kind of inconsistency which the Modulation Corollary implicitly tolerates' (Kelly, 1955, pp. 87-88), viz. successive employment of construction sub-systems which are 'inferentially incompatible' with each other. This possibility has ultimately to be allowed due to the notion of inconsistency - opposite contrast - which is at the heart of his notion of the personal construct. Kelly thus signals unequivocally his departure from the orthodox account which demands that for every 'theoretical statement' there must be 'correspondence rules' which 'will inferentially connect it with certain observational statements' (Papineau, 1979, p. 11). In PCP, whilst we can be sure (and only sure) that changes that take place from old to new
constructs do so within a 'larger system' (Kelly, 1955, p. 83), those constructs which we construe to have had an impact on later (i.e. larger) constructs and, hence, to be 'legitimate precursor [s]' (ibid) of the larger construct, may only be said to share a 'collateral [relationship], [ ], rather than a lineal one' (ibid: my emphasis). Kelly summarizes the overall import of these views thus:

'[ ] while a person's bets on the turn of minor events may not appear to add up, his wagers on the outcome of life do tend to add up. He may not win each time, but his wagers, in the larger contexts, do not altogether cancel themselves out. The superordinate permeable features of his system may not be verbalized, they may be more "vegetative" than "spiritual", or they may be seen as what Adler would have called a "style of life"; but they are part of a system and, therefore, may be considered from the viewpoint of their lawful as well as from the viewpoint of their free aspects.'
(Kelly, 1955, p. 88: original emphasis).

Now, although PCP qua meta-theory may be seen as a principled rejection of functional invariants in learning or method, at any moment in time Kelly would seem to endorse some "Postulate of Constructive Connectedness" which complements his 'Postulate of Cosmic Connectedness' (discussed in section 4.4, below), viz. within a particular application of a construct, predicated upon a particular purpose for that construct, we are entitled to infer some kind of "constructive causality" - but only that once. I suggest that this, together with his requirement for a dimension of opposite contrast in all knowledge claims, represents Kelly's commitment to a theoretical context account of meaning.
If a time frame is imposed, however, then Kelly's Modulation and Fragmentation Corollaries undermine any implication of 'logical determinism' or any demand for 'formal consistency' within his theory. Accordingly, Personal Construct theorists talk of 'construct heterarchies' (Glanville, 1982) and the like. Similarly, if my suggestion that Kelly's notion of 'collateral' relations obtaining between otherwise 'inferentially incompatible' construction sub-systems may itself be construed as incommensurability relations is accepted, then the overall conception of knowledge that emerges from Kelly's account would seem to be "isomorphous" with that of Feyerabend who talks of knowledge as 'an ever increasing ocean of mutually incompatible (and perhaps even incommensurable) alternatives' (Feyerabend, 1975b, p. 30: original emphasis - quoted more fully earlier).

In PCP, dimensionality in knowledge claims may be understood to be a fundamental orienting process by which object relations (including other minds) are imposed. Where something resembling formal deductive relations between rival theories can be derived, only ad interim commonality can be inferred. Where Einstein said

'There can be no fairer destiny for any physical theory than that it should point the way to a more comprehensive theory, in which it lives on as a limiting case.'
(Einstein, speaking in 1917, quoted by, e.g., Popper, 1978, p. 38)\textsuperscript{15}

- a remark seized upon by orthodox theorists and objectivist-methological constructivists as a mandate for hypothetico-deductivism and CER, I can now propose a "Kellyian" alternative:
There can be no fairer destiny for any personal construct than that it should make an impact upon a larger construction system, in which it lives on as a relevant contrast.

This, I suggest, has been the honourable fate of classical physics in contemporary science. But I also see the ideas of Piaget and Popper to be 'relevant contrasts' to those of Kelly, Feyerabend and the ACM as considered in the context of concerns of this thesis. My remaining sub-sections and chapters, then, shall partially serve to demonstrate further this view.

4.3 Dimensionality in Knowledge Claims

In this sub-section I shall take it largely for granted that I have already demonstrated compellingly that dimensionality (i.e. a dimension of opposite contrast) in knowledge claims is a cardinal feature of PCP and that this feature is absent from, indeed, denied by, all objectivist traditions in epistemology.

Dimensionality in knowledge claims enters post-empiricist philosophy of science through an increased interest in, and emphasis upon, meaning, context-dependency and dialectic in the making and testing of scientific knowledge claims.

I contend that in the case of Kuhn qua post-empiricist philosopher of science, however, the sheer scale of his explanatory notion of the 'paradigm', in this 'disciplinary matrix' meaning for the term lessens or, at least, obscures from consideration, any quality of
dimensionality in its component notions. Herein lies my main reason for preferring Feyerabend's version of the incommensurability thesis and which I shall now discuss.


Admitting of certain ambiguities in the way in which he used this term in his earlier work, however, Kuhn then goes on to clarify two main meanings which he intends for it.

Kuhn identifies the first of these meanings by the phrase 'disciplinary matrix' and which he explicates thus:

'[ ] "disciplinary" because it is the common possession of the practitioners of a professional discipline and "matrix" because it is composed of ordered elements of various sorts, each requiring further specification.' (Kuhn [1974] 1977, p. 297).

Amongst the 'ordered elements' of a disciplinary matrix which Kuhn cites and discusses are 'exemplars':

'Exemplars [ ] are concrete problem solutions, accepted by the group as, in a quite usual sense, paradigmatic.' (Kuhn, [1974] 1977, p. 298).
Although Kuhn holds that exemplars are partially constituent of a disciplinary matrix, he also claims that they represent his second, and 'more fundamental', meaning for 'paradigm'.

For Kuhn, it is by means of encountering exemplars that student scientists learn 'how their job is to be done' (Kuhn 1970a, p. 187), viz. they learn, for example, how to interpret scientific terms and to use the particular form or derivation of a 'shared symbolic generalization', such as $F = ma$, which is judged appropriate by the community of scientists. It is important to stress, however, that whilst Kuhn suggests that exemplars 'can serve cognitive functions commonly attributed to shared rules' (Kuhn, [1974] 1977, p. 319), he also claims that they are not themselves amenable to such rules or to comprehensive systematization.

Since Kuhn's first meaning of 'paradigm' entails his second I shall henceforth use the term 'paradigm' to refer to both, but with emphasis upon the former.

For Kuhn, commitment by a community of researchers to a particular paradigm is the most important characteristic of science and results in what he terms 'normal science'. During periods of normal science, practitioners or members of the paradigm unquestioningly endorse and employ the fundamental assumptions constituent of the paradigm in their research activities. These latter are construed as 'puzzles' rather than 'problems' since their solution is confidently anticipated – and only permitted – to be achieved entirely within the framework provided by the paradigm. Failure to solve a puzzle is understood to reflect lack of ingenuity on the part of the scientist rather than theoretical and/or methodological weakness.
The sharing of a paradigm by a community of researchers is construed by Kuhn to be the sign of scientific "maturity" of the investigative field and, indeed, also serves as an informal demarcation criterion for science. Zylberstajn (1983, Ch 1, pp. 5-6) selects from Kuhn's work the following 'classical' ("meta-paradigmatic") examples of periods of normal science: astronomy during the Middle Ages (Ptolemaic paradigm); mechanics during the eighteenth and nineteenth centuries (Newtonian paradigm); and quantum mechanics after the 1930's (Copenhagen interpretation paradigm).

As the probing of the paradigmatic disciplinary matrix becomes deeper and more extensive, the puzzles posed and their suggested solutions become correspondingly more sophisticated. Eventually, however, it is a characteristic of the history of science that some of these later puzzles shall persistently deny the attempts of even the ablest members of the research community to solve them. When this happens, confidence in the adequacy of the disciplinary matrix to provide a foundational framework for puzzle solutions may begin to falter, generating a state of crisis. Such crises generally occur rarely in the 'mature' sciences but may only be re-solved by the emergence of a new disciplinary matrix which is judged capable of providing a framework for solving at least the most relevant of the puzzles of the previous matrix and also promises to do so for a new set of interesting and important puzzles. When this happens a 'paradigm shift' or 'scientific revolution' has occurred, e.g. the overthrow of Ptolemaic by Copernican astronomy.

The constituents of any paradigm are informed by a set of fundamental metaphysical (i.e. ontological and/or epistemological) commitments. Those of one paradigm are incommensurable with those
of another. Kuhn stresses that a scientific revolution is not led
or induced by objective 'refutations' of theories comprising the
earlier paradigm: an incommensurable alternative must first have
been suggested, albeit, in a characteristically more tentative and
necessarily less developed form.

The incommensurability of paradigms, and of certain theories within
them, are only given explicit, sustained, consideration by the
community of scientists during the relatively short periods of
scientific crisis and revolution. For Kuhn, the uncritical
allegiance of scientists to a paradigm during the far lengthier
periods of normal science is what allows serious and successful
scientific research to get done since it avoids, indeed, debar,
mainstream research effort being diluted in endless debate over
fundamentals.

Now, as I have already intimated in section 4.2, above, Feyerabend's
notion of incommensurability differs in emphasis from that of Kuhn.
I shall now try to show that this difference renders Feyerabend's
ideas much closer to those of Kelly (and myself) relative to Kuhn's.

My discussion of this issue may helpfully be introduced by
presenting one of Feyerabend's summarized accounts in which he
delineates his treatment of the incommensurability thesis from that
of Kuhn:

'General assertions about incommensurability are more
characteristic for Kuhn [ ]. There do exist cases
where not only do some older concepts break down the
framework of a new theory, but where an entire
theory, all its observation statements included, is
incommensurable with the theory that succeeds it, but
such cases are rare and need special analysis.'

(Feyerabend, 1981a, p. xi: original emphasis).

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Feyerabend's statement of his views in the quotation above may seem to imply a less radical stance than Kuhn's; however, the reverse is actually the case, for whilst Feyerabend agrees with Kuhn that the adoption of fundamental assumptions play a crucial part in the interpretation of specific situations, he proceeds to challenge Kuhn's claims for the pervasiveness, at any one time, and the longevity, through time, of the commitment of a community of researchers to any particular paradigm and hence also for the corresponding rarity of scientific crises and revolutions.

Feyerabend begins his principal critique of Kuhn's ideas by claiming 'ambiguity of presentation' in the latter's work:

"Whenever I read Kuhn, I am troubled by the following question: are we here presented with methodological prescriptions which tell the scientist how to proceed; or are we given a description, void of any evaluative element, of those activities which are generally called 'scientific'?" (Feyerabend, 1970, p. 198: original emphasis).

To consider Feyerabend's second, "descriptive", interpretation first, Feyerabend re-examines the history of science and concludes that normal science does not exist. Part of his argument is as follows:

"In the second third of [the last] century there existed at least three different and mutually incompatible paradigms. They were: (1) the mechanical point of view which found expression in astronomy, in the kinetic theory, in the various mechanical models for electrodynamics as well as in the biological sciences, especially in medicine (here the influence of Helmholtz was a decisive factor);"
(2) the point of view connected with the invention of an independent and phenomenological theory of heat which finally turned out to be inconsistent with mechanics; (3) the point of view implicit in Faraday's and Maxwell's electrodynamics which was developed, and freed from its mechanical concomitants, by Hertz.' (Feyerabend, 1970, p. 207: original emphasis).

Feyerabend goes on to argue that far from being 'quasi-independent' it was their 'active interaction' which brought about the downfall of classical physics - for reasons which I shall not attempt to discuss here (but see Feyerabend, 1970, p. 208ff). At any point in the history of science up to and including the present, it can be shown that there are just too many eminent scientists, recognized and funded as such, who are working outside the ruling paradigm of the day for the notion to remain tenable (for one contemporaneous example amongst many, of Bohm's 'hidden variables' interpretation of quantum mechanics with the allegedly 'paradigmatic' Copenhagen interpretation). Moreover, if, for the sake of argument, it is granted that paradigm bound puzzle-solving activities exist to the extent that Kuhn claims, then they constitute a pretty useless criterion for science for such a criterion cannot exclude, for example, 'organized crime' (Feyerabend, 1970, p. 200).

More serious for Feyerabend than such "academic" disputes, however, is the "prescriptive moral" for scientists - and would-be scientists - which flow explicitly from Kuhn's (relatively speaking) "monolithic" interpretation of history of science. Here Feyerabend is totally at odds with Kuhn.
For Feyerabend, contra Kuhn, the danger is not that ongoing, centre-stage, metaphysical disputation amongst scientists shall impede the conduct of scientific research, but, rather, that its endorsed avoidance would diminish the very processes of psychological channelization that created scientific knowledge in the first place. The result would be a degeneration into scientific dogmatism and anaemia: the ruling paradigm of the day would be embraced for no other reason than that it is the current orthodoxy (of. Papineau's, 1979, p. 40 commentary). Elsewhere, Feyerabend mercilessly concludes his appraisal of Kuhn's work thus:

'Kuhn's ideas are interesting but, alas, they are much too vague to give rise to anything but lots of hot air. If you don't believe me, look at the literature. Never before has the literature on the philosophy of science been invaded by so many creeps and incompetents. Kuhn encourages people who have no idea why a stone falls to the ground to talk with assurance about scientific method. Now I have no objection to incompetence but I do object when incompetence is accompanied by boredom and self-righteousness. And this is exactly what happens. We do not get interesting false ideas, we get boring ideas or words connected with no ideas at all. Secondly, wherever one tries to make Kuhn's ideas more definite one finds that they are false. Was there ever a period of normal science in the history of thought? No - and I challenge anyone to prove the contrary.'

(Feyerabend, 1975a, p. 6: original emphasis).

Now, I suggest that Kuhn's notion of normal science also renders the intellectual edifice of science too monolithic to be compatible with Kelly's notion of a construction system, for commonality amongst construction systems does not alter their basic constructive structure. When Kelly says that the person 'is an inveterate collector of paradigms' (Kelly, 1969, p. 47) I choose to interpret it not only in the 'perspective of centuries' but also in the
here-and-now sense. (I am not, of course, suggesting that Kelly was using the term 'paradigm' specifically in any of Kuhn's specialized senses).

Furthermore, I consider Kuhn's notion of normal science only to apply (or to apply best) to the traditionally most influential body of science pedagogies, as concretely instanced by science text-books. It is from here that my principal objections to Kuhn arise - perhaps not surprisingly since I am an educationalist.

To explain, the monolithic character of normal science not only misrepresents the actual conduct of science as it usually is, it also does so in a way which effectively obscures and precludes dimensionality (and hence relevance and responsibility) in knowledge claims. The most obvious and defensible pedagogic interpretation of Kuhn's ideas is thus a very conservative one. The excellence of science in general, and the ruling paradigm in particular, is assumed; it is not argued for. This accounts for Zylberstajn's omission, as I see it, of 'philosophers' science' from his model (cf. Chapter 2). Though Kuhn is not an objectivist in his philosophy of science, he may, I suggest, be legitimately treated as one in his science pedagogy.

These recent points may be illustrated and partially corroborated by reference to Kuhn's emphasis upon all-or-none 'gestalt switches' or 'conversion experiences', as he terms them, in the learning of science via repeated encounters with exemplars, e.g.
'After he [the student] has completed a certain number [of exemplary problems], which may vary widely from one individual to the next, he views the situations that confront him as a scientist in the same gestalt as other members of his specialists' group. For him they are no longer the same situations he had encountered when his training began. He has meanwhile assimilated a time-tested and group-licensed way of seeing.' (Kuhn, 1970a, p. 189: my emphasis).

From here it may be but a short route to argue for mere indoctrination of student scientists in the preferred paradigm as the most efficient teaching strategy. This would run directly counter to my views on what science education (and, more generally, education) is for, and which I shall elaborate in detail in Chapter 10. At the risk of slightly pre-empting my exposition of these, however, I shall quote the educationalists Rowell and Cawthorn who lucidly demonstrate that Kuhn himself is not altogether unaware of the paradoxes and other difficulties which his epistemological doctrines create for the development of compatible pedagogies:

'It is time we asked ourselves: 'What do we want our students to know about science?' But if the question looks disarmingly simple, the answer is less so. For example, on the one hand Kuhn himself flays our science texts for their 'misrepresentation' of what science is (Kuhn, 1970[a], p. 140), and on the other he commends an education based on them for equipping the budding scientist 'almost perfectly' for 'normal' scientific work (Kuhn, 1970[a], pp. 165-166). Certainly our texts portray science as some inexorable linear pursuit of truth (Cawthorn and Rowell, 1978). But Kuhn's deliberations as presented here, paint less than half our picture. Science is not taught only to intending scientists; indeed that group is a relatively small minority. And even for them it is certainly arguable that they must be 'hoodwinked' in order to become effective normal scientists.' (Rowell and Cawthorn, 1982: original emphasis, my square brackets).
Thus, it may further be argued, whilst Kuhn states that 'of course, [science education] is a narrow and rigid education, probably more so than any other except perhaps in orthodox theology' (Kuhn, [1962] 1970, p. 166), his epistemological doctrines complement just such an education and, moreover, would undermine a more 'revolutionary' and, as I would see it, enlightened one (cf. my discussion of compatible and incompatible relations between epistemology and pedagogy in Chapter 5).

Now, although Feyerabend makes it clear that he considers incommensurability and meaning to be closely related in the natural sciences (as in all "domains" of knowledge), he does not decisively embrace dimensionality in his theoretical context account of meaning. This notwithstanding, I contend that Feyerabend proceeds in a manner which amounts to his doing so, viz. unlike Kuhn, Feyerabend elevated incommensurability to an article of scientific-method.

Thus, Feyerabend exhorts that the scientist who wishes to maximise the empirical content of the views they hold, and/or to advance their understanding and knowledge in any other way, must embrace what he terms the 'principle of proliferation' and which constitutes a 'pluralistic methodology', viz.

'The principle of proliferation: Invent, and elaborate theories which are inconsistent with the accepted point of view, even if the latter should happen to be highly confirmed and generally accepted. Any methodology which adopts the principle will be called a pluralistic methodology. The theories which the principle advises us to use in addition to the accepted point of view will be called the alternatives of this point of view.'

In a pluralistic methodology, the scientist 'must compare ideas with other ideas rather than with 'experience' and he must try to improve rather than discard the views that have failed in the competition.' (Feyerabend, 1975b, p. 30: my emphasis). By such means, older views shall be elaborated and used to measure the success of 'modern' views; history of science is construed to be an inseparable part of science itself, contributing both to the development and to the content of theories. He argues that the creation, recognition and discussion of particular cases of incommensurability 'often reveal hidden ideas, replace them by ideas of a different kind, and change overt as well as covert classifications.' (Feyerabend, 1975b, p. 225).

One example amongst many which Feyerabend discusses is Galileo's analysis and re-interpretation of the so-called 'tower argument' which at the time was accepted and used by many (e.g. Tycho) as an irrefutable justification for the view that the earth is motionless. The tower argument drew its principal empirical sustenance from the commonsense observation that heavy, falling bodies go by a straight, vertical line to the surface of the earth. Galileo's counter-argument (which need not concern us here) not only vanquished the tower argument, but also, according to Feyerabend, 'led to a clearer formulation of the Aristotelian theory of space and [ ] revealed the difference between impetus (an absolute magnitude that inheres in an object) and momentum (which depends on the chosen reference system).' (Feyerabend, 1975b, p. 225; also see his Chapters 6 and 7).
For Feyerabend, the possibility that any earlier theory — and from 
any "domain" of knowledge, e.g. history of science, alchemy, 
Genesis, Greek mythology (students' science?!), might be enlisted in 
this way for the development of scientific knowledge remains forever 
open. No view can ever be omitted from a comprehensive account: as 
in PCP, any notion might someday become a relevant contrast and so 
Feyerabend urges that we should not endorse methodological rules 
which would prevent this. History of science is thus an essential 
part of scientific-method — a view which is totally at odds with 
Kuhn's rather conventional endorsement of praxis:

'Science textbooks are studded with the names and 
sometimes with portraits of old heroes, but only 
historians read the old scientific works. In science new 
breakthrough[s] do initiate the removal of suddenly 
outdated books and journals from their active 
position in a science library to the desuetude of a 
general depository. Few scientists are ever seen in 
science museums, of which the function is, in any 
case, to memorialize or recruit, not to inculcate 
craftsmanship or enlighten public taste. Unlike art, 
science destroys its past.'
(Kuhn, 1977, p. 345).

It is worth noting that Feyerabend's views also further eradicate 
the traditional distinction between art and science that even Kuhn 
shows himself to support by this comment.

Pluralistic methodology is not only the necessary and principal 
means by which Feyerabend supposes science to be carried out in 
fact, but also is an ad interim maxim by which our valuations of the 
fruits of scientific activity may be saved from lapsing into mere 
dogmatic allegiance to them sided and abetted by (meta-)theory.
Consistent with these views, Feyerabend argues that outside the domain of empirical generalizations

'[...] the methodological unit to which we refer when discussing questions of test and empirical content consists of a whole set of partly overlapping, factually adequate, but mutually inconsistent theories.'
(Feyerabend, 1981a, p. 72: original emphasis).

And elsewhere that

'i. Traditions are neither good nor bad, they simply are. 'Objectively speaking' i.e. independently of participation in a tradition there is not much to choose between humanitarianism and anti-semitism. Corollary: rationality is not an arbiter of traditions, it is itself a tradition or an aspect of a tradition. It is therefore neither good nor bad, it simply is.
ii. A tradition assumes desirable or undesirable properties only when compared with some tradition i.e. only when viewed by participants who see the world in terms of its values. The projections of these participants appear objective and statements describing them sound objective because the participants and the tradition they project are nowhere mentioned in them. They are subjective because they depend on the tradition chosen and on the use the participants make of it. The subjectivity is noticed as soon as participants realize that different traditions give rise to different judgments.'
(Feyerabend, 1978, pp. 27-28: original emphasis).

I consider these and many other views expressed by Feyerabend to demonstrate his awareness of the importance of opposite contrasts in the making of knowledge claims. Moreover, and as I shall make explicit in Chapters 5 and 10, I consider them closely to complement both the investigative methods and the educational values of the ACM.
It is also worth noting that providing Lakatos' objectivistic pretensions are ignored (i.e. providing that his ideas are given relativistic or post-empiricist interpretation), then his view that 'history of science suggests that [ ] tests are - at least - three-cornered fights between rival theories and experiment' (Lakatos, 1970, p. 115) and the 'pluralistic system of authority' implied by his 'methodology of scientific research programmes' may similarly be argued to complement Kellyan (and ACM) notions.

Finally, having argued that Feyerabend (and, to a lesser extent, Lakatos) embraces in effect, if not in fact, a Kellyan requirement for dimensionality in knowledge claims through their ideas on scientific method, let me turn it around and consider possible similarities between the methodology of PCP and their epistemologies. Here, I believe that an equal case can be made - no stronger than that. Thus, whilst Kelly does not actually exhort us to embrace a 'principle of proliferation', his advice that 'we must continuously and adventurously hold all matters open to the possibility of fresh reconstruction.' (Kelly, 1966, p. 5) amounts to the same thing. Similarly, his psychotherapeutic (or as Kelly preferred to put it, 'reconstructive') tactics, perhaps most obviously his 'Fixed Role Therapy' (discussed in Chapter 10), may be interpreted as a pluralistic methodology.

4.4 Relevance and Responsibility in Knowledge Claims

Earlier on (in section 4.2) I suggested that CER and correspondence theories of truth play no role whatsoever in post-empiricist traditions in philosophy of science and that this has been used by
objectivist-realists to mount a *prima facie* case that such
traditions knowingly or unknowingly embrace *irrationalism* in
matters of theory-choice and evaluation - or, to dignify it,
'idéalism', 'extreme relativism' or even 'solipsism', but
objectivists tend to use the terms interchangeably.

Since I perceive these features of post-empiricism (i.e. rejection
of CER and correspondence theories of truth) also to be judged
Anpalatable by most science educationalists and by virtue of the
same "reasons", in this section I shall attempt to show that this
objectivist critique represents a paradigm example of "lumping the
contrasting with the irrelevant" and that with respect to the
relativist-methodological-constructivist approach which I endorse
and develop in this thesis in particular they are simply irrelevant.

I am sympathetic to certain of Barnes and Bloor's comments on
relativism and which I believe shall help me further to delineate
the species which I endorse and shall inform this aspect of my
counter-critique of objectivism:

"There are many forms of relativism and it is
essential to make clear the precise form in which we
advocate it. The simple starting-point of relativist
doctrines is (i) the observation that beliefs on a
certain topic vary, and (ii) the conviction that
which of these beliefs is found in a given context
depends on, or is relative to, the circumstances of
the users. But there is always a third feature of
relativism. It requires what may be called a
'symmetry' or an 'equivalence' postulate. For
instance, it may be claimed that general conceptions
of the natural order, whether the Aristotelian world
view, the cosmology of a primitive people, or the
cosmology of an Einstein, are all alike in being
false, or are all equally true. These alternative equivalence postulates lead to two varieties of relativism; and in general it is the nature of the equivalence postulate which defines a specific form of relativism.

The form of relativism that we shall defend employs neither of the equivalence postulates just mentioned, both of which run into technical difficulties. To say that all beliefs are equally true encounters the problem of how to handle beliefs which contradict one another. If one belief denies what the other asserts, how can they both be true? Similarly, to say that all beliefs are equally false poses the problem of the status of the relativist's own claims. He would seem to be pulling the rug from beneath his own feet.

Our equivalence postulate is that all beliefs are on a par with one another with respect to the causes of their credibility. It is not that all beliefs are equally true or equally false, but that regardless of truth and falsity the fact of their credibility is to be seen as equally problematic.1 (Barnes and Bloor, 1982, pp. 22-23: my emphasis).

I shall argue that relativist-methodological-constructivism accepts '[(i)]' and '[(ii)*] in Barnes and Bloor's account and also their version of the equivalence postulate. As I have earlier mentioned, however, I depart from the exclusively sociological approach pursued by these authors. I shall make clear the reasons for, and nature of, my departure from such an approach later in this section. This notwithstanding, since Barnes and Bloor's work may be argued to extend and to be partially derivative of Feyerabend's and especially of Kuhn's ideas, I shall first examine the way in which these latter two authors have responded to the objectivists' charge of irrationalism, or, as Barnes and Bloor might put it, to the charge of endorsing "naive" forms of equivalence postulate.
Thus, Kuhn argues that

'Later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied. That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress.

[ ]. Some critics claim that I am confusing description with prescription, violating the time-honoured philosophical theorem: 'Is' cannot imply 'ought'.

That theorem has, in practice, become a tag, and it is no longer everywhere honored. A number of contemporary philosophers have discovered important contexts in which the normative and the descriptive are inextricably mixed. 'Is' and 'ought' are by no means always so separate as they have seemed.'

(Kuhn, 1970a, pp. 206-207).

And elsewhere that

'What I am denying [ ] is neither the existence of good reasons nor that these reasons are of the sort usually described. I am, however, insisting that such reasons constitute values to be used in making choices rather than rules of choice. [ ]. Simplicity, scope, fruitfulness and even accuracy [Kuhn has earlier cited these as examples of 'good reasons'] can be judged quite differently (which is not to say they may be judged arbitrarily) by different people. Again, they may differ in their conclusions without violating any accepted rule.'

(Kuhn, 1970b, p. 262: my emphasis).

By means of such arguments I judge Kuhn plausibly to evade at least the charge of irrationalism (though perhaps not some others as I shall later discuss).
Feyerabend may be argued to escape the objectivists' claim for his irrationalism by a slightly different route from Kuhn's, viz. unlike Kuhn, Feyerabend does not claim to articulate an enduring or developed philosophical system of thought, indeed, he explicitly rejects such an interpretation of his work and identifies it as the principal source of irrelevant criticism of his ideas (see, especially, Feyerabend, 1978, Part Three). Irrationalism is precisely what Feyerabend argues that objectivists (rationalists, methodological monists) cannot preclude from their accounts.

Even if one concedes, as I do, that there is an element of disingenuity in Feyerabend's claim not to elaborate any position of his own, however, there is still plenty in his account to demonstrate that the charge of irrationalism cannot be made to stick. Moreover, while his ideas on this issue bear many similarities to Kuhn's they demonstrate a closer congruency to PCP. Accordingly, I shall first consider them in their own right and in some detail.

Here, the basic difference between Kuhn and Feyerabend may be said to begin with the latter's explicit identification of, and emphasis upon, metaphysics as values intrinsic to methodological rules and principles. The role and status which Feyerabend attributes to metaphysics in the conduct of science and of philosophy of science, however, may best be brought out by first contrasting his approach with that of Popper and objectivists.

Recall (now in slight elaboration of my earlier comments) that although Popper claims metaphysics to be not only meaningful but also, in many cases, important in the development of scientific theories as a source of initially 'dogmatic' or 'pre-critical'
inspiration, he ultimately perpetuated the empiricist doctrine of excluding it from his demarcation criterion for science - metaphysics are relegated to 'pre-scientific' or 'metaphysical research programmes' (see, e.g. Popper, 1978, pp. 150-151).17

Some of Popper's disciples have attempted to render more credible his claims for the importance of metaphysics in science by articulating a more developed role for it. Agassi (1964), for example, argues that metaphysical commitments are responsible for guiding scientists in their choice of problems and, when such commitments are shared, for co-ordinating their research efforts.18 Although Agassi questions Popper's deductive theory of explanation as being the only means of comprehension (Agasi, 1964, p. 199), he nevertheless ends up basically endorsing the orthodox Popperian line: he excludes metaphysics from his demarcation criterion for science in order to preserve the claim for the objectivity of scientific knowledge.

Now, although historically it would not seem to have happened in quite this way, Feyerabend may usefully be construed as having radically extended Agassi's views on scientific metaphysics - to the demise of objectivism.

Thus, Feyerabend argues that the use of methodological rules and standards is every bit as much an expression of a commitment to metaphysical values as is the creation and/or use of a theory. To put this another way, use of such rules has what Feyerabend (e.g. 1978, p. 213) calls 'cosmological implications', i.e. involves cosmological assumptions or principles. For example, the methodological rule 'Increase empirical content!' would eventually
cease to be applicable in a finite universe. In such a world one might expect an "ultimate" explanation which subsumes all other explanations. This could not be the result in an infinite universe, where, instead, one might expect an infinite sequence of explanations and so, also, might venerate 'criticizability' in our conjectures. Methodologies are theories. Feyerabend thus concludes that

'The standards we use and the rules we recommend make sense only in a world that has a certain structure. They become inapplicable, or start running idle in a domain that does not exhibit this structure.' (Feyerabend, 1978, p. 34).

And he repeats these sentiments in this more "metaphysical" rendering of the incommensurability thesis:

'We have a point of view (theory, framework, cosmos, mode of representation) whose elements (concepts, 'facts', pictures) are built up in accordance with certain principles of construction. The principles involve something like a 'closure': there are things that cannot be said, or 'discovered', without violating the principles (which does not mean contradicting them). Say the things, make the discovery, and the principles are suspended. Now take those constructive principles that underlie every element of the cosmos (of the theory), every fact (every concept). Let us call such principles universal principles of the theory in question. Suspending universal principles means suspending all facts and all concepts. Finally, let us call a discovery, or a statement, or an attitude incommensurable with the cosmos (the theory, the framework) if it suspends some of its universal principles.' (Feyerabend, 1975b, p. 269: original emphasis).
Now, I find the links that Feyerabend draws between methodological rules and scientific metaphysics under the aegis of universal 'constructive principles' to be a helpful development of Kuhn's ideas - a judgement which I have already alluded to by my inclusion of constructive principles within my model in Chapter 2 and which I shall elaborate upon in my discussion of Kelly later in this section.

To press on with Feyerabend's counter-critique of objectivism (and to press home his critique of it), however, we have already seen his caution that one should not be brow-beaten into believing in the objectivity of objectivist methodological prescriptions merely by virtue of their 'objective formulation' (Feyerabend, 1978, p. 23; cf. my discussion in section 3.4.2).

Moreover, Feyerabend argues that

'Philosophers of the Vienna Circle and Popperians are fond of turning cosmological principles such as the principle of causality into formal rules. As a result they eliminate circumstances that might endanger the rules.'


For Feyerabend, all empirical 'evidence', whether in corroboration or refutation of a theory, has a 'historico-physiological character'; that is to say, it does not simply describe, in some as yet uninterpreted way, some objective state of affairs, 'but also expresses some subjective, mythical, and long-forgotten views' concerning such a state (Feyerabend, 1975b, p. 67: original emphasis). There can thus be no 'factual-' or 'observational core' in matters of theory-choice (Feyerabend, 1965, p. 203 and p. 216):
methodologies, like all theories, are "value-laden". From this, Feyerabend draws the methodological inference that

'[ ] it would be extremely imprudent to let the evidence judge our theories directly and without further ado. A straight forward and unqualified judgment of theories by 'facts' is bound to eliminate ideas simply because they do not fit into the framework of some older cosmology. Taking experimental results and observations for granted and putting the burden of proof on the theory means taking the observational ideology for granted without ever having examined it.'  
(Feyerabend, 1975b, p. 67: original emphasis).

Such an examination, however, may only be achieved by pluralistic methodology.

The objectivist-methodological-constructivists' version of the correspondence theory of truth is thus not as liberal a proposal as it might initially seem for it amounts to an absolute theory of approximate truth. As Feyerabend points out

"Truth" is such a nicely neutral word. Nobody would deny that it is commendable to speak the truth and wicked to tell lies. Nobody would deny that - and yet nobody knows what such an attitude amounts to. So it is easy to twist matters and to change allegiance to truth in one's everyday affairs into allegiance to the Truth of an ideology which is nothing but the dogmatic defence of that ideology. And it is of course not true that we have to follow the truth. Human life is guided by many ideas. Truth is one of them. Freedom and mental independence are others. If Truth, as conceived by some ideologists, conflicts with freedom, then we have a choice. We may abandon freedom. But we may also abandon Truth. (Alternatively, we may adopt a more sophisticated idea of truth that no longer contradicts freedom; [ ]).' 
(Feyerabend, 1975a, p. 4: original emphasis).
In the wake of objective epistemology, Feyerabend argues, we are left with our subjective wishes and desires in matters of theory-choice. Our methods and rules for comparing theories, however, are 'arbitrary' or 'subjective' only in the sense that it has hitherto proven impossible in practice (and is very likely impossible in principle) to find 'wish-independent arguments for their acceptability' (Feyerabend, 1978, p. 68: cf. Kuhn's, 1977, pp. 337-338, remarks on subjectivity). Far from being something to be deplored, it is by recognising and celebrating this fact that relevance and responsibility of science for society might be achieved to an unprecedented extent and duration. Feyerabend's views on philosophy of science and on politics may be seen to fuse in the following two statements which he makes:

'Pluralism of theories and metaphysical views is not only important for methodology, it is also an essential part of a humanitarian outlook.'
(Feyerabend, 1975b, p. 52).

'For me democracy, the right of people to arrange their lives as they see fit comes first, 'rationality', 'truth' and all those other inventions of our intellectuals come second.'
(Feyerabend, 1978, p. 145 n. 8).

These are views which I wholeheartedly endorse. (Note that none of the above denies the possibility that certain values might actually be true; what is denied however, is that we presently have sufficiently good reasons for deciding which and when).

Now, Hesse has commented that

'Post-empiricist analyses of science have placed more emphasis on theories than their empiricist predecessors, but in the end they support rather than
undermine the conclusion that natural science is essentially instrumentalist.' (Hesse, 1980, p. 177).

If we ignore Feyerabend's "no position" ("or pluralistic position") claim as sufficient defence against this assertion, if held to apply against him, then I suggest that its force is still undermined by many of his interpretations of particular episodes in the history of science and which demonstrate the high utility - in context - of (ontologically) realistic thinking, albeit, with no claim to objectivity in epistemology. The most noteworthy example here is Feyerabend's discussion of the so-called Copernican Revolution as mediated by Galileo and which he presents through several chapters of his most famous work, Against Method (Feyerabend, 1975b).

Certainly, Feyerabend often talks in the manner of a realist. Thus he states, for example, that 'scientific theories are ways of looking at the world; and their adoption affects our general beliefs and expectations, and thereby also our conception of reality.' (Feyerabend, 1965, p. 29) - a view which McEvoy (1975, p. 49) points out is in stark contrast to the positivism which sees the aim of science as being the systematization of experience that exists independently of any scientific theories. Feyerabend, however, cannot straightforwardly be said to be either a realist or an instrumentalist: he is each in different contexts.

It is also important to note that Feyerabend uses historical episodes in the history of science not only to undermine objectivist accounts but also to demonstrate the utility of what he terms 'methodological suggestions' or 'hints' (Feyerabend, 1978, p. 143). These include the use of ad hoc theories, connections with influential ideologies, political force to revive theories that are
(For a list of around eight such suggestions, together with references to his detailed discussion of them, see Feyerabend, 1978, p. 143 n. 4). This effectively protects Feyerabend against the frequently made charge that he is a 'nihilist' and, moreover, may be construed as demonstrating some parallels with the 'technical eclecticism' (Karst, 1977) sanctioned by PCP in its psycho-therapeutic setting (cf my extension of this notion to pedagogy in Chapter 10). The suggestive or invitational character of Feyerabend's methodological suggestions is guaranteed by the context-dependency, indeed, I propose as an informal rule for recognizing and appraising this quality in any field of research that the invitational, as opposed to the prescriptive, nature of a methodological idea might be said to vary in direct proportion to its context-dependency (or in negative proportion to its context-independency).

Feyerabend's most famous slogan - 'anything goes' - is thus not a 'central thesis' or a methodological 'principle' which he personally endorses, as close reading of Against Method soon reveals. But he surely makes this point unequivocably clear in a later reply to a critic; 'anything goes' is the naive equivalence postulate which rationalists (objectivists) cannot preclude from their methodologies:

'[...]'anything goes' does not express any conviction of mine, it is a jocular summary of the predicament of the rationalist: if you want universal standards, I say, if you cannot live without principles that hold independently of situations, shape of world, exigencies of research, temperamental peculiarities, then I can give you such a principle. It will be empty, useless, and pretty ridiculous - but it will be a 'principle'. It will be the 'principle'

'anything goes.'
(Feyerabend, [1978] 1978, p. 188: original emphasis).
Feyerabend demonstrates that everything can stay in (cannot be excluded from) attempts to construe scientific method as a 'context-independent organon of thought' and, further, that everything would stay in a comprehensive account — but only rationalists and self-deluders would wish actually to attempt these things. As we have already seen, Feyerabend holds that in the face of the failure of objectivist epistemology, science emerges as a 'context dependent plausibility' (Feyerabend, 1981b, p. 18: quoted more fully in section 4.2, above).

Hesse's characterization of what she terms 'extreme forms' of relativism shall also help me further to delineate Feyerabend's pluralist interpretation and thence my own, basically Kellyan, brand of relativist-methodological-constructivism:

'In extreme forms of relativism theories are regarded only as internally connected propositional systems, or 'language games'; they are world views to be given significance in their own right. 'Truth' is defined as coherence with the theoretical system and 'knowledge' becomes socially institutionalised belief. The view is 'relativist' in the sense that there are no cross-theory criteria for belief, nor progressive approximations to universally valid knowledge in the theoretical domain.' (Hesse, 1980, p. xiv).

Now, although Feyerabend might be said to endorse each of these features in Hesse's characterization, above, I contend that he does so in ways which set him apart from most other post-empiricist traditions and that these differences bring his ideas closest from amongst such traditions to those of Kelly.
Thus, Feyerabend's context-dependent defences of realism distances his ideas from the straightforwardly idealist accounts implied by Hesse's first sentence. With respect to her second sentence, his acceptance of it would be heavily qualified by his advocacy of pluralistic methodology: one of his most important themes is his rejection of what he terms the 'consistency condition', viz. the methodological requirement that new hypotheses should agree with accepted, well established, theories (Feyerbend, 1975b, esp. Ch. 3). Feyerabend would wholeheartedly agree with Hesse's last sentence since it may be interpreted as a re-statement of the incommensurability thesis.

What I wish now to argue, in outline only, is that it is "sociological" perspectives on the construction of knowledge which are most susceptible of the charge of 'extreme relativism' by virtue of their effective (if unacknowledged) idealism whereby they open the door to many of the worst excesses of objectivist-empiricism, including dogmatism, authoritarianism and conservatism, plus one of their own: a crucial vagueness over the issue of (e.g. scientific) method. This part of my case hinges on the evaluation of values (constructive principles) underlying theories.

Thus, having demonstrated (as I interpret him) the value-ladenness of theories, Feyerabend goes on, however, to argue against
attributing 'objectivity' to values merely by virtue of their 
(degree of) sharedness amongst persons:

'Reduction to shared principles is not always possible 
and so we must admit that the demands or the formulae 
expressing them are incomplete as used and have to be 
revised. Continued insistence on the 'objectivity' 
of value judgments however would be as illiterate as 
continued insistence on the 'absolute' use of the 
pair 'up-down' after discovery of the spherical shape 
of the earth.'
(Feyerabend, 1978, p. 23).

This not only pulls the rug from under any "final move" of the sort 
attempted by Popper and the more astute objectivist-methodological-
constructivists (cf. Popper, [1945] 1980b, p. 231: quoted and 
discussed in section 3.4.2) but also from objectivist sociologists 
of knowledge19, such as Bloor, who argues that

'The objectivity of knowledge resides in its being the 
set of accepted beliefs of a social group [ ]. The 
authority of truth is the authority of society.'
(Bloor, 1976, p. 76).

Notwithstanding Bloor's awareness of the dangers of too simplistic a 
construal of such a notion (which I shall term 'social 
objectivism'), I contend that there are many others who do so 
subscribe and lapse into a viciously circular or naive form of 
functionalist ideology which Bloor has elsewhere specifically 
criticized and rejected:

'As a doctrine about society, functionalism, in its 
most naive form sees all the institutions, norms, and 
groupings of people as related to one another in ways 
that subserve the ends of the existing form of 
society, and its survival and stability through 
time. The well-known shortcoming of naive 
functionalism is that it systematically obscures 
those modes of interaction between component parts of
Long-term and deep-rooted conflicts of interest, for example, are difficult to grasp if one's starting-point is to see each component of society as having a function that is to be understood purely in terms of existing form of society.' (Bloor, 1973, p. 9: original emphasis).

Even Bloor himself and others, such as Kuhn, remain too close to functionalist thinking for my taste.

Those who seek to extend (or over extend) social objectivism to encompass the world's peoples escape Mannheimian paradoxes of 'total ideology' only by arbitrarily granting privileged status to certain 'domains' of knowledge, usually science, and usually also to certain categories and schemas for categorization within them.

Some social "constructivists" vaunt not only consensus as a criterion for knowledge, but also communicability as a criterion for meaning: Ziman, for example, argues that

'[ ] the absolute need to communicate one's findings, and to make them acceptable to other people, determines their intellectual form. Objectivity and logical rationality, the supreme characteristics of the Scientific Attitude, are meaningless for the isolated individual; they imply a strong social context, and the sharing of experience and opinion [ ] we come to believe in them as authorities in their own right.'

Ziman's disarmingly modest caveat that the book from which I have excerpted this quotation represents only a work of 'amateur philosophy' (p. xi) should not, however, be allowed to excuse or disguise the extremity of his doctrine of meaning and he makes it without a 'private language' argument in sight (cf. Hesse's allusion
to Wittgenstein's notion of 'language games'). Such commitments to
the social construction of knowledge have been used by some to
promote what I see as a "false-antithesis" between personal and
social construction (see my arguments in Chapter 10 against the
science educationalist Joan Solomon in this regard).

In sociological and consensual accounts of knowledge, the agency and
integrity of the individual person is either ignored or denied. The
latter is particularly apparent in sociological theories which have
a strongly political ideological base, e.g. orthodox Marxist
'analyses'. I have never seen a remotely convincing account that,
for example, Einstein created General Relativity Theory in response
to the 'class struggle' - or, for that matter, any other social
"forces" or "pressures". Nor do I envisage one ever being
forthcoming (though a tentative case for the social construction of
his 'special' theory might be made).

Alternatively, to pursue a more Wittgensteinian line, philosophy
becomes an interminable therapeutic activity, as opposed to theory,
in which (on one interpretation of his ideas) the 'utility', or
meaning, of a word becomes the sum total of its possible uses.
Echoes of such an approach are to be found in the recent writings of
philosophers such as Rorty (1980) whose ideas are gaining influence:
he talks of philosophy becoming a series of 'edifying discourses'.

While I have considerable sympathy for such approaches (when freed
from Wittgenstein's private language argument) I feel that they
consistently over extend the universe of discourse in the
explication of particular words and ideas. As a result, they fail
to capture the context-dependent pragmatism of knowing in general
and knowing scientifically in particular. Although relativist-
methodological-constructivism means that we cannot ever know our
theories to "cut the world at its joints" this is exactly the
assumption we would seem to have to make if we are actually to get
on with the business of knowing— and these constructive processes,
I contend, can only plausibly and economically be elucidated by
meta-theory. As if to demonstrate this point, Wittgensteinians are
notorious for "lapsing" into theory in order to develop their
philosophical activities (cf. Scarlefs, 1982, remarks on
Wittgensteinians and the philosophy of language).

I consider the forward looking immediacy and pragmatism of science
to have been most brilliantly identified and discussed by Feyerabend
and that at least part of this has to do with his emphasis upon the
interest, expectations and contributions of the individual
scientist.

Now, I have already remarked upon the pre-eminent concern of PCP
with the "perspective of the personal" and this is, of course, a
crucial similarity between Kelly's and Feyerabend's approaches.
Moreover, this concern of Kelly's would seem to derive from a
similar construal of constructive dynamics.

As I interpret Kelly, true 'sociality' of construction is impossible
in all non-trivial knowledge claims, and, however it be understood,
"consensus" cannot be elevated into a criterion for objectivity as
Kelly himself explains:
human constructions derive their objectivity wholly from the way they cast events into varying arrays — or simply from the lines of perspective they provide. Actually it is in terms of such arrays that consensual judgment becomes psychologically possible. Consensus itself, while often cited as the criterion of objectivity, does not properly define the psychological grounds on which objectivity rests. Only sociological grounds are implied. (Kelly, 1969, p. 290).

I suggest that, instead, Kelly's notion of 'commonality' of constructions, i.e. inter-subjective similarity between constructions, allows us usefully to (re)construe all knowledge claims and situations which social "constructivists" consider to be quintessentially "social" but yet without denying or pre-empting individual contributions to the collective constructive processes which created it (cf. Kelly's, 1955, pp. 90-94, 'Commonality Corollary': also my discussion in Chapter 10 where I consider the dynamics of social construction of knowledge from a Kellyan perspective).

Kelly himself clarifies how his notion of personal meaning relates to commonality and communication in the following way:

'This concern with personal meaning should prove no less valuable to the scientist than it has to the psychotherapist. It stems from the notion that when a person uses a word he is expressing, in part, his own construction of events.' (Kelly, 1969, p. 74: my emphasis).

And he expresses his commitment to method thus:

'[ ] humanistic psychology needs a technology through which to express its humane intentions. Humanity needs to be implemented, not merely characterized and eulogised.' (Kelly, 1969, p. 135).
Kelly renders this latter claim palpable by articulating a whole battery of 're-constructive' technologies, of which Repertory Grid Techniques (RGT) share the closest relation to his formal theory (cf. my discussion of RGT in Chapter 7). Kelly, then, is a relativist—methodological-constructivist.

Now, Feyerabend's and Kelly's joint pre-eminent concern with the "perspective of the personal" brings risks of its own, however, as Trigg warns:

'Whereas the temptation of those who talk of the social construction of reality must always be relativism, that of those who deal with the individual's construction of it must be solipsism.' (Trigg, 1980, p. xv).

I shall not try to defend Feyerabend against the possible charge of solipsism (or, more generally; of idealism): his "no position" claim would not warrant me even trying to do so. Rather, I shall use my recent discussion of Feyerabend's ideas as a backdrop so as to defend those of Kelly and to uphold my contention that the latter's ideas constitute a metatheory which is application to science. I judge Kelly's ideas to provide superior ground for 'rational integration' (Karst, 1980, p. 167) than do those of Feyerabend, but yet they remain equally non-pre-emptive.

Kelly himself was not unduly concerned about demonstrating the epistemological realism of his theory. Perhaps because his principal concerns in psychotherapy lay elsewhere, Kelly simply stated that:
'We presume that the world is really existing and that man is gradually coming to understand it.'
(Kelly, 1955, p. 6: my emphasis).

And elsewhere he deals with this issue in a downright peremptory manner:

'The fact that my only approach to reality is through offering some responsible construction of it does not discourage me from postulating it is there. The open question for man is not whether reality exists or not but what he can make of it.'
(Kelly, 1969, p. 25: my emphasis).

In statements such as these, then, Kelly demonstrates his commitment to ontological realism and his sympathy for CER. Close scrutiny of Kelly's text, however, reveals that he does not argue for CER. He similarly does not propose or endorse a correspondence theory of truth or, indeed, a theory of truth of any kind. Even the eminent personal constructivists Landfield and Leitner concede that 'Personal Construct Theory makes some concessions to realism but [it is] predominantly idealistic' (Landfield and Leitner, 1980, p. 3).

All this begs an obvious question: is PCP realistic enough to pass muster as a constructivist epistemology? Or is its claim to realism 'no more than an assertion of dogmatic faith belied by all the facts' (Trigg, 1980, p. 36)? In other words (to continue in Trigg's idiom), is 'reality at risk' in PCP?
In response to such questions, I shall argue that, suitably interpreted (and, perhaps, augmented), PCP does provide compelling grounds for it to be considered to be a realist theory of knowledge (or, better, since epistemic objectivism is rejected, a 'realist meta-theory').

I am particularly keen to delineate "Kellyan" relativist-methodological-constructivism from idealist brands of relativism because I find the Idealist Assumption, with its epistemological consequence that sense experience is "rhetorical" and nothing more, impossible to accept - yet I have heard my work, and that of my immediate colleagues, described as "anti-realist".

My rejection of idealist epistemologies begins with two strongly held personal convictions, viz. contra idealists, (a) I do believe that our useful, and usable, theories share a relationship with ("collide" with) a mind-independent reality; and (b) I do believe that our useful, and usable, theories are partially constrained by our sense-experience. I further contend that these convictions can be expressed within Kellyan meta-theory.

Now, to justify my further contention would seem to require that I postulate, at the very least and in a manner which does not contradict any of my earlier endorsed views, (a)' a relationship between our theories and reality (i.e. a theory of reference); and, (b)' a role for sense-experience in acts of theory-choice (i.e. crucial experiments).

I shall begin my demonstration of fulfillment of requirement (a)' by examining the idea that any construct or construction system implies
a kind of "conceptual closure" and is thus "absolute" or "deterministic" in some sense: within PCP, "freedom" and "determination" are not held to be mutually exclusive or contradictory qualities, indeed, they are deemed to be complementary notions (cf. my earlier remarks on "constructive causality" in section 4.2). Kelly introduces this point by proposing two forms of determinism which are pertinent to his theory:

'...there are two forms of determinism which concern us. The one is the determinism which is the essential feature of any organised construction system - the control of superordinate constructs over subordinate elements. The second is implied in our notion of an integral universe.'
(Kelly, 1955, p. 20: my emphasis).

Kelly dismisses the second form of determinism as being 'relatively unimportant' (Kelly, 1955, p. 20) to his theory and discusses it only very briefly. I suggest that this judgement was basically sound with respect to the original focus and range of convenience Kelly anticipated for his theory, viz. 'human readjustment to stress' (Kelly, 1955, p. 12) and, more generally, the concerns of clinical psychotherapy. But it does not necessarily follow that this judgement of relative importance should be upheld in other foci and ranges of convenience to which PCP might be extended - a point of which Kelly was well aware (Kelly, 1955, pp. 10-12). I contend that such is the case when attempting, as in this thesis, to develop the epistemological aspect of PCP for application in formal educational settings; Kelly's second form of determinism constitutes an important part of my defence of his epistemological realism. Accordingly, I shall discuss it first and defer consideration of the other form of determinism until later in this section.
I interpret Kelly's second form of determinism - the notion of an 'integral universe' - to stem from a commitment to the meta-physical doctrine (which is applicable at least to the world of middle sized objects) that time has an "arrow", viz. that time flows in one direction only: the 'Arrow of Time Thesis'(ATT).

Thus, Kelly endorses the view that 'time provides the ultimate bond in all relationships' (Kelly, 1955, p. 6: original emphasis) and goes on to argue, by way of a principal consequence, that this results in an integral relationship between all events comprising our world. This is held to be the case however tenuous this relationship might appear to be in specific instances. Kelly proposes, as an example to demonstrate this last point, that there is ultimately even a connection between the present motion of his fingers on the keys of his typewriter (presumably typing that part of the text to his 1955 work) and 'the price of Yak milk in Tibet' (Kelly, 1955, p. 6). This second form of determinism, he tells us, 'is assumed in the so-called First Postulate of Logic, [i.e.] the Postulate of Cosmic Connectedness' (Kelly, 1955 p. 20: my emphasis).

Now, I feel that Kelly overstates his case vis à vis the relative unimportance of the second form of determinism when compared to the first and this even within his original focus and range of convenience for PCP. This is because I do not think that the first form of determinism could be sustained without the second and, further, that the second is logically prior to the first. I contend that Kelly himself, however, comes to the brink of under-mining the importance of the integral relationship existing between all events through trivialising it. For example, the nub of his justification for the arrow of time consists of the statement that
'The universe flows on and on. While one may abstract certain repetitive features in its course, it never actually doubles back on itself. Matters would become enormously confused if it ever did. (The very idea of a universe that doubled back on itself is highly amusing and might even have some relativistic significance for the cosmic theorists).'

(Kelly, 1955, p. 21: my emphasis).

Maybe Kelly had in mind something like the consequences Lewis Carroll describes in his delightful 'Wool and Water' chapter from Through the Looking Glass? I contend, however, that much more than "amusement" or even 'relativistic significance for the cosmic theorist' is at stake here, for I concur with Popper's view that the reality of time and change are fundamental to, and necessary for, any realist epistemology (Popper, e.g. 1978, p. 129) and that this has profound significance for morality.

To render this last conclusion compelling, Popper draws attention to some of the broader and undesirable implications of abandoning the arrow of time. By way of an example, Popper recalls his criticisms of Boltzmann's 'H Theorem':

'I think Boltzmann's idea is staggering in its boldness and beauty. But I also think that it is quite untenable, at least for a realist. It brands unidirectional change as an illusion. This makes the catastrophe of Hiroshima an illusion [my emphasis]. Thus is makes our world an illusion, and with it all our attempts to find out more about the world. It is therefore self-defeating (like every idealism).'


With this quotation in mind, then, I identify my own commitment to ATT and, thence, to epistemological realism, to be primarily a matter of my personal ethics. Alas, I conclude that Popper
undermines his own insight with respect to the 'catastrophe of Hiroshima' and his otherwise excellent principles of an 'Open Society' (Popper [1945] 1980a, b) by insisting upon the objectivity of his method.

Kelly uses his commitment to the second form of determinism to present in outline what amounts to a **metaphysical** argument against "ontological determinism" or (realist) inductivist epistemology. Thus, he argues that any sequence of events is **unique** due to the assumption that the universe does not "double back on itself" (i.e. ATT). From this he concludes that there is 'not much point' in singling out any particular event and claiming that it was 'determined' for 'It was a consequence'—**but only once!** (Kelly, 1955, p. 21: my emphasis).

Now, for a theory of knowledge to be consistent with the Idealist Assumption, viz. "the world is my (or our) idea", **nothing** can be assumed to be shared between persons subscribing to different views. As I intimated earlier, I find this consequence intuitively implausible. In light of my recent discussion of ATT, however, I can now develop my **epistemological** reasons for rejecting idealism/defending Kellyan realism by discussing a "thought experiment" proposed by Trigg in a section of his book entitled, appropriately enough, 'Alternative Conceptual Frameworks':

"One difficulty arising when we want to talk of similarity of belief and yet of possible variations in conceptual systems is that beliefs themselves are conditioned by conceptual systems. The belief that there is a cat on a wall presupposes that the believer thinks of cat and wall as separate entities. The tribe which views it as a single entity will have a different concept. Can we say that any belief is shared when the relevant concepts are not?"
It seems as if there has to be some neutral underpinning of them for understanding and translation to occur. I have said that there is an objective situation which we would describe as a cat on a wall and others might see as one entity. For us there can be cats not on walls, and walls without cats. We would not be disturbed if that cat jumped off that wall. The possible tribe we are considering would think an indissoluble unit had gone when the cat left the wall. Presumably they would think something else had come in its place when the wall remained. This is of course very significant. What the tribe says is constrained by what happens. The entity cat-on-wall no longer continues when the cat goes, and their description has to change when ours does. We each respond to what objectively happens and this gives the point of contact between the systems.'

(Trigg, 1980, p. 107: original emphasis).

Or in a diagrammatic form:

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(a) At time t: Western Person infers Tribe's Person
C1 = "Cat on wall"       C2 = "Cat-on-wall"
    (two entities)      (one entity)
```

...
If we agree with the anti-idealist thrust of Trigg's point, then it is important not to proceed to run aground upon the opposite bank. This is exactly what objectivist-methodological-constructivists do, for they, accepting the possibility of a point of contact between systems of belief, then hold that empirical evidence alone can constitute a neutral, objective, arbiter in matters of theory-choice (i.e. standard empiricism) providing, of course, that such evidence is mediated by the particular engine of method that they propose. This last part, however, begs the very question at issue, i.e. objectivity. From a relativist-methodological-constructivist viewpoint, if such objectivists' claim to objectivity is upheld, then they end up attributing agency to empirical evidence itself (and, thereby, to reality itself) thus undermining their claim to constructivism (CKT) and hence also their entire epistemology.
In relativist-methodological-constructivism, by contrast, theories are held to be entirely theoretical, or 'fully abstract' as Kelly might say, in the sense that there is no 'factual core' (Feyerabend, 1965, p. 203: original emphasis) which may be appealed to in acts of theory-choice. This notwithstanding, even, indeed, especially, in cases of alternative-conceptual-frameworks - incommensurable theories - the assumption of realism combined with the arrow of time thesis would seem to render epistemological realism compelling.

To explain, it seems to me that incommensurable, or genuinely alternative theories provide not only the best source of criticism and thus method for improving our theories, as Feyerabend has argued long and hard, but also, in light of Trigg's "thought experiment", the best evidence we may yet have that our preferred theory genuinely refers (to a mind-independent reality). From this I propose what I shall term a 'transformational theory of reference' whereby we can know that our theory 'refers', but not that it 'corresponds'. CKT prevents us from being more specific than this and positing, say, a 'topological' theory of reference in which we might presume identity of 'topological types' between the cosmological assumptions underlying the rival theories and the structure of reality itself: for that we would have to know the 'genus' of reality itself. The transformational theory of reference is, of course, furthest removed from the correspondence theory of truth favoured by naive realist epistemology (classical empiricism) and which might be termed topographical. I fully realise that from all objectivist points of view, the transformational theory of reference shall be construed as, at best, "toothless" due to its subjectivist element - it is a subjectivist theory of objective
reference. Considering the increased potential for critical freedom of (re)construction and for ontological responsibility that such a theory of reference admits, (about which, more later) however, the possibility of that criticism does not worry me in the least.

Turning now to (b)' - the requirement for a specified role for sense-experience in acts of theory-choice - I shall begin by considering Kelly's views on test-criteria for constructs in light of my earlier discussion of the links Feyerabend draws between cosmological assumptions and methodology. I hope that in the course of doing this I shall also be able to clarify further the transformational theory of reference.

Recall that Kelly stated concisely his views on construct test-criteria:

'[...] the criteria by which a person chooses between alternatives, in terms of which he has structured his world, are themselves cast in terms of constructions. Men not only construe their alternatives, but they construe also criteria for choosing between them.' (Kelly, 1969, p. 85).

Perhaps, however, Kelly is a little too concise for my purposes since he does not elaborate substantially upon this subject in this paper or elsewhere. This notwithstanding, certain consequences may be shown to follow more-or-less directly from his proposition. For example, if test-criteria are themselves to be construed as personal constructs, then they are necessarily superordinate constructs. To pursue this, if test-criteria are personal constructs, then they would seem each to require a dimension of relevant contrast. Or do
they? I suggest that ultimately all test-criteria may be cast in terms of "metaphysical principles": a test-criterion of prediction', for example, is surely predicated upon some notion of 'non-randomness in nature'? Indeed, I think it reasonable to say that the more highly superordinate a construct is, the more like a test-criterion or a metaphysical principle it is: any sufficiently superordinate construct is epistemology. Conversely, the more subordinate a construct is, the more truly construct-like it is. This needs more elaboration.

Recall Kelly's Range Corollary:

'Regime Corollary: A construct is convenient for the anticipation of a finite range of events only.' (Kelly, 1955, p. 68: my emphasis).

The "need" for this corollary is that a construct which included all possible elements (objects, events) would leave us with little or no advance upon the 'undifferentiated homogeneity' (Kelly, 1955, p. 9) prior to any construction. Herein lies the context-dependency of constructs. Nevertheless, the more highly superordinate a construct is, the more nearly infinite the class of abstracted elements (i.e. range of convenience) shall be. In such cases, the dimension of relevant contrast - a necessary feature of any construct - begins to break down. Kelly, however, recognizes this phenomenon through his notion of a 'Regnant Construct', viz.

'A regnant construct is a kind of superordinate construct which assigns each of its elements to a category on an all-or-none basis, as in classical logic. It tends to be non-abstractive.' (Kelly, 1955, pp. 564-565).
I construe Kelly's idea of the regnant construct to be an asymptotic notion which coincides with my understanding of a metaphysical principle, viz. a constructive principle or cosmological assumption, as discussed earlier. Regnant constructs may similarly be thought of as "ultimate test-criteria", "absolute postulates" and "values". Epting has commented in a complementary way upon the nature of regnant constructs:

'The regnant construct might be thought of as an express train that runs directly from the superordinate (value like constructs) down to the constructs that are concerned with everyday activities.' (Epting, 1984, p. 45).

Specific examples of regnant constructs in science might include simplicity, coherence and non-randomness in nature.

Now, I believe that the characteristic logical form of scientific laws (laws of nature), viz. universal statements, means that they may be construed as regnant constructs. Consider, for example, Boyle's Law for Ideal Gases. Since this law - by virtue of it being a law - is held to apply to all (ideal) gases, it is difficult fully to imagine it in terms of a construct because there would seem to be an absence of possible opposite contrast (though we may readily imagine its irrelevancy when applied, say, to solids). This apparent paradox is resolved if we re-examine part of Kelly's rationale for breaking with the traditional notion of 'concept';
Specifically, a concept 'on the occasion of its use' must be a construct. Relating this to scientific laws, we can say that whilst they may be "regnant in form", they must be "instantial" at the point of use, viz. we must have a finite number of individual elements and a dimension of relevant contrast to appraise them within.

Strictly speaking (from a Kellyan point of view), then, a scientific law – comprising, as it does, a proposition applicable to a single class of elements of potentially infinite size – is psychologically footless. But when we apply a scientific law, we operate at a lower level of superordination, viz. we operate at a level where a dimensionality of relevant contrast may be created, albeit, perhaps implicitly.

Now, Popper distinguishes between 'strictly universal' synthetic statements, to which he is committed in his epistemology and which he claims is the form of scientific theories and laws, and 'numerically universal' synthetic statements. For Popper, a strictly universal statement is an 'all-statement', i.e. a universal assertion about an unlimited number of individuals. So interpreted it clearly cannot be replaced by a conjunction of a finite number of singular statements'. (Popper [1934] 1980, p. 63: original emphasis). Numerically universal statements, by contrast, 'can, in principle, be replaced by a conjunction of singular statements; for given sufficient time, one can enumerate all the elements of the (finite)
class concerned.' (Popper, [1934] 1980c, p. 62: original emphasis). Herein lies a fundamental difference between Kelly and Popper (and all objectivist-methodological-constructivists?) for although a personal construct cannot be replaced by a conjunction of singular statements, in principle, it would be possible to enumerate all the elements comprising its range of convenience (i.e. finite class).

Scientific laws are, in Kelly's terms, 'superordinate', 'loose' and 'highly permeable' constructs.

Permeable constructs have 'more of the qualities of a theoretical formulation' (Kelly, 1955, p. 81: my emphasis) by contrast with 'hypothetical formulation[s]' (ibid.) which, in science, are 'deliberately constructed so as to be relatively impermeable and brittle, so that there can be no question about what it embraces and no doubt about it being wholly shattered if left intact at the end of an experiment' (ibid.).

I construe Kelly's meaning of a 'hypothetical formulation' closely to resemble that for his notion of 'an utterly concrete construct', viz. a construct which is 'not [ ] permeable at all' (Kelly, 1955, p. 79), by virtue of the fact that its range and focus of convenience coincide with (and are limited to) the minimum requirement of three elements for it to be a construct.

Certain of Kelly's elaborative remarks concerning his notion of an 'utterly concrete construct' suggest that it is an asymptotic notion equal and opposite to that of the regnant construct. This may crudely be represented by the following diagram:
Where lines 'A' and 'B' represent the limits to Kelly's formal theory.

Figure 4.6. Diagram Showing Construct Characteristics of Regnant Constructs and of Concrete Constructs

Many other construct(ion) characteristics may be added to those I have included in Figure 4.6, including looseness vs. tightness, dilation vs. constriction. Beyond lines 'A' and 'B' we are in the realm of 'preverbal construction'. To take account of this, Figure 4.6 may usefully be re-drawn thus:

Figure 4.7. Diagram Showing Areas Covered and Not Covered by the Formal Content of PCP
It is in the zone of 'pre-verbal construction' that we may discuss notions such as "revelation", "intuition", "tacit knowledge" and the like.

These recent views would suggest that in Kellyan meta-theory, as in (Feyerabendian) post-empiricist philosophy of science, acts of theory-choice ultimately and necessarily involve a clash of values - understood as constructive principles - and, furthermore, that it is principally values that inform any such decision. Hence the role that I propose for empirical evidence is that it serves to focus and direct attention to a relevant sub-set of competing values in acts of theory-choice. Empirical evidence itself it relatively unimportant as a criterion in any choice of theory, notwithstanding the fact that it may be the first, and constitute almost exclusively, the evidence we cite. That this last part is especially likely to be the case when appealing to another person(s) comes as no surprise. This is because sense-experience constitutes an experience, perhaps the only experience, that we can reasonably predict that we might share something with another person. What the relativist-methodological-constructivist refrains from doing however, is equating empirical evidence with this small, and otherwise unknown, "something" within their epistemology. (This does not deny the possibility that, on occasion, "crucial" tests or experiments may be negotiated by persons subscribing to different values).

The importance of empirical evidence in acts of theory-choice is thus somewhat paradoxical: on the one hand, it seems to be very important, even necessary, as a means of "identifying" or "evoking" relevant values; on the other hand, it seems very unimportant.
relative to values — empirical evidence itself is the first casualty amongst criteria.

Values are implicit to all our theories and formalisms. We can invite, but we can never be justified to demand, acceptance of the existence of "objective values", i.e., values that are universally held. Whilst our theories and formalisms may help us to know and to revise our values, they should never be mistaken for values themselves. There can be no "objective" expression of values, for values really are the bottom-line: this is the most important limit of formalism. Hence, relativist-methodological-constructivism endorses the third equivalence postulate of Barnes and Bloor: 'all beliefs are on a par with one another with respect to the causes of their credibility.'

I suggest, then, that the role for empirical evidence in acts of theory-choice is analogous to icons in traditions of religion. Just as a lamb with a flag is a simple problem in Christian iconography, (Murray and Murray, 1972, p. 214), so too are certain arrays of tracks in a cloud chamber. Or less directly, Michaelson and Morley's aether experiments, Eddington's lunar eclipse observations and the Chesapeake Bay atomic clock experiments (among others) may be said to have become an "icon" of Relativity Theory. I shall term this role the 'Icon Theory of Empirical Evidence'.

Now, as we have seen, personal meaning in relativist-methodological-constructivism is held to be achieved by the meaning-maker positing a dimension of relevant contrast. This, however, is a formalism: it says little of the nature of the actual experience involved, especially in the early stages of developing a personal meaning.
To elaborate, then, personal meaning may be understood to consist of the “apprehension” (whether intuitive and implicit or formalised and explicit) of some kind of relationship between one's values and one's "life experiences".

Now, consideration of another person's idea constitutes an important example of life experience for most persons. When a person apprehends a relation of sameness or similarity between their personal values and those implicit to the idea of the other person, agreement shall be the probable result (notwithstanding possible disputations over details of formalisation). When a person apprehends a relation of difference or dissimilarity between their personal values and those implicit to the idea of the other person, disagreement shall be the probable result. When no relation is apprehended the other's idea shall be construed as meaningless, as, e.g., "just so many words". It should be noted that as a doctrine of meaning this could scarcely be less dogmatic for, methodologically speaking, it allows that, at most, we can tentatively identify "value clusters" associated with different theories and activities and then invite other persons to share them with us.

Personal values may change (be revised) and theorising may play an important role in mediating such change, but empirical evidence is neither a necessary nor a sufficient condition for meaning. In practice, however, the derivation of personal meaning from another person's idea relies very much upon the ability to derive an empirically knowable consequence (though one is usually cited by the proposer). In general, the more discrete and accessible the empirical consequence is, the easier it is to derive personal
meaning from another person's idea. Where such empirical consequences are absent, or are otherwise unclear, vastly more effort may be required—often more that a person is able or willing to give: as I suggested earlier, sense-experience possibly represents the only experience where we can be reasonably sure, and sure in advance of our efforts to comprehend, that we might achieve at least some point of contact between our belief system and that of another person. (The specified consequential or evidential status of an empirical event, whether predicted or instantiated, is always open to doubt and re-interpretation due to the Theory Ladenness Thesis).

This is why comprehension of certain traditions such as theoretical physics, or (especially) pure mathematics and religion, is so notoriously difficult, is "esoteric": it is precisely such traditions which lack clear, accessible, (empirical) "icons" and hence require more "value awareness"—and value similarity—in the initial stages of attempted comprehension.

Knowing sameness or similarity between one's personal values and those of another person may be a very intuitive affair. We often experience it all the same. This is what happens when we unaccountably, but unmistakably, "click" with some persons and, equally, do not with other persons. We may not be able to account to others, or even to ourselves, as to why we like or dislike a certain other person, we cannot "put our finger on why", i.e. we cannot find an appropriate "icon" to evoke in others, or even in ourselves, our relevant set of personal values ("feelings") at work: sometimes every piece of empirical evidence that we try in this role somehow seems to be inadequate.
The examples I have recently presented have tended to focus upon persons' personal visions of the ultimate nature of the physical universe (whether consciously or unconsciously held and whether explicitly or implicitly stated). Earlier on, however, I asserted that whilst values may be distinguished, both one from another and from theories, they can never be entirely separated in either respect. This also has the consequence that personal ethics and (in societies) politics can never be entirely separated from any act of theorising, e.g. in science, in pedagogy.

In relativist-methodological-constructivism, then, 'is' statements are not regarded as categorically separate or separable from 'ought' statements. Similarly, "social laws" and "natural laws" are not considered to be essentially different in kind, notwithstanding vast possible differences in degree. Again, the 'context of discovery' is held not to be independent from the 'context of justification'. These conclusions may be summarized by saying that to a large, though not entire, extent, where an 'ought' was, an 'is' shall be and that this ultimately follows from the view that our minds are not only a part of nature but also always the most pertinent part.

Now, although I have been highly critical of objectivist epistemologists, I do not wish to call into question the good intentions of all proposers of such. Popper is a case in point for somewhere he defends his commitment to (and the general desirability of) 'objective method' on the grounds that any 'irrationalist' alternative leaves the door open for, amongst other things, racist interpretations and criticisms of scientific theories. There is a prima facie credence to his claim because (to take Popper's example) in Nazi Germany some physicists, and many
non-physicists, criticized (especially) Einstein's theory in regard of its alleged "Jewishness" and of being "Jewish physics" (cf. Clark, 1973, p. 494 who cites some spectacular, if disturbing, examples).

Alas, Popper's noble aim is undermined by the history of objectivist epistemology for such 'methodolatory' (Bakan's, 1969, p. 158, excellent expression) can be turned to any purpose. In support of this, I point out that Mengele and his co-workers committed most of their very worst atrocities against inmates of Auschwitz in the name of objectivity, viz. amongst other things, their explicit remit was to seek and find 'objective', 'scientific', proof of the correctness of the doctrine of the Aryan Race.

Objectivism in epistemology is culpable because it sanctions, indeed, requires that person's (or persons') specific purposes and criteria in the making and appraising of a knowledge claim, and which cannot interpretatively be subsumed by the objectivist's method, be excluded from consideration. In this way, science and the philosophical evaluation of science become divorced from the social context and hence also from social responsibility. Given my earlier arguments, I conclude that there is something inherently contradictory - and ultimately disingenuous - about "valuing objectivity". The appeal of the objectivist approach is, however, quite understandable for the possibility of objective knowledge promises also the possibility of an unimpeachable vindication of one's "personal" views.
Now, practically speaking, I suspect that there are few persons indeed whose commitment to objectivity consistently exceeds their other commitments. My fear, nevertheless, is that with what I consider to be the illusion of objective epistemology, and hence the possibility of objective knowledge, before scientists and general public alike, (especially politicians) and human ideational fecundity being what it is, there would almost always be so many ideas proposed that would be capable of meeting the pre-testing requirements of an objectivist epistemology that inter-subjective moral appraisal (ethical and political) shall only be brought to bear upon those objectivistically formulated knowledge claims which have also been objectivistically tested. Indeed, in objectivist-methodological-constructivist epistemologies, such as Popper's, some degree of (successful) empirical testing is ultimately required to render the theory of assuredly 'scientific' status, as we have seen. But surely this is to put the cart before the horse, for by then it may be fatally too late? The only 'degree' of empirical testing for the atom-bomb, for example, was the "absolute" degree of actually exploding it: the critical mass required for such a weapon ruled out "small scale experiments". If a person's, or a society's commitment to objectivism is over-arching, then this shall necessarily delay, perhaps even indefinitely, their personal and/or societal moral appraisal of any action.

I contend that even modern, "sophisticated", objectivist epistemologists end up by sanctioning a morally blind (which is not to say neutral) approach to knowledge creation. This may be summarized by one of Bacon's famous dictums:

- 4.111 -
'The end of our Foundation is the knowledge of causes and secret motions of things, and the enlarging of the bounds of Human Empire, to the effecting of all things possible.'

Now, Easlea (1983) argues compellingly, albeit in a self-consciously ptolemical style (Easlea, personal communication), that gender is an important dimension for understanding the creation of nuclear weapons and the perpetration of the (nuclear) arms race in particular, and the dominant historical orientation of the development of scientific knowledge in general.

With close reference to feminist intellectuals such as Simone de Beauvoir and using Mary Shelley's novel Frankenstein or the New Prometheus as a sub-text, Easlea identifies and critically examines the prevalence of birth and sexual metaphors in the (predominantly) male accounts of science. Easlea has elsewhere summarized his argument thus:

'My point of view [ ] is that in patriarchal society, and particularly in advanced capitalist industrial societies, men tend to suffer in varying degrees from nearly zero to nearly total from what I call a 'compulsive masculinity syndrome'. I also claim that in its near total form this syndrome manifests itself in a pathological striving for power and domination together with a pathological hostility towards what is perceived as 'feminine'. My general argument is that abolition of the sexual division of labour would do a great deal to reduce the pervasiveness and intensity of the 'compulsive masculinity syndrome' and hence make more likely and more sustainable a just and peaceful society.'
(Easlea, 1985, p. 17).
The Baconian mandate - 'to the effecting of all things possible' - emerges from Easlea's treatise as just one manifestation of the 'truly masculine birth of time' (Bacon, quoted by Easlea, 1983, p. 19) which Bacon explicitly initiated and promoted and which he identified with science. It is a dimension which is perpetuated by objectivist philosophers of science, such as Popper, and individual scientists, such as Teller ("Father" of the H-Bomb). I shall discuss Easlea's thesis in the context of an interview with a young student scientist (section 9.3).

In complementary vein, Maxwell argues for the need to put into practice 'a profound and comprehensive intellectual revolution, affecting to a greater or lesser extent all branches of scientific and technological research, scholarship and education.' (Maxwell, 1984, p. v). He argues that the realization of such a revolution would bring about a shift in the aims and methods of rational enquiry from the relatively modern tradition of what he terms the 'philosophy of knowledge' in preference for the 'philosophy of wisdom' and which has antecedents in the approach of the historic Socrates.

Maxwell characterizes 'wisdom' as 'the desire, the active endeavour, and the capacity to discover and achieve what is desirable and of value in life, both for oneself and for others.' (Maxwell, 1984, p. 66). Wisdom includes knowledge and understanding but goes beyond them. Wisdom, like knowledge, can be conceived of, not only in personal terms, but also in institutional or social terms. Accordingly, the philosophy of wisdom enjoins that 'the basic task of rational enquiry is to help us develop wiser ways of living, wiser institutions, customs and social relations, a wiser world.'
Maxwell argues that the standard empiricist view qua philosophy of knowledge, 'seriously misrepresents' the true intellectual aim of science, viz.

'The aim of science is not merely to discover truth per se, nothing being presupposed about the nature of the truth to be discovered. A basic aim of science is to improve our understanding of the world. Science seeks explanatory truth.' (Maxwell, 1984, p. 96: original emphasis).

Whilst I am most sympathetic to Maxwell's main thesis, I feel that he 'seriously misrepresents' both Feyerabend and Kelly in advancing his case.

From these recent ideas, it would seem that in creating and appraising theories one must attempt - and be willing - to "lay one's personal values on the line", amongst other things. This, however, may involve great personal risk, for one's personal values may run into direct conflict with those of another person(s). Furthermore, one's personal values may come to be rejected - and not only by another person(s), but also by oneself. The willingness to entertain such risks, however, is a necessary, though not a sufficient, condition for true personal responsibility ever to be achieved.

Now, personal responsibility can be assumed only if a person has first identified what their values are. This is often not an easy task and it is not helped by the fact that there is little tradition for it in Western civilisations and which might help. I contend that the long, and still pervasive, influence of 'British
Empiricism has actually stunted the development of techniques for ordinary (i.e. non-philosophers) persons to "get in touch" with their values. Modern objectivist theories of knowledge, however, simply perpetuate rationales for evading personal responsibility on the past of those who propose and/or use them.

At this point it may be timely both to elaborate upon my earlier point concerning personal risk and to relate it to (and in counter-critique of) an "extra formal" criticism of relativist epistemology, viz. I conjecture that the aspect of risk involved in taking personal responsibility that is most unpalatable amongst critics of Relativism is the possibility that having succeeded in "finding out" one's relevant set of values one may not approve of what one has found!

I suggest that many persons have some uncomfortable inklings that this might, indeed, be the case when they argue against Relativism. Feyerabend has made some remarks which are most pertinent and complementary to this notion:

'Relativism if often attacked not because one has found a fault, but because one is afraid of it. Intellectuals are afraid of it because relativism threatens their role in society just as the enlightenment once threatened the existence of priests and theologians. And the general public tyrannized by intellectuals has learned long ago to identify relativism with (social) decay. This is how relativism was attacked in Germany's Third Reich, this is how it is attacked again today by Fascists, Marxists, Critical Rationalists. Even the most tolerant people dare not say that they reject an idea or a way of life because they don't like it - which would put the blame on them entirely - they have to add that there are objective [original emphasis] reasons for their action - which puts at least part
of the blame on the thing rejected and on those enamoured by it. What is it about relativism that seems to put the fear of god into everyone?

It is the realization that one's most cherished point of view may turn out to be just one of many ways of arranging life, important to those brought up in a corresponding tradition, utterly uninteresting and perhaps even a hinderance to others [ ]. Tolerance does not mean acceptance of falsehood side by side with truth, it means human treatment of those unfortunately caught in falsehood. Relativism would put an end to this comfortable exercise in superiority — therefore the aversion.' (Feyerabend, 1978, pp. 79-80: my emphasis).

Now, when conjoined with the incommensurability thesis, the arrow of time thesis and the transformational theory of reference, Kelly's notion of the construct may be said to allow us legitimately to have it both ways with regard to realism and idealism: a construct may be said to be realist in conceptualization, but instrumentalist in application. Our commitment in any particular application to only one pole of a construct but combined with dimensionality and the three augmentative theses above allows and ensures our 'ontological responsibility', viz. acceptance of personal responsibility for the entities one claims to exist. The moral aspect of such responsibility, however, resides not in acceptance of one's personal act of construction of such entities (God, atom etc) but of personal choice. Were is possible to insist on acknowledgement of personal construction as a criterion for personal morality in PCP, then this would be pre-emptive. For example, I, as an agnostic, can only anticipate construing God as a personal construct, but constructs with a high degree of commonality amongst theists, such as 'God vs. Devil', are presently irrelevant if not totally meaningless to me.
We are always free to choose and we are always free to re-construe. Responsibility then fuses with Kelly's first 'form' of determinism, viz. 'the control of a superordinate construct over its elements' (Kelly, 1955, p. 20), and which he elaborates upon in the following way:

'It should now become clear what is not determined. For one thing, an element does not determine the constructs which are used to subsume it; for another, an element which falls outside the purview of a construct is independent of it. The latter type of independence or freedom is relatively unimportant to us; it is only the freedom of chaos. The former type of independence or freedom is highly significant, for it implies that man, to the extent that he is able to construe his circumstances, can find himself freedom from their domination. It implies also that man can enslave himself with his own ideas and then win his freedom again by reconstruing his life.

One thing more: since determinism characterizes the control that a construct exercises over its subordinate elements, freedom characterizes its independence of those elements. Determinism and freedom are thus inseparable, for that which determines the other is, by the same token, free from the other. Determinism and freedom are opposite sides of the same coin - two aspects of the same relationship.'


Constructive Alternativism may thus be seen to emerge as a philosophical stance which celebrates persons' ability to force distinctions but upholds a principled rejection of absolute separations. It is not that Kelly denies that absolute separations can and do exist; but when they do, then they lie outside the range of possible experience. In PCP, we are behoven to accept ontological responsibility for our choices and to achieve relevant contrasts. In social contexts we should also strive to articulate
and to communicate them. As Bannister puts it 'we ought not to proclaim publicly that which has not personal meaning for us' (Bannister, 1979, p. 28). Whereof one cannot construe, thereof one should remain silent. In general summary of the arguments of this chapter so far, I propose the following syllogism:

All observations are theory-laden;
All theories are value-laden;
Therefore, all observations are value-laden.

What general consequences might these values and arguments have for the conduct of science? I am sympathetic to Carl Friedritch's speculation that

'All we can really ask of the individual is that he pay careful and scrupulous attention to the wider framework into which all scientific and technical progress must fit, even when this does not seem to further his immediate interests.' (Friedrich, speaking in conversation soon after having received news of the atomic attacks on Hiroshima and Nagasaki; remarks attributed by Heisenberg, 1971, p. 196).

Now, I suggest that objectivist epistemology would undermine this admirable exhortation in an unacceptably high proportion of cases - and where it did not this would be fortunate coincidence. Relativist-methodological-constructivism, by contrast, could greatly facilitate implementation of such a proposal. Kelly has commented pertinently upon the issue of science policy:
'The use of the construct rather than the concept as a unit opens for the scientist a door to quite a different line of thinking. It enables him to regard science, along with other modes of endeavour, as a system of ventures open to psychological study, quite apart from their material referents and outcomes. More than this, it turns our attention to the matter of relevance, a matter that has aroused the special concerns of both the Vienna Circle and of the critics of the atom bomb.'
(Kelly, 1969, p. 11).

And with closer reference to the formal content of PCP, Kelly argues that

'The construct dimension that lends structure to the behavioural event does so by providing contrasting poles, and unless we take the trouble to explicate both of them the directional trend of the behaviour we have observed cannot be plotted.'
(Kelly, 1969, p. 12: my emphasis).

From this we may infer that the process of achieving social responsibility and relevance of science would begin with the individual scientist who would strive to make explicit to themself exactly which relevant contrast is at work in their intended application of their theory. The matter would not rest there, however, for social responsibility and relevance could only be ensured if appropriate interpersonal, institutionalised, practices were created and maintained. To elaborate, ideally, science policy (including resource allocation) would be decided only where an opposite contrast had been able to have been created and had been defended. Given that our experienced commitment in any particular application, or intended application, of a theory is only to one pole, however, the defence of the contrast pole should be undertaken by another person. There should, in other words, be a "tender for
contrast poles" and a debate prior to deciding research policy. Even this would not be sufficient, however, for the frequent homogeneity of scientists' values in some areas of science may be such that any decision would be prejudiced from the start. The search for a contrast pole would therefore have to be open and extended to traditions other than science.

This begs the question of who is to decide science policy? I agree with Feyerabend's radical answer to it:

'[ ] fundamental debates between traditions are debates between laymen which can and should be settled by no higher authority than again the authority of laymen, i.e. democratic councils.' (Feyerabend, 1981b, p. 32: original emphasis).

This stands in radical contrast to present practice and, indeed, I suggest that objectivist epistemology has been used as a device for (amongst other things) restricting or denying public executive involvement in science policy. Relativist-methodological-constructivism would help put an end to this practice. This, however, broadens the scope to embrace politics and, indeed, Feyerabend's recommendation for 'democratic councils' stems from his notion of a 'free society' based upon 'democratic relativism':

'A free society is a society in which all traditions have equal rights and equal access to the centre of power (this differs from the customary definition where individuals have equal rights of access to positions defined by a special tradition - the tradition of Western Science and Rationalism [as per, e.g. Popper's 'Open Society']).' (Feyerabend, 1978, p. 9: original emphasis).
I shall not discuss the details of Feyerabend's vision of a 'free society' (for which see especially Feyerabend, 1978 - some elements from which I have, in any case, already presented) beyond saying that his ideas on this subject are explicitly in the tradition of radical liberalism originating from J. S. Mill and that I regard them to be wholly compatible with the tenets and ethos of PCP. I would also suggest that a complementary economic theory is at hand in the works of Ernst Schumacher (see, especially, Schumacher, 1973) and those inspired by him (e.g. Kumar, (ed.) 1982).

Clearly the open and democratic nature of science conducted in this manner is at odds with our present society which employs over 40% of its physicists (and similarly large proportions of scientists from the other branches of science) in "classified", i.e. military, research. If the prognosis for a 'free society', as outlined above, seems alarming and unlikely, then it should be stressed that the profound intellectual revolution sought would not first require political revolution: Feyerabend claims that the necessary institutions for bringing it about 'already exist' and that 'The question is therefore not how to construct such a machinery, the question is how to loosen it up and to detach it from the traditions that are now using it exclusively for their purposes; for example, how to separate state and science.' (Feyerabend, 1981b, p. 33: original emphasis). He argues that such "loosening up" should not come 'from above', e.g. be imposed by a 'gang of radical intellectuals', but, rather, should come 'from within', i.e. 'citizens' initiatives', and the rate of change should be that which they, collectively, decide (ibid.). 'Citizens' initiatives' discussed or mentioned by Feyerabend include ecological, peace, consumer, and women's groups. Maxwell (1984, Chapter 11) proclaims
that 'the revolution is underway' and cites similar sources. The impact or effectiveness of such initiatives upon the conduct of science may, indeed, be inferred from such occurrences as the moratorium on recombinant DNA research in the mid 70's, the 25% reduction in UK funding of high energy physics in favour of the life sciences and the increased vigour with which august bodies such as the Royal Society are seeking to re-establish or increase the general public's valuation ('understanding') of science in the face of such reverses (e.g. The Royal Society, 1985).

Finally, I point out that Feyerabend's maxim - 'Citizens' initiatives instead of philosophy!' (Feyerabend, 1981b, p. 33: original emphasis) - is reflexive with respect to philosophers, viz. they are citizens too (though not more so), and as such they still have a role to play in proposing 'initiatives'. In the remaining chapters, then, I shall attempt to show that the values and arguments of relativist-methodological-constructivism are plausible and desirable in formal educational settings by showing that to a considerable extent they are anticipated and already realized (if not always recognized) in the work of the Alternative Conceptions Movement in educational research.
Notes to Chapter 4

n. 1. I am here alluding to Bacon's dictum 'Eliminate prejudice!' - a term he used synonymously with that of metaphysics.

n. 2. In the early years after 1905, Einstein made many similar remarks which were also embraced by positivists. His discussion of the concept of simultaneity, in particular, was given an 'operationalist' interpretation by the physicist and philosopher of science P. W. Bridgeman in publications dating from 1927.

Popper describes the philosophical influence of Einstein's early statements of his epistemological views and also the profound change as he underwent (at least in his expressed views) as he became progressively committed to epistemological realism:

'It is an interesting fact that Einstein himself was for years a dogmatic positivist and operationalist. He later rejected this interpretation: he told me in 1950 that he regretted no mistake he ever made as much as this mistake. The mistake assumed a really serious form in his popular book, Relativity: The Special and General Theory. There he says [ ]: 'I would ask the reader not to proceed further until he is fully convinced on this point'. The point is, briefly, that "simultaneity" must be defined - and defined in an operational way - since otherwise 'I allow myself to be deceived... when I imagine that I am able to attach a meaning to the statement of simultaneity.' Or in other words, a term has to be operationally defined or else it is meaningless. (Here in a nutshell is the positivism later developed by the Vienna Circle under the influence of Wittgenstein's Tractatus, and in a very dogmatic form.) (Popper, 1978, pp. 96-97: original emphasis).

Einstein's realism eventually led to his rejection of the quantum theory - a theory he had helped to create - because he could not accept the notions of uncertainty and indeterminism which came to be seen as intrinsic to it in the Copenhagen Interpretation. Pagels (1982, Part I, Chapter 1) has described Einstein as 'the last classical physicist'.

n. 3. Mach had described his epistemology as 'neutral monism' and Carnap went on to describe him as 'methodological solipsism' (see Suppe, 1974, I).

n. 4. Suppe makes the interesting point that

'That the Received View survived so long after logical positivism had been rejected initially seems rather surprising. The explanation of this lies, I think, in the fact that positivism unreasonably had tried to force all empirical knowledge into the scientific
mold, and many who rejected positivism as a general
epistemology did so on the grounds that not all
empirical knowledge was like scientific knowledge;
thus in rejecting logical positivism as a general
epistemology, they were willing to concede that
positivism was adequate as an analysis of scientific
knowledge [.]. Logical positivism thus became
philosophy of science, and continued to survive as a
philosophy dealing with a restricted range of
empirical knowledge – scientific knowledge.
(Suppe, 1974, n. 7, p. 6).

n. 5. I have noticed a recurrent confusion in the educational
literature concerning the direction of theory 'reduction'. This
stems, I believe, from Nagel's use of the terms 'primary science'
and 'secondary science'. The secondary science is (usually)
temporarily prior to the primary science. In any event, however, it
is the primary science that is the 'reducing science': the
secondary science is 'reduced' by (or 'to') the primary science in
the sense that it is held to be logically derivable from the primary
science which encompasses a larger class of phenomena.

n. 6. Whist Laudan's arguments against R5 are clear and fairly
detailed, I can summarize my own reasons for rejecting R5 by
quoting, once again, Hughes comment that 'ontological and
epistemological questions are not to be answered by empirical
inquiry since they are concerned with, among other things, the
nature and significance of empirical inquiry.' (Hughes, 1980, p.7).

n. 7. Of. my characterization of 'radical relativity of knowledge',
below, to which I interpret Kelly (and Feyerabend) to subscribe.

n. 8. The matter is complicated by his metaphysical commitment to
the notion of 'simplicity' in his later works: this undermines his
objectivist-empiricist commitment to the view that empirical
considerations alone are to dictate the choice of theory (see

n. 9. I have described Popper as being under the 'spell' of
deductivism because he seems to see nothing unfair in his
criticizing inductive reasoning for its lack of (possible)
jjustification, on the one hand, but yet proclaiming the arbitrariness
of adopting his deductive methodology on the other hand (see Popper,
1966b, p. 231 quoted below).

n. 10. Notwithstanding his attempts to counter this criticism by
means of 'methodological falsificationism' (cf. my discussion,
section 3.4.2).

n. 11. Feyerabend would seem to have been trying to be a "good
Popperian" in his early forays into the thesis for his concern was
almost exclusively with increasing the testability of theories
(without regard for their meaning). Kuhn, his own person right from
the start, has done much to bring to light the truly radical
elements always within Feyerabend's work.
n. 12. I have not been able to decide where Papineau himself stands upon this issue.

n. 13 Feyerabend goes on to employ essentially the same technique with scientific theories.

n. 14. In conversation with Bohr and other around 1926-7, Schrodinger, himself a realist, remarked that

'If all this damned quantum jumping were really here to stay, I should be sorry I ever got involved with quantum theory.'

(Schrodinger, remark attributed by Heisenberg, 1971, p. 75).

n. 15. I would not wish to imply that Einstein is either orthodox or Popperian by my repeated use of his remark. He also stated, for example, that

'No sooner has the epistemologist, who is seeking a clear system, fought his way through such a system, than he is inclined to interpret the thought content of science in the sense of his system, and to reject whatever does not fit into his system. The scientist, however, cannot afford to carry his striving for epistemological systematic that far... the external conditions which are set for him by the facts of experience, do not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. 'He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist.'


n. 16. Feyerabend had elsewhere commented that

More than one social scientist has pointed out to me that [having read Kuhn] now at last he had learned how to turn his field into a 'science' - by which of course he meant that he had learned how to improve it. The recipe, according to these people, is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal science that had this one theory as its paradigm. Students must be prevented from speculating along different lines and the more restless colleagues must be made to conform and 'to do serious work'. Is this what Kuhn wants to achieve?'

(Feyerabend, 1970, p. 198: original emphasis).

I was deeply depressed to hear that at the 6th International Congress on Personal Construct Psychology last year, Professor Peter Stringer - himself a respected personal constructivist - was complaining (as I interpret the report) about those who were
criticizing, re-construing and otherwise diluting the orthodoxy of the Master as per 1955: he specifically cited Holland (1970).

Now, I cannot imagine a complaint being more at variance with the "spirit" of Kelly's work - or more in keeping with Kuhn's doctrine of normal science. In the article in question, Holland did no more than take Kelly at his word, albeit, brilliantly.

n. 17. This notion of a 'metaphysical research programme' was given a new and scientific lease of life within Lakatos' 'methodology of scientific research programmes' (Lakatos, 1970), as Lakatos acknowledges and Popper points out in these respective works.

n. 18. Cf. Lakatos' (1970) notion of a 'negative heuristic'.

n. 19. It is also at variance with those who might be called "consensualists" of knowledge, such as Kuhn, though without undermining them.

n. 20. See, for example, Sylvan's (1985) critique of 'Deep Ecology' and Low's (1982) critique of 'General Systems Theory' - the latter is written from a PCP perspective.

n. 21. Were a theory of truth to play a role in PCP (or, more generally, in relativist-methodological-constructivism), then I suggest that it would be along the lines of what is now called the 'adversary theory of truth' developed by J. S. Mill (see Himmelfarb, 1974, p. 25) and which I suggest is encapsulated in the following line from On Liberty: 'He who know only his own side of the case knows little of that.' (Mill, 1977, p. 45). Cf. Feyerabend's claim that 'prejudices are found by contrast, not by analysis.' (Feyerabend, 1975b, p. 31).

n. 22. The technical details of Boltzmann's H. Theorem need not concern us beyond saying that it is a probabilistic derivation of the second law of thermodynamics from the kinetic theory - a derivation which has the cosmological implication that the objective "universe" is completely symmetrical with respect to the two directions of time.

n. 23. Feyerabend characterizes an incommensurable theory as the 'strongest' amongst a battery of 'alternatives' (Feyerabend, [1965] 1981a, p. 109 et seq.). My somewhat simplified distinction of 'genuinely alternative' vs. 'trivially different' is due to the pedagogic use I intend to make of it in Chapter 5.

n. 24. Cf. their place within my model for the transformation of scientific knowledge in educational settings section 2.4.1.

n. 25. Cf. my discussion of Popper's detailed meaning of a universal statement, shortly below.

n. 26. The 'historic Socrates', or what we know of the sayings of the real Socrates, as opposed to the Socrates of Plato as presented in his Meno and Phaedo.
n. 27. Thus, Maxwell argues that

'Kelly, gripped by the philosophy of knowledge, is obliged to interpret the personal inquirer as a sort of scientist, seeking knowledge. Personal construct theory then itself seeks to develop academic psychological knowledge about the knowledge acquiring, or construct building, endeavours of individuals.'
(Maxwell, 1984, p. 143).

The pursuit of merely 'academic' psychological knowledge could not be further from Kelly's stated aims and purposes for his theory and, moreover, is a charge that cannot be made to stick if the formal content of his theory (as opposed to his slogan 'Man-the-Scientist') is examined. The beauty of Kelly's theory is that it provides us with some concrete but non-pre-emptive techniques for pursuing and implementing 'aim oriented rationality'—unlike Maxwell.

Against Feyerabend, Maxwell argues (for example) that:

'Scientists, historians, philosophers and sociologists of science have all been too quick to identify rational inquiry with science, and the success of science with the adoption of some version of standard empiricism, so that an attached on standard empiricism is interpreted as an attack on science itself, and reason itself! Even Feyerabend, the licensed court jester of orthodoxy, in effect also makes these elementary mistakes, in that his challenge to orthodoxy takes the predictable form of romantic irrationalism or, as he calls it, methodological anarchism. If standard empiricism must be rejected, Feyerabend in effect presumes along with his opponents, then reason itself must be rejected'.
(Maxwell, 1984, p. 36).

On the contrary, Feyerabend does not reject reason; rather, he rejects a certain kind of Rationalism—the objectivist, empiricist, monistic, comprehensive, systematic kind. I suspect that Maxwell, like so many of Feyerabend's critics, has mistaken Feyerabend's use of reductiones ad absurdum for direct arguments. The 'elementary mistakes' are therefore Maxwell's

n. 28. Sufficiency requires acceptance of personal choice in dimensionality of knowledge claims—see my discussion, below.

n. 29. There are close parallels between what I am suggesting here and Chalmers' notion of 'unrepresentative realism':

'Unrepresentative realism is realist in two senses. Firstly, it involves the assumption that the physical world is the way it is independently of our knowledge of it. The world is the way it is whatever individuals or group of individuals may think about the matter. Secondly, it is realist because it
involves the assumption that, to the extent that theories are applicable to the world, they are always so applicable, inside and outside of experimental situations. Physical theories do more than make claims about correlations between sets of observation statements. Unrepresentative realism is unrepresentative insofar as it does not incorporate a correspondence theory of truth. The unrepresentative realist does not assume that our theories describe entities in the world, such as wave functions or fields, in the way that our common sense ideas understand our language to describe cats and tables. We can appraise our theories from the point of view of the extent to which they successfully come to grips with some aspect of the world, but we cannot go further to appraise them from the point of view of the extent to which they describe the world as it really is, simply because we do not have access to the world independently of our theories in a way that would enable us to assess the adequacy of those descriptions.' (Chalmers, 1982, p. 163: original emphasis).

n. 30. This notwithstanding, constructs such as 'God vs. Devil', Good vs. Evil' and so on are all great improvements on the psychologically footless formulations such as 'Moral vs. Immoral' (whatever that means) currently favoured by our now largely atheistic society precisely because they are constructs. This is to be expected because religions are self-professedly moral enterprises. If meanings for such religious constructs seem elusive (as they are to me), then I suggest that this would not be so were the reader already committed to a theistic (or Christian) ontology. Moreover, many constructs proposed by religious leaders do have personal meaning and utility for me: for example, I am most sympathetic to Dr. David Jenkins' (the Bishop of Durham) arguments for 'conflictual collaboration', as contrasted with 'compromising consensus', as a means of effecting social policy (Jenkins, quoted by Margison, 1986, p. 150: original emphasis).

My earlier remarks should not be taken to excuse or deny that crimes against humanity have frequently been committed in the name of religion - I have already alluded via a Kelly quotation to the witchcraze. I account for this by suggesting that amongst persons claiming to be religious, there have been all too few who have been able or willing to implement their beliefs in a way even barely resembling the ontologically responsible approach of, say, Sir Thomas More (at least, as he is portrayed by Bolt, 1960). Most usually, I suppose that such crimes have had nothing whatever to do with the personal utilities of faith.

n. 31. This is a point on which Kelly has frequently been misunderstood (see Bannister's, 1979, counter-critique of Skelton-Robinson) but I would not like to be too dogmatic about it. If we exmplain the tragic fact that the vast majority of concentration camp victims literally walked quite knowingly to their
deaths by invoking titanic acts of re-construction on their part, then we should counter-balance this by considering that there may be minimum physiological and perhaps even psychological antecedents for further (re)construction to take place.

n. 32. This proposal may seem less outlandish if, in addition to reading Feyerabend's own detailed arguments, the reader consults Perry (1965) who argues that 'commonsense' thought, knowledge and judgment should neither be regarded as 'the touchstone of knowledge and the arbiter of reason, [n]or for that it is crude, primitive, and finally to be superseded.' (P. 125). Commonsense has a relevance and applicability which expert or specializes knowledge can never have - but they serve different functions. Such views inform my treatment of the fate of students' alternative-conceptions in a 'free society' (cf. discussion below, also in Chapter 10).

Chapter 5. Personal Construct Psychology and the Alternative Conceptions Movement in Educational Research

'The opposite of a correct statement is a false statement, but the opposite of a profound truth may be another profound truth'.
(Bohr, oral comment, attributed by Heisenberg, 1971, p.102).

5.1 Introduction

My prime purpose in this chapter is to make the case that the classroom research of the 'Alternative Conceptions Movement' (ACM) (Gilbert and Swift, 1985) now commands the status of a delineable research tradition or programme by virtue of its distinctive value commitments as complemented and reflected by its central meta-theoretical notions and by its characteristic research methods. (I shall defer consideration of a compatible theory of teaching until Chapter 10). An intrinsic part of my argument is that these aspects of the ACM are compatible, indeed, in many areas, isomorphous, with the Kellyan and the Feyerabendian brand of relativist-methodological-constructivism which I have argued for in the previous two chapters.

In an attempt to keep closely to this aim, I shall not discuss or summarise the classroom interpretations of ACM research directly - this has already been done both admirably and extensively by others (for references, see Chapter 1, also below). Rather, I shall concentrate upon presenting meta-theoretical and methodological issues as they are discussed in accounts of such field-research and in the growing numbers of
papers which seek to articulate commonalities across these studies considered as a whole. By so doing, I hope also to present a counter-critique of the "critical backlash" now facing the ACM and which I mentioned in Chapter 1.

In my account of ACM meta-theory, I shall try to preserve something of its chronology in development, but, in the interests of brevity, I shall not attempt to be comprehensive or systematic in drawing links between it and the ideas of Kelly and Feyerabend, relying upon the reader to discern many of the commonalities for themself in the light of my earlier discussions.

Another purpose for this chapter is to articulate and to develop the relation between ACM meta-theory, as I have interpreted it and as it has most usually been applied in investigations of alternative-conceptions in science, and my own classroom research into alternative-conceptions of service (described in chapters 6, 7, 8 and 9). By way of further context for my enquiries in the field, I shall also present a selective review of previous research into educands' and educators' personal epistemologies of science.

5.2 The Alternative Conceptions Movement as a Delineable Research Tradition

A perennial problem in the field of science education may be encapsulated by the question 'why do so many school students experience so much difficulty understanding the conceptual content in our science courses?'
I suggest, however, that co-extensive with educators' and educationalists' concern with this problem there has also been a general recognition that recurrent themes exist amongst such difficulties in understanding and that these occur both within and between educands.

Now, working from a model of learner as passive receiver of information ('tabula rasa') the traditional pedagogic response to learner difficulties has been to emphasise "gaps" in the learners' knowledge and which then lead to "systematic mistakes". The teaching solution to such "mistakes" was seen to require first uncovering where such "gaps" were, and then "filling" them with the curricular orthodoxy. Accordingly, the prime pedagogic concern was with the "structure of knowledge".

As a constructivist working in the early decades of this century, however, Piaget was instrumental in initiating a shift in orientation in the traditional pedagogic response to learner difficulties. Learners were now understood to be active construers of their environment and of the world. As such, they were anticipated - and found - to hold their own conceptions on many subject areas prior to encountering them in formal educational settings.

Piaget's early studies (e.g. Piaget, 1929, 1930) were 'naturalistic', ideographic and informal - they were effected chiefly through talking with children. As Piaget went on to develop his logical meta-theory, however, his research methods and those of his co-workers - 'Piagetian School' (PS) - changed, in accordance with their new research interests.
Thematic difficulties in concept learning came to be understood to reflect not gaps in their knowledge, but, rather, in their (universal) 'cognitive structures' - though it would seem that Piaget probably confounded structure of knowledge with structure of cognition (Brown, 1984: personal communication, see Appendix 2). Elicitation techniques became predominantly de-verbalised within the PS. Despite the shift in orientation achieved by Piaget's influence, the traditional pedagogic response to learner difficulties remained essentially unchanged as a result: the alleged 'universal necessity' of his cognitive structures meant that learners could still be unilaterally attributed with "systematic mistakes"; the remedy would be basically the same.

Now many ACM workers cite both the subject of Piaget's early researches, i.e. children's conceptions of the world, and his principal investigative method, i.e. the clinical interview, amongst their chief sources of inspiration. Disaffection with Piaget's ideas, on the part of members of the ACM, has come both early and late - and this both in their own research commitments and in the development of Piaget's theory. There would seem, however, to be a consensus within the ACM that wherever and whenever made, it is Piaget's later preoccupation with the development of universal logic operations which represents the point of departure. From an ACM perspective, these are not only construed to be of greater interest and importance to Piagetians than the ideas and concerns of the individual child, but also to distance and obstruct investigation of the latter - to the ultimate detriment of the educational welfare of the child (cf. arguments of section 3.4.1).
I suggest, then, that the emergence of the ACM may best be construed as a self-conscious step back to an earlier age of theoretical "innocence" in order to remain close(r) to the actual child rather than as a result of the many formal and empirical difficulties which now beset Piaget's structuralist meta-theory. A useful and instructive concern to develop formalisations was ever more widely construed to have become a sterile and esoteric exercise in formalism. Many researchers, however, initially perceived their divergence to be only a change of emphasis in their own work relative to that of Piaget himself and his closest colleagues, viz. away from Piaget's later preoccupation with articulating the universally necessary development of "logical operations" in favour of his earlier interest in the idiosyncratically pertinent development of 'causal frameworks'. The relationship between these two aspects of PS research may be elucidated by reference to a diagram used by Driver:

![Diagram Showing Relation Between Research into the Development of Logical Operations and Research into the Development of Causal Frameworks (after Driver, 1982, p.360).](image)

**Figure 5.1** Diagram Showing Relation Between Research into the Development of Logical Operations and Research into the Development of Causal Frameworks (after Driver, 1982, p.360).

Now, the point at which a change of emphasis in research may be said to
have resulted in delineable and incompatible research traditions depends upon the breadth of 'core commitments' one attributes to each approach. Clearly, both ACM and PS are epistemologically constructivist traditions since they each embrace both AER and CKT, as we shall see in the latter's case. These, however, are only necessary conditions for a constructivist theory of knowledge - alone they are not sufficient; and demonstration of sufficiency in each approach reveals differences which show them more plausibly to be different research programmes rather than rival versions of the same programme, as some - mainly members of the fading PS - have tried to argue. (See, for example, Gilbert and Swift, 1985, - included as Appendix 1, for a Lakatosian analysis of PS and ACM and from which they emerge as rival research programmes).

The ACM's central meta-theoretical notion was christened the 'alternative framework' by Driver and Easley. (1978, p.62). Part of these authors' meaning for this expression is as follows:

'In learning about the physical world, alternative interpretations seem to be the product of pupils' imaginative efforts to explain events and abstract commonalities they see between them. These may be in keeping with their experience although they may be recognised as partial explanations of limited scope'. (Driver and Easley, 1978, p.62: my emphasis).

The quality of 'alternativeness' intrinsic to these authors' notion may be construed to represent a further divergence from the Piagetian inspired idea of a 'causal framework'. No doubt this was prompted, in part, by the semantically and conceptually problematic notion of 'causality' itself and which may be construed to be underwritten and reflected by Piaget's
structuralist psycho-logic in a very deterministic manner.

As with most emergent traditions, however, implicit value commitments and tacit knowledge within the ACM remain well in advance of their explicit and formalised counter-parts. Thus, some four years after Driver and Easley's seminal paper, in a survey article of the by then burgeoning research literature which claimed or could be argued to be sympathetic to the idea that educands might have personal, alternative, interpretations in the domain of science, Driver and Erickson (1982, p.17) were only able to identify the following three, highly generalised, commonalities amongst assumptions informing the body of work which they examined:

(1) Some form of 'cognitive structure' is presupposed. It is, perhaps, a structure of 'content-independent' processing skills, but for most of the newer investigators (as they interpret them) it is a structure of 'content-dependent' elements such as particular concepts or propositions;

(2) A 'constructivist epistemology' is also assumed;

(3) It is taken on trust that understanding educands' ideas is important in formal educational settings.

I suggest that the looseness in the wording of these shared assumptions stems partly from the radical nature of certain of Driver and Easley's value commitments and their implications concerning the (potential) status of educands' personal knowledge within the education system. These were
value commitments which were only fully shared by a minority of other researchers (amongst whom I include myself and my immediate colleagues at the University of Surrey - about whom, more later), and which, moreover, were not even noticed by many. These value commitments may be summarised as a conscious and conscientious desire not to pre-judge, in educational research, the worth of educands' personal knowledge relative to the curricular orthodoxy. This runs counter to traditional pedagogy - including that sustained by the PS.

To elaborate upon these points, I shall begin by returning to Driver and Easley's (1978) influential paper in which they outlined both the semantic rationale for their choice of the expression 'alternative framework' and their epistemological commitments which informed it. These authors suggest that there are some who would question why a study of educands' conceptualisations warrants attention at all and that such persons would construe them as 'wrong' ideas, being the result of 'incorrect observations', or 'illogical thought'. Driver and Easley claim that Ausubel's term 'preconception', for example, carries something of this connotation. Similarly, the widely used term 'misconception'. They conclude that

'Research reported later on common misconceptions in various areas of science reflects the fact that this term tends to be used in studies where pupils have been exposed to formal models or theories, and have assimilated them incorrectly'.

and go on to argue for their notion of an alternative framework:

'A distinction needs to be made between this source of misunderstanding and the situation in which pupils have
developed autonomous frameworks for conceptualising their experience of the physical world; these we will call 'alternative frameworks'.

(Driver and Easley, 1978, p. 62: original emphasis).

In this paper, Driver and Easley do not articulate their epistemological commitments beyond endorsing the basic constructivist stance (cf. my earlier quotation from this paper), though for reasons that should by now be clear, I consider it unfortunate that they cite only Popper in opposition to empirical-inductivism.

In a paper entitled 'The Pupil as a Scientist', Driver has claimed that

'[] pupils, like scientists, come to science lessons with some ideas or beliefs already formulated. These beliefs affect the observations they make and the inferences they draw from them. Pupils, like scientists, have construed a view of the world to enable them to cope with situations. Changing this view is not as simple as giving pupils additional experiences or sense data. It also involves helping them to reconstruct their theories or beliefs, to undergo, if you like, the paradigm shifts which have occurred in the history of science'.

(Driver, 1979, my emphasis).

Now, the commitment to some kind of constructivist theory of knowledge is clear enough in quotations such as these but there is little detailed specification of the particular "brand" of constructivism which is endorsed. Without such specification, however, the pedagogic import of, for example, the suggestion made in the last quotation that student scientists should undergo 'paradigm shifts which have occurred in the history of science' must remain somewhat ambiguous, viz. this could be
given a very (by now) traditional 'discovery learning' interpretation.

More formal references to constructivist epistemology have tended to be tantalisingly brief and similarly ambiguous. Driver and Erickson (1983, p.55) for example, claim to reject a 'rational-empirical view' - but since they do not discuss the matter further it is not clear whether they are merely following the trend in rejecting positivistic theories of knowledge or, more radically, those which I have termed objectivist-methodological-constructivist - such as that due to Popper and his school.

In a survey of antecedents to the ACM, Watts (1983, Ch.1, p.10), who acknowledges the central influence of Kelly's ideas on his approach, proposes the following four dimensions for appraising notions of 'concept' and 'concept teaching', and which gained prominence in the curriculum reform movement beginning in the late 1950s and to which the ACM might be seen as its heir:

1. Process ---------------- Product
2. Informal ---------------- Formal
3. Hypothesis testing ------- Abstraction
4. Semantic ---------------- Logical-mathematical

As I interpret him, Watts argues that the meta-theoretical notions and work of the ACM reside predominantly, though not exclusively, on the left hand side of these dimensions.

In elaboration of, and in addition to, Osborne's (1980) constructivist
distinction between Children's Science (Sch) and Scientists' Science (Ss) - notions complementary to Driver and Easley's idea of alternative frameworks - Gilbert, Osborne and Fensham (1982) further delineated 'Teachers' Science' (St): cf. my discussion of these authors' meanings for these terms in Chapter 2. With the help of a series of diagrams Gilbert, Osborne and Fensham articulated three possible assumptions held by educators, educationalists and curriculum planners:

![Diagram](image)

Service teaching in which it is assumed the learners have no theoretical views of the topic or phenomena under study.

(a) The "Blank-Minded" or "Tabula Rasa" Assumption

![Diagram](image)

Science teaching in which it is assumed that learners may have theoretical views but that these are easily displaced by the views presented by teachers.

(b) The "Teacher Dominance" Assumption
Science teaching which recognises that learners often do hold strongly entrenched theoretical views that persist in the face of teaching.

(c) The "Student Dominance" Assumption

Fig. 5.2. Diagrams showing Three Possible Assumptions of Science Pedagogy (after Gilbert, Osborne and Fensham, 1982)

These authors also considered the way in which the assumptions informing the content of science curricula and their associated teaching materials might interact with St:

![Diagram showing Three Possible Assumptions of Science Pedagogy](image)

Strongly held teachers' views of science may persist or interact with the views in science curricula.

Figure 5.3. Diagram Showing Interactive or Independent Relationship between Teachers' Science and the Science of Curricula (after Gilbert, Osborne and Fensham)

Finally, Gilbert, Osborne and Fensham proposed five possible outcomes to teaching and learning based upon different relationships between $S_{Ch}$ and $S_T$:

![Diagram showing Interactive or Independent Relationship between Teachers' Science and the Science of Curricula](image)

A pre-learning or children's view of science can persist unchanged by science teaching.

(a) The Undisturbed Children's Science Outcome
Science teaching can result in a second view acquired for use in school but the original children's view persists elsewhere.

(b) The Two Perspectives Outcome

The original children's view is strengthened by science teaching which now is misapplied to support it.

(c) The Reinforced Outcome

Science teaching resulting in a mixed outcome where children's science and teachers' science now co-exist together.

(d) The Mixed Outcome
Science teaching which extends children's science and teachers' science to a more unified science view.

(e) The Unified Scientific Outcome

Figure 5.4. Diagrams Showing Five Possible Consequences of Children's Science for Science Teaching (after Gilbert, Osborne and Fensham, 1982)

As we have seen in Chapter 2, Zylberstajn (1983) named the collective assumptions informing science curricular 'Curricula Science' (Scr) and further delineated 'Students' Science' (Sst). Zylberstajn also articulated these notions, together with those proposed by Gilbert, Osborne and Fensham, within a generalised sequence of transformations of Ss through formal science education, his diagrammatic depiction of which I re-present for the sake of completeness:

Figure 5.5. Diagram Showing Generalised Sequence of Transformations of Scientists' Science through Formal Science Education (after Zylberstajn, 1983)

(Cf. my addition of 'Philosophers' Science' and other modifications to
Zylberstajn's model in Chapter 2).

Now, as ACM research has gathered momentum, a number of issues have come to the fore which have been widely recognised (both within and without the ACM) as urgently needing clarification and which have called into question the adequacy of original formulations of the meta-theory. Prominent amongst such issues is the 'stability' of alternative conceptions - stability through time (sometimes referred to as 'durability') and stability across context, and whether the most appropriate unit of analysis should be the individual person or a population (see, e.g. Driver and Erickson, 1983, p.46, for a discussion).

In an influential article, Gilbert and Watts (1983) have responded to such issues and controversies. They suggest that while the expression 'alternative framework' has become widely used as a descriptor for the outcome of classroom investigations it commands little consensus in its detailed meanings and applications; 'contextual boundaries' and the quality and range of 'prediction' within educands' alternative conceptualisations nevertheless emerge as two useful markers to help distinguish meanings intended for the meta-theoretical expression(s) used. These they incorporate within their own distinction between 'conceptions', 'categories', and 'frameworks':

'Our proposal here is that 'conception' be used to focus on the personalised theorising and hypothesising of individuals. Our contention is that each person's knowledge is unique (though not infinitely diverse), which thus limits the generalisability of the single case study. Conceptions are accessed by the actions (linguistic and non-linguistic, verbal and non-verbal) of the person, often in response to particular questions.
To generalise beyond the individual is to construct groupings of responses which are construed as having similar intended meanings. This is to construct a category of response commonly in the context of single, or a specific set of, questions. Categories are not individualised and represent an interpretation of statements at a more general, but functional, level.

Alternative frameworks can profitably be seen as generalised non-individual descriptions. That is, their relation to the data base is one level further removed than that of a category of response. They can be seen, then, as short summary descriptions that attempt to capture both the explicit responses made and the construed intentions behind them. They are thematic interpretations of data, stylised, mild caricatures of the responses made by students’.

(Gilbert and Watts, 1983, p.69: my emphasis).

Gilbert and Watts illustrate their distinction between the terms by means of the following diagram:

![Diagram](image)

Figure 5.6. Diagram Showing the Relationship between Meanings for 'Conception', 'Category' and 'Framework' (after Gilbert and Watts, 1983, p.70).

By way of further interpretation of these terms, Gilbert and Watts go on to suggest that
These represent levels of interpretation of the data and there is no suggestion that any one is preferable over the other. Each has its own focus and limits. Conceptions focus on the personalised accounts of individuals whereas categories interpret multiple data. They represent functional subdivisions of bulk data according to some features ascribed by the researcher, within a logical context. Frameworks focus upon a characterisation of responses and bridge small local changes in context'.

(Gilbert and Watts, 1983, p.70: my emphasis).

Now, given my stated interests and aims for this thesis, the thing that I find most noteworthy throughout the more influential amongst ACM researchers who have turned their attention to broader issues of meta-theory and their implications for curricular and teaching reform is their tacit or ambiguous acceptance of the 'unified scientific outcome' to science teaching.

Gilbert, Osborne and Fensham (1982), for example, make no evaluation of the different outcomes to science teaching - which, I suggest, amounts to a tacit endorsement of the unified scientific outcome as per the status quo. Similarly, while I wholeheartedly agree with Driver and Erickson when they urge that

'What is called for is a clarification and redefinition of what is taken to be 'school science'. If the 'student as scientist' metaphor [ ] is to be taken seriously then this requires opportunities for young people to explore both new phenomena and new ideas; to listen to and appreciate alternative points of view without losing confidence in their own capabilities to comprehend and to act; to construct their own knowledge and, perhaps by so doing, gain also some appreciation of science as a pursuit of the human imagination'.

(Driver and Erickson, 1983, p.55)

I contend that the implications for pedagogy may be very radical indeed.
for systematic variance from the curricular orthodoxy of educands' alternative conceptions is all that has so far been claimed. I also contend that for such suggestions actually to be implemented requires further articulation and agreement of the brand of constructivist epistemology endorsed, otherwise the stance taken on crucial issues such as assessment shall remain ambiguous - a contention which is central to my choice of concerns explored in this thesis. Meanwhile, this ambiguity has afforded a toe-hold for critics from more traditionalist research programmes (mainly the PS) to try to assimilate what I believe to be the radical and good implications of ACM research for pedagogic reform (discussed in Chapter 10) back into the status quo. As attention within the ACM focusses upon curricular and teaching reform there is, in addition, some evidence to suggest that certain members are now experiencing difficulty in living up to their own liberal, relativist, principles of research method, predicated upon notions of alternative-conception (no 'wrong' answers), in their attempts to articulate a compatible theory of teaching, indeed, there would seem to be some degree of 'fragmentation' (in Kelly's sense) between the theories of learning and theories of teaching that are espoused.

Now, these issues are large and complex: I empathise with the difficulties involved and I cite the authors above only to illustrate the nature and extent of the problem. Moreover, I have, in a sense, been unfair (though, I contend, not grossly so) in the manner in which I have de-contextualised some of the statements above for there is, indeed, epistemological sophistication within the ACM - but, at the present time, it has principally to be inferred from the movement's characteristic research
methods and from the criticisms which they have drawn.

Accordingly, to complement the 'conservative activist vs. revolutionary - activist' (Lakatos) and 'empiricist vs. post-empiricist' (Hesse) frameworks for appraising epistemology that I have used in earlier chapters, I now invoke a dimension for construing research methods and which has been called 'Paradigm 1 vs. Paradigm 2' as proposed and discussed by Gilbert and Pope (e.g. 1984a, b).

These authors summarise the chief characteristics of the research methods of each paradigm by means of the following dimensions (Gilbert and Pope, 1984b, p.18):

<table>
<thead>
<tr>
<th>Paradigm 1</th>
<th>Paradigm 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Traditional</td>
<td>Non-traditional</td>
</tr>
<tr>
<td>Scientific</td>
<td>Artistic</td>
</tr>
<tr>
<td>Experimental</td>
<td>Naturalistic</td>
</tr>
<tr>
<td>Reductionist</td>
<td>Holistic</td>
</tr>
<tr>
<td>Prescriptive</td>
<td>Descriptive</td>
</tr>
<tr>
<td>Quantitative</td>
<td>Qualitative</td>
</tr>
<tr>
<td>Experiment</td>
<td>Case Study</td>
</tr>
<tr>
<td>Nomothetic</td>
<td>Ideographic</td>
</tr>
</tbody>
</table>

I concur with those (e.g. Gilbert and Watts, 1983) who suggest that the work of the ACM falls under the aegis of paradigm 2 and I further suggest that the work of the PS (that due to 'Piagetian technologists', cf. my discussion in section 2.4.1) is predominantly paradigm 1.

In partial elaboration and justification of these claims, I suggest that the mainstay of ACM research methodology i.e. the clinical or focussed interview and case study, chiefly reflect the movement's pre-eminent
concern with eliciting and developing shared meanings (verstehen) in volitional purpose rather than in nomethetically ascribing explanations invoking causal factors (erklären).

Recognising the value-laden nature of all research interpretations, 'triangulation' of research methods is thus a more characteristic feature in the work of the ACM than in that of, say, the PS. Methodological triangulation has been summarised by Denzin as involving

'[

] a complex process of playing each method off against the other so as to maximise the validity of field efforts

[ ] the flaws of one method are often the strength of another'.


Thus, for example, Gilbert and Pope (1983) used a combination of videotaped educand group discussions of IAI cards (described in section 7.4.2.1) and research-educand "debriefing" focussed interviews to explore the educands' conceptions of energy. Bell, Brook and Driver (1984) used written exercises in combination with focussed interviews to investigate educands' personal understandings of the particle theory of matter. I have used written exercises (Chapter 7) in conjunction with focussed interviews (Chapter 8).

I shall not labour over articulating the many technical similarities that exist between the research methods (and the epistemological assumptions underpinning them) of the ACM and those of relativist-methodological-constructivism as realised by Kelly and Feyerabend.

It is worth first noting, however, that although Kelly is best known for
his involvement with psychotherapy he himself had a Master's degree in educational sociology and considered psychotherapy and education to be essentially the same enterprise. Similarly, Feyerabend, best known for his work in history and philosophy of science, claims that his work originated with an interest and concern for the 'problem of knowledge and education in a free society' (Feyerabend, 1978, p.107: my emphasis).

I suggest then, that the triangulation of research methods which is characteristic of the ACM's research approach may readily be construed in terms of Feyerabend's notion of a pluralistic methodology. A similar methodological commitment may be discerned in Kelly's ideas — see Swift, Watts and Pope (1983) where we argue that PCP embraces '(m)ethodological pluralism'.

The ACM emphasis upon the content dependency of constructive structure has led to the choice development and use of research methods in a manner which complements Kelly's view that researchers

' [...] should not overlook what their subjects have to contribute for psychological research as I see it is a cooperative enterprise in which the subject joins the psychologist in making an enquiry. I am very sceptical of any piece of human research in which the subject's questions and contributions have not been elicited or have been ignored in the final analysis of results.' (Kelly, 1969, p.132).

I suggest that, content-dependency may most fruitfully be thought of as a requirement of investigative methodology which complements context-dependency in constructivist epistemology.
Again, the ACM's notions of constructive structure which permit the simultaneous existence of rival points of view within an individual's mind complement Kelly's notion of fragmentation and Feyerabend's incommensurability thesis as do the proposed dynamics of learning, and the growth of knowledge (discussed in Chapter 10). (Kelly's claim that meaning requires a dimension of relevant contrast currently enjoys only marginal acceptance within the ACM, however. This I believe has been largely due to the perceived difficulties of construing scientific theories and laws in these terms - difficulties which I hope to have elucidated, if not yet overcome, in my previous chapter).

Critics of, and would-be sympathisers to, the ACM who claim to be constructivists often first betray their conservative-activism by couching their methodological complaints, comparisons and commentaries in the terms and values of Paradigm 1.

A case in point concerns a symposium convened by Dr. Michael Shayer (who aligns himself with the PS) for the 1984 annual conference of the British Educational Research Association and dedicated to appraising how 'complementary' or how 'incompatible' the approaches of the ACM and PS might be. In preparing my contribution to that symposium (Swift, 1984), however, I felt that only around half of the list of six issues, suggested in good faith by Dr. Shayer for discussion, were cast in terms equally appropriate to either approach, viz. I judged his suggestions for considering 'reliability', 'validity' and 'generalisability' of research inferences to reflect values and commitments specific to Paradigm 1 and, as such, were inappropriate standards for appraising the work of the
ACM. (see Bassey, 1981, 1983a,b, for spirited and compelling arguments in favour of pedagogic research into 'singularities', i.e. case-studies, and against research seeking generalisations; also see Phillips, 1974, for arguments for 'conservative' influences upon 'experimental', i.e. Paradigm 1, research in education).

From the point of view endorsed in this thesis, I would suggest that the closest respective counter-parts to the Paradigm 1 standards of 'reliability', 'validity' and 'generalisability' would be 'durability', 'authenticity' and 'commonality'.

To elaborate slightly, I suggest that there has been some confounding by Paradigm 1 researchers between their methodological demand that research interpretations be reliable and the ACM's existential claim that some alternative-conceptions are durable, i.e. exist through time and are resistant to attempts to change them. 'Authenticity' of research interpretations is a methodological issue which I shall discuss later in this chapter, and I have already discussed 'commonality' (in Kelly's sense) in Chapter 4.

Now in describing his own work and in responding to criticisms of it from members of the ACM, Shayer similarly identified his commitment to objectivist epistemology by claiming value neutrality of his educational research.

Thus, he responded to the ACM query, that the impact of his psychometrically orientated research upon teaching policy might lessen or
underline the development of autonomy in the individual child by stating that

'I am bound to say [ ] that in relation to this question the published work of Piagetians is, in itself, neutral'.
(Shayer, 1984, p.2).

And elsewhere, with a colleague, Shayer has responded to the criticism of 'labelling children' by arguing that

'[ ] there seems to be no more grounds for objecting to Piaget's work because of its determinist picture of human mental capacity than to object to the science of physiology for similar reasons. The ability to describe and estimate a person's present mental capacity is no more an instrument for maintaining the person at that level than is our ability to describe a bodily state in physiological terms. Indeed, both increase our capacity to intervene in what is the present described condition'.

That education research methods are not neutral with respect to educational values and teaching policy, has been argued with great clarity, and many examples, by Carr, who proposes that

'Specific educational research methods entail specific educational values'.
(Carr, 1983, p.6).

Briefly stated, Carr's justificatory argument for this proposition is that whilst 'explanatory frameworks' educational research methods cannot, strictly speaking, be said to entail conclusions as to educational values. They can, nevertheless, be demonstrated to (tend to) support some such conclusions, and to (tend to) undermine others. Put another way, there is no sharp division that can be drawn between knowledge claims and knowledge uses.
This often unrecognised and misunderstood point may be further elucidated by examining some further remarks made by Shayer, about his work, his critics and the ACM, in the symposium mentioned earlier:

'To begin to realise my own intentions, I have had to spend three years following the CSMS [Concepts in Secondary Mathematics and Science] programme research, using two major and different models of intervention, and it is noticeable that both Driver and Erickson (1983) and Gilbert and Watts (1983), are somewhat embarrassed in their surveys in bringing in intervention studies, recognising I believe the lack of necessary connection.' (Shayer, 1984, p.2).

What is of epistemological interest in Shayer's comment (and several others which he makes in that paper), is his tacit appeal to Hume's Law, viz. he is accusing Driver and Erickson and Gilbert and Watts of embarrassment due to accepting that an 'ought' (i.e. an educational value informing a policy decision) cannot be derived from an 'is' (i.e. from the 'facts of educands', 'mental capacity', uncovered by educational research).

As either a critique or a counter critique of the ACM, however, this is doubly inappropriate. Firstly, Hume's Law employs the traditional categorical distinction between factual and evaluative knowledge, yet, as I have argued in Chapters 3 and 4, the possibility of value-free knowledge claims is denied by relativist-methodological-constructivism and thence also in Paradigm 2 research. Invoking Hume's Law, therefore, constitutes neither a criticism of the ACM, nor a defence of the PS, though there may be other criticisms and defences. Secondly, as Haack points out
'even if it is held that there is no entailment or derivability relation between a set of factual statements (assuming that these can be delineated clearly) and a value statement, this is not to hold that there is no relation between the two,' (Haack, 1976, p.162: original emphasis)

In some of their appeals to Hume's Law, however, Paradigm 1 workers do make this further unjustified assumption: Shayer, as we have seen, claims his educational research (and that of the PS) is neutral. I suspect that some confusion may have resulted over the intended meanings for terms such as 'implies', and 'consequence', viz. in the context just discussed, ACM workers do not use such terms in formal deductive senses. I hope to have clarified this issue by establishing that educational research methods and educational values share a relation of neither derivability nor independence.

So, to Shayer's claim that Driver and Erickson and Gilbert and Watts are 'somewhat embarrassed in their surveys in bringing in intervention studies', I am not aware that these authors experienced any embarrassment - why should they? Certainly the quality of "constructive connectedness" which I argued to exist within a Kellyan construction system (section 4.2) would indicate that even classroom research (as opposed to teaching itself) is intrinsically interventive. And in their paper, to which Shayer refers, Gilbert and Watts (1983, p.66) explicitly endorse a notion of constructive connectedness within their 'actional' view of concept. For the educational researcher, no less than for the teacher, the classroom is always a "room for improvement". I suggest, however, that what the ACM workers, whom Shayer cites may in fact have been experiencing, was not 'embarrassment', but rather a certain amount of diffidence in
articulating the exact nature and extent of the classroom 'interventions',
that shall necessarily occur if a theory of teaching, which is compatible
with ACM meta-theory and research interpretations, is, indeed, realised
(cf. my discussion in Chapter 10, of individual 'integrity' as opposed to
'autonomy', within a 'personal construct pedagogy').

Now in a review article on the ACM, in 1982, Sutton, a "sympathetic"
critic, concluded that

'[members of the ACM] hope to be able to discern the
organisation of [educands'] prior knowledge, but so far
descriptions of the supposed "frameworks", "alternative
conceptions", "personal constructs", or "learners' mini-
theories" are not very precise, and no method of mapping
them yet commands wide assent'.
(Sutton, 1982, p.42).

Developments to ACM meta-theory such as Gilbert and Watts (1983),
delineation of 'conceptions', 'categories', and 'frameworks', discussed
earlier, have gone a considerable way to counter such early criticisms.
Certain others, however, have been more recalcitrant.

I suggest that, amongst these latter, should be included McClelland's
(1984) thoughtful and provocative article. McClelland claims to accept
the ACM proposition that 'children spontaneously theorise about events in
their experience', but he has 'reservations about the level and
pervasiveness of the activity' (p.2); he argues that many, indeed, most
alleged alternative conceptions could be construed' more probab[ly] and
more parsimonious[ly]' in terms of educands' 'strategic inattention',
'piecemeal explanation' 'instant invention' and so on.
McClelland's article is unusual amongst critics of the ACM because he begins by presenting his personal meaning of an alternative conception (alternative framework):

'An alternative framework or private version is alternative if it opposes or fails to fit into an accepted pattern of scientific explanation, and it may be termed a framework to the degree that it consists of interlocking concepts unifying more than one set of phenomena. It is science if it is the result of a conscious attempt at theorising'. (McClelland, 1984, p.1).

And then the epistemological nub of his critique:

'The last of these points requires elaboration, for there is a weak sense and a strong sense, in which the terms 'science' and 'scientific' are used - at least, in English. Such phenomena as atmospheric pressure, solution of salt, heat conduction, vegetable reproduction, and so on, form part of what is commonly termed the content of courses in science. Learning about such phenomena thus comes to be called learning about science. However, phenomena are not science, nor are ideas about them necessarily scientific in a strong sense, otherwise there are no ways of conceiving of them which are not scientific, to suppose that children are scientists of a sort when they think about such phenomena seems to me to misconstrue totally the meaning and purpose of science. The distinction between such thinking and that of a science identified by Osborne [Bell and Gilbert] (1983) is categoric, not one of degree. Scientific concepts are deliberately theoretical and formulated with aspirations to inclusiveness. This ambiguity has underlain many arguments about appropriate content in science. Phenomena are not the content of science but the vehicle for learning it, for learning theories. Children in all societies meet a wide range of phenomena, but a glance at history and anthropology is enough to remind us that interpretations in terms of reproducible, explicable, causally related events are not automatic features of human thoughts'. (McClelland, 1984, p.1).

Now, I consider McClelland's distinction between 'strong' and 'weak' senses of 'science' and 'scientific' to be a useful one, indeed, I use it to categorise part of my interview data (Chapter 8). His epistemological
claim that the difference between 'thinking' about phenomena, and
scientifically 'theorising' about them, is 'categoric, not one of degree'
may, however, quickly be dispensed with for this merely states an
objectivist's position on the matter. As I have argued in Chapter 4,
objectivist epistemologists including objectivist-methodological-
constructivists always preserve a 'factual core' in matters of theorising
and of theory choice; hence they may distinguish phenomena (or merely
'thinking' about them) from theories. In relativist-methodological-
constructivism, by contrast, there can be no such factual core.
Consequently, to think about a phenomenon is to theorise about it, at
least to some degree, for otherwise it would be beyond the realm of
possible experience. Thus, from the relativist-methodological-
constructivist's point of view, which I have argued coincides with the
constructivist epistemology of the ACM, the distinction between thinking
and theorising about phenomena is, indeed, one of degree not of kind - as
is normally implied about subjects which are distinguished by the
adjectives 'strong' and 'weak'.

Certain amongst McClelland's methodological criticisms of the ACM are
not so easy to dismiss. Thus he argues that

'If a research worker effectively says to a child, 'I want
you to tell me what you think about X', and X is part of
usual experience, it would be very difficult for the child
to reply, 'I do not think about X'. Some sort of answer
is a social imperative'.
(McClelland, 1984, p.4: original emphasis).

and suggests recourse to 'rules and rituals' and 'instant invention' as
likely response strategies for the child who does not have 'fully formed
Now, in taking note of these plausible possibilities I feel that it is important not to misconstrue the task ahead: the notion of an alternative-conception has great influence upon the choice and development of research methods.

Thus, if it is accepted that there can be no absolute, unilaterally ascribed, judgements made about the worth of another person's construct, then all inferences made about them must be negotiated, indeed, evidence of negotiation constitutes the principal criterion for the 'authenticity' of such inferences. The more evidence of negotiation of research inferences that there is, the more authentic such inferences are judged to be. Authenticity is thus primarily a methodological requirement or quality but it is one that is highly complementary to the philosophical basis that we endorse.

This notion of authenticity contrasts markedly with what we judge to be the nearest 'functional equivalent' in Paradigm 1 research, namely, 'validity'. In classical applications of the notion of validity, attempts are made to keep persons researched in ignorance of the researcher's interests and conclusions. Investigations conducted on the approach we endorse, however, are fundamentally collaborative - a quality which prompted Swift (1984) to suggest that 'collaborates' would be a more appropriate term rather than 'subject' to refer to persons whose ideas are investigated. Hence McClelland's point about a child being obliged to present some sort of answer in response to a research worker's question
may be accepted without the same inference being drawn. To explain, as a member of the ACM, I can recognise the sort of situation he describes from my own research experience but I consider it to reflect an unfortunate aspect of the present education system. Accordingly, I have gone to considerable lengths to develop investigative techniques such that a child feels able or willing to express their view - even if that view is that they have no view.

I feel, then, that McClelland is being overly cynical about children's willingness to be open and frank in interview situations and that this stems, in part, from his objectivism. I suggest that members of the ACM believe that there are ways, sophisticated, sensitive, ways of effectively asking a child 'I want you to tell me what you think about X' so that they do not feel that to answer is a 'social imperative', but rather, is an invitation which can be refused if desired. Clearly there is no independent, 'objective', means for checking this assertion - or, indeed, any of the ACM's research inferences, for this is precisely what the approach denies. Ultimately, the whole research enterprise of the ACM rests upon a willingness to invest some degree of trust in the person whose views are sought. I suspect that Paradigm 1 researchers find this notion particularly hard to swallow. McClelland's arguments are no less tautological than those of the ACM - if anything, they are more so through not having been negotiated with any persons. Moreover, from a relativist-methodological-constructivist perspective, the principle of parsimony, which he invokes, is as value-laden a methodological rule as is any other.

Supposing, however, that McClelland is right and most children have indeed
"instantly invented" the ideas that members of the ACM have labelled 'alternative-conceptions'. Surely this would render the intellectual and imaginative capacities of the children even more staggering than is being claimed on the alternative-conception interpretation - at least, with respect to instances of 'instant invention'? And it would make a very complementary point to that presently propounded by the ACM. Even if the only impact that the ACM's central meta-theoretical notion had upon educationalists and educators was to be construed as a stimulus to develop alternatives ('instant invention' etc), then proposing it would have been worthwhile. This is because alternative accounts and interpretations, such as those McClelland proposes, may also provide valuable insights into the present education system and children's thinking processes within it.

Now, as I intimated in Chapter 1, I believe that the ACM can and should concede to critics that there have been some instances where invoking the notion of an alternative-conception has been conceptually extravagant or even gratuitous. Specifically, in departing from the traditional notion of a "systematic mistake", existing ACM meta-theory still fails to provide an adequate rationale for distinguishing the "trivially different" learner conception from the "genuinely alternative" learner conception.

Is there some non-preemptive way in which the notion of an alternative-conception can be rendered more discriminating and so meet such a criticism? I believe there is.

With the links that I have drawn in Chapters 3 and 4 between the formal content of Kelly's theory and Feyerabend's post-empiricism in mind, it
shall not surprise the reader that I propose an 'incommensurability
criterion' whereby the alternative-conception is the incommensurable
conception (Swift, 1985b).

To elaborate slightly, a putative alternative-conception may be said to
be such if, and only if, a relation of incommensurability can be shown to
exist between it and the reference conception, i.e. the two conceptions
can be demonstrated to exhibit (1) mutual factual adequacy, (2) mutual
ontological incompatibility. This criterion may help to explain and,
perhaps, to justify, the characteristically " atheoretical" form which ACM
field reports take, as may be further elucidated by Feyerabend's remark
that

'As incommensurability depends on covert classifications
and involves major conceptual changes it is hardly ever
possible to give an explicit definition of it. Nor will
the customary 'reconstructions' succeed in bringing it to
the fore. The phenomenon must be shown, the reader must be
led up to it by being confronted with a great variety of
instances, and he must judge for himself'.
(Feyerabend, 1975b, p.225).

So too with educands' alternative-conceptions (cf. Black's, 1986, review
of Driver, Guesne and Tiberghien, 1986 - excerpted in Chapter 1). This
notwithstanding, for a compatible theory of teaching ever to be developed
and implemented, some kind of 'rational integration' of ACM research must
be achieved. In Chapter 10 I shall argue that the formal content of PGP
could fulfill this role.
5.3. A Selective Review of Research into Students' and Teachers' Personal Epistemologies of Science.

My review of the relevant literature, which follows shortly, shall concentrate exclusively upon my stated focus of field research, i.e. students' and teachers' personal epistemologies of science. Accordingly, I shall defer until Chapter 10 my consideration of research which explores highly related subjects and issues such as implications for curricular reform and the epistemological assumptions of science curricula and the image which they project in the classroom. This deferment applies also to those studies which also consider such issues and which are included in my review, below.

Notwithstanding the plethora of studies which have used questionnaires to investigate educands' 'attitudes' to science and their 'image of the scientist', as broadly interpreted by researchers upon an objective - subjective dimension, there has been very little detailed enquiry conducted in light of traditions in philosophy of science and using Paradigm 2 research methods into educands' and educators' personal meanings of science and scientific-method - and none (that I am aware of) into educationalists' views on these matters.

Partly due to the constraints I impose for my immediate purpose and partly due to its apparent novelty, then, my review of the literature shall encompass only two studies. These are due to Rowell and Cawthon (1982) and Dibbs (1982). I shall consider them in this order.
Rowell and Cawthon (1982) devised a questionnaire to survey the awareness and impact of the conflicting ideas of Popper and Kuhn upon University populations. Their sample was drawn from two South Australian universities and was composed of a total of 254 students and 52 staff. The items on their questionnaire consisted of verbatim, or near verbatim, statements from the written works of Popper, Kuhn, and 'inductivist-empiricists'. A five point (1 to 5) scoring system was used to provide an appropriate position on a continuum varying from 'strong agreement' to 'strong disagreement'. Two responses on such scales were required to each statement: the first asked for a reaction to the statement as a comment on what science is, the second, a reaction to the statement as a comment on what science should be.

Within the 'as is' and the 'should be' data sets, the factor pattern matrices produced by the statistical methods employed indicated the general stability in the clustering of variables. However, the nature of both the 'as is' and 'should be' factors provided little support for the hypothesis that the three images of science were 'comprehensively and separately operated' by groups within the sample to provide 'internally consistent frameworks' for their views of science. Rather, what these authors interpreted to emerge from the data was a majority view of science reflecting a hybrid of Popperian ideas with the earlier more 'traditional ones'. Similarly, there was little consensus with respect to the Kuhnian view, though factor analysis was taken to suggest that the position is known, or is at least meaningful, to some respondents.

From my point of view, the research inferences of this study are
interesting in two main ways. Firstly, they suggest that, overall, such student learning of the 'process of scientific investigation and reasoning', now included amongst most curricular objectives (Fensham, 1983), as may have been achieved has not yet been mediated by formal traditions in philosophy of science; likewise, such expertise and sophistication as educators might possess in understanding scientific process and reasoning. Secondly, that there is a degree of fragmentation within the construction systems of many educands and educators.

In a Ph.D thesis, Dibbs (1982) conducted an investigation into the nature and consequences of secondary school science teachers' implicit philosophies of science for teaching. By use of questionnaire techniques and follow-up interviews with a sub-sample of respondents, Dibbs identified four 'extreme types', viz. the 'H type', or hypothetico-deductive teacher; the 'I type' or inductive teacher; the 'V type' or verificationist teacher; and the 'O type' who has no discernable philosophical beliefs about the nature of science or its methodology.

Again, my interest in this study is for two main reasons. Firstly, I note the pluralism of epistemological commitments across the population of teachers, albeit, (secondly) tempered by the apparent absence of Weltanschauungen, or post-empiricist, notions ('O' types?).
Chapter 6. The Context of my Field-Research

6.1. Introduction

My field-research had dual foci within secondary and tertiary levels of education and used a number of complementary investigative methods. These latter may be grouped under the following three headings:

(1) Lesson Observation (LO)
(2) Written Exercise (WE)
(3) Focussed Interview (FI)

The research was conducted through a series of 'pilot' studies culminating in a 'main' study, though I have felt free to attach equal interpretative worth to material drawn from any stage of the enquiry. The distinction between 'pilot' studies and the 'main' study is made principally by virtue of the coincidence in the development of the final forms of WE and FI, rather than LO.

As my title for this chapter suggests, my overarching purpose within it is to provide a context in which to construe my discussion of these methods. Hence I shall briefly describe the educational institutions in which I conducted the research, my means of entree to them, and my rationale for choosing them and the sample of subjects from within them. Since LO's
were themselves principally intended to provide a context for myself when later employing WE and FI (described in later chapters), I shall interpolate information concerning my use of this technique with other background details prior to discussing the technique in detail later in this chapter.

6.2 Choice of Educational Institutions and Human Subjects

My decision to split the focus of my field-research between secondary and tertiary levels of education was prompted by a desire to explore the possibility that science lecturers' personal meanings of science and scientific-method might be different from those of science teachers by virtue of the former being actively engaged in scientific research, and, further, that this might affect the image of science which they project to their students.

The research was conducted within five schools (middle and secondary) and a university, all of which were located in the south of England.

6.2.1 The Schools

My first choice of school was largely opportunistic – being decided, in the first instance, by an entree facilitated by one of my supervisors who had prior links with that (middle) school.

My choice of the remaining (secondary) schools, however, was made chiefly
by virtue of them all being state comprehensives and their geographical proximity, each to the other and all to my place of residence at the time. With these schools, I effected entree by means of a letter (or a telephone call) to the headmaster, which in some cases was followed by an interview. In these, as with later preparatory letters and meetings which I had with teachers who had expressed a willingness to be observed whilst teaching, I outlined the nature of my intended research and gave assurances that I would strive to preserve the anonymity of all concerned (i.e. staff and pupils), both within and without the school, with respect to my research inferences.

My early pilot research in these schools was conducted over a period of a few days in each and consisted only of informal (i.e. unrecorded) lesson observations and interviews with staff and pupils. At the end of this period I decided to concentrate my research efforts within only one school since I judged the staff and students there to be particularly willing to cooperate with the further ramifications (impositions!) that would be involved with my main study.

The school in question was opened in the late 1950s and was formally recognised by the local education authority as a Community School in the late 1970s. It had a roll of around 1,500 pupils, of whom approximately 200 were in the sixth form. The Science Department taught CSE Integrated Science (IS), 'O' level Schools Council Integrated Science Project (SCISP), and Nuffield 'A' level. The staff of that department were predominantly young and enthusiastic in their teaching.
The LOs of my main study, numbering some 32 in all and observed consecutively, were conducted in a lower sixth 'A' level biology class of 7 pupils and taught jointly (alternately) by Mr. H. and Ms. S.

I conducted WE and FI with all these pupils and with both these teachers (amongst others – see Chapters 7 and 8 for details).

6.2.2. The University

My choice of university was again decided partly by its geographical proximity and partly by my ready entree to it due to having met at a conference one of the lecturers, whose lectures and tutorial groups I later observed (Dr. T). An important additional factor, however, was that this university ran an optional course for undergraduates entitled 'Principles and Perspectives of Science', and I was interested to explore undergraduates' reaction to it.

This university was founded in the early 1960s and has enjoyed in equal measure a reputation for radical student politics and research excellence, particularly in the life sciences. The student population numbers around 3,500 and staff-student relations are widely thought to be good.

I chose to observe first year undergraduates since I felt that temporal proximity to the interface between secondary and tertiary levels of science education might bring epistemological awareness and issues to the fore.
I attended one compulsory course ('Molecules, Cells, and Organisms') for first year undergraduate students in the School of Biological Sciences. This lasted an entire term and consisted of 23 lectures, 8 pre-lab lectures, 8 labs (i.e. practicals), and 8 post-lab lectures.

By observing this course I was guaranteed to share at least some of the immediate formal learning context of students whom I observed within two tutorial groups which met twice weekly and whom I observed throughout the period - 16 Tutorial Observations (TO).

The first of these tutorial groups (TG1) was led by Dr. McG. and was comprised of 5 students, TG2 was led by Dr. T with 4 students.

I conducted FIs with all but one of these students, who was absent from TG2 during the interview period, and with both the lecturers. (I did not use WE with tertiary level subjects).

6.3 Method of Observing Lessons

The method of observation that I used was loosely based upon a model proposed by Schatzman and Strauss (1973).

Using this model, the researcher organises their observation notes into

"[ ] relatively distinct "packages" of material according to whether they constitute "Observational Notes" (ON), "Theoretical Notes" (TN), or "Methodological Notes" (MN)."


These authors later characterise each of their "packages" in the
following way:

**ON:** Observational notes are statements bearing upon events experienced principally through watching and listening. They contain as little interpretation as possible, and are as reliable as the observer can construct them. Each ON represents an event deemed important enough to include in the fund of recorded experience, as a piece of evidence for some proposition yet unborn or as a property of context or situation. An ON is the Who, What, When, Where, and How of human activity. It tells who said or did what, under stated circumstances.

Each ON is constructed as a unit event that can stand by itself as a datum, or can be fully understood in the context of other ON's on any given date or circumstance. If it records actual conversation, the researcher quotes exact words, phrases, or sentences; otherwise he uses the apostrophe (single quotes) to indicate somewhat lesser certainty, or he paraphrases as best he can. If the observer wishes to go beyond the "facts" in the instance, he writes a theoretical or inferential note.

**TN:** Theoretical notes represent self-conscious, controlled attempts to derive meaning from any one or several observation notes. The observer as recorder thinks about what he has experienced, and makes whatever private declaration of meaning he feels will bear conceptual fruit. He interprets, infers, hypotheses, conjectures; he develops new concepts, links these to older ones, or relates any observation to any other in this presently private effort to create social science.

**MN:** A methodological note is a statement that reflects an operational act completed or planned: an instruction to oneself, a reminder, a critique of one's own tactics. It notes timing, sequencing, stationing, stage setting, or manoeuvring. Methodological notes might be thought of as observational notes on the researcher himself and upon the methodological process itself; as complete a chronicle as the recorder finds necessary or fruitful. Were he to plan on writing for later publication about his research tactics, he would take detailed notes; otherwise his MN consists mainly of reminders and instructions to himself'. (Schatzman and Strauss, 1973, pp.100-101).
Use of this system over a number of observation sessions enables the researcher to derive 'thematic categories', (i.e. classes of events deemed interesting to the researcher).

I recorded my observations in a 15 x 21 cm. notebook (usually) while sitting at the back of the classroom. In addition, I audio-recorded the LO. Wherever possible, I entered both the time and the tape rev-counter number next to ON's on the pages of the left hand side only, leaving the right hand side free for TN and MN. As was to be expected, some of these only occurred to me after the actual LO - here I found the audio-recording of particular help in stimulating my recall of events.

During LOs I did not converse with either students or staff. After a few sessions I got the impression that the pupils and students took no significant account of my presence. On one occasion in the school, for example, when the teacher (Mr. H) had left the room for a few minutes the pupils openly cheated in a test they had been set. In undergraduate practicals students likewise soon got used to the "mute demonstrator"!

I augmented my LOs by collecting and examining all teaching materials used and also the educands' files - this constituting an informal form of documentary analysis. I also wrote (after the LO) short synopses of the lessons using basic categories for ON/TN for didactic tactics such as 'pupil activity' and 'guided discussion'. Finally, I would invite the teacher to talk about the lesson they had just presented if there was an opportunity to do so.
6.3.1. Example of a Lesson Observation

The lesson that I have chosen to illustrate my method of observing classes and the sort of research inferences I felt able to make was given by Ms. S to a third year SCISP group upon the subject of 'The Action of Saliva upon Starch'. Although this LO occurred during an early pilot stage of my field-research, I nevertheless present it as my example because it was instrumental in my decision to switch the emphasis of my investigation away from educands' and educators' alternative-conceptions 'in science' and towards their alternative-conceptions of science.

Synopsis of the Lesson

This lesson, and the one preceding it, comprise the third year digestion syllabus.

Teacher begins with a recapitulation of previous lesson. That lesson had itself been largely devoted to revising the human digestive system conceived in basic terms (see Appendix 4 for second year digestion syllabus). The form and function of villi had been introduced (see Appendix 4 for teaching materials used).

Guided discussion: Teacher's questions introduce control necessity of experiment. Experimental design 'negotiated':

- 6.8 -
Each boiling tube plus contents immersed in water bath (37 deg. C) for 15 minutes. Contents of visking tubes then tested for presence of starch and sugar. Teacher distributes 'Patterns 1' text-book. Advises pupils to refer to relevant section for details of method upon which class experiment is based (see appendix for photo-copy).

Teacher requests pupils to work in groups and urges task-sharing within conduct of experiment.

**Pupil activity:** set up experiment. Teacher supplies methodological advice on request or need.

**In the last 5 minutes:** Teacher advises that not enough time to carry out
starch and sugar tests. Will therefore leave experiment running until next lesson.

**Homework:** Teacher requests pupils to (1) write notes on how food enters blood-stream, (2) write up class experiment thus far conducted. She stresses the need to include methodological details not included in her blackboard diagram of the apparatus in order to enable replication of experiment by a naive third person.

(N.B. First 15 minutes of next lesson allowed for pupils to conduct starch and sugar tests. Short discussion of results).

---

**Figure 6.2:** Photoreduction of a Page from my L0 Note-Book Recorded during this Lesson
In an informal interview with Ms. S. after this lesson, I was able to
glean something of her personal (or professional) epistemology of science
and the role it played in her teaching when I asked her about an aspect of
the lesson:

I 'In the lesson the children seemed quite at home with the design of
the experiment. Have you ever specifically - you know
abstractly - taught them the concept of control?'

Ms. S 'Not control as such but I do teach them the basic scientific-
method: Aim (can double as a title), Apparatus (if special),
Method, Result, Conclusion. I try to get them out of all that
personal stuff - "Miss S(.) did this, I put that on there"
etc. I try to train them in the scientific way - to be as
efficient and as laboratory like as possible. Most of my kids
have now got the method pretty well but are still too rowdy
really'.

Miss S. labelling and sequencing of stages in scientific-methodology, her
repeated suppression of the human subject and use of the definite article
suggest a sympathy for classical empiricist epistemology. (also cf.
Medawar, 1964, for arguments that this format of writing scientific
papers misrepresents the processes of scientific reasoning and
investigation, viz. as empiricist-inductivist, and as such perpetrates a
'fraud').

The informal interview continued:

Ms. S 'We have a lesson devoted to it in the second-year - well, and the
laboratory safety. Its usually the first or second lesson of term
so we also have to get all the files handed out and generally tell
them what's what'.

I believe Ms. S' comment is indicative of the generally low status
ascribed to scientific-epistemology within the school science curriculum, viz. part of one lesson. She termed this her "paradigm lesson".

Ms. S. also claimed that

'The sort of science they get further up the school messes up all my good work (...) they mess up all my ground work ... I don't like what they do up there really ... dilutes it'.

Ms. S.' class was a third year SCISP class. "Further up the school" would bring pupils closer to the philosophical orientations of SCISP and Nuffield 'A' level. Although not without faults, I regard those approaches as being more philosophically enlightened than Ms. S.' rather traditional (i.e. empiricist-inductivist) one.

Further support for my interpretation that Ms. S. intended to impart an empiricist-inductivist image of science came from her treatment of history of science in the written work of her pupils.

When I examined her pupils' work-files I found that Ms. S. had only required notes on the history of science to be recorded on two occasions over a 9 month period.

The first of these occasions consisted of dictated notes concerning the development of atomic theory:

'The Atom

About 400 B.C. a Greek called Democritus said that matter was composed of atoms. He said they were all the same, but combined in different ways to make different substances. He also believed that atoms could not be divided or destroyed.
The first real atomic theory was found in 1803 by John Dalton. He said that different chemical elements were composed of different atoms. He also believed that the atom could not be divided.

Modern atomic theory was formulated by Lord Rutherford in 1911. He said that the atom is composed of a central nucleus surrounded by circling electrons:

The nucleus is made up of two different particles, protons and neutrons. There is always the same number of protons as there are electrons in an atom. Every element has a different number of protons in its nucleus:

The number of protons = atomic number of the element

An example is Hydrogen [.....]

I suggest that this first rare inclusion of history of science within Ms. S.' lesson may be construed to illustrate the way in which accounts of the elaboration of scientific theories complements an 'accumulative fragmentalist', i.e. empiricist-inductivist, epistemological image of science. Earlier theories are never actually rejected - they are just made more detailed. This is possible (in this example) only by strictly adhering to the particulate theory of matter and, hence, by making a highly selective appeal to history of science.

The second occasion on which Ms. S. had required notes on the history of science to be made consisted of an essay she had set for homework. This was simply entitled 'Sir Isaac Newton' but issued with the instructions 'write an essay on his life and work'.
When I read the pupils' essays, I found it instructive to note the sort of statements 'allowed' by Ms. S. I resisted the temptation to draw inferences relating to the personal epistemologies of science of the pupils themselves from this source, because I believe that many had simply copied from books. Whilst this was suggestive of the general epistemological orientation of text book and encyclopaedic accounts of Newton's achievements, no pupil included a bibliography at the end of his essay, so I did not pursue it. Cawthron and Rowell (1978) however, have drawn attention to the empiricist-inductivist slant to many science textbooks.

I construed most pupils to reconstruct Newton's contribution to science in empiricist-inductivist terms as evidenced by statements such as the following:

Pupil A '(...) a scientific law is a statement which explains the results of an experiment. The study of how forces act on objects are summed up in Newton's three laws of motion. (...)'.

Pupil B '(...) his calculations about the laws of gravity at this time agreed pretty nearly with the known facts, but to Newton this was not good enough, so he laid aside the study of gravitation for the time being. (...)'.

Pupil C '(...) it is said that while in his garden at Woolsthorpe he saw an apple fall from a tree, then he wondered why the apple fell straight downwards. Then he decided that the Earth attracted the apple. This led on to the discovery of Gravity. (...)'.

In all these accounts theory is subservient to 'facts'.

In some pupils' scripts, however, there were references to rival theories, the epistemological implications of which were left ambiguous.
(...) he gave a paper reporting his works on optics and fell fowl [sic] of Robert Hook, who upheld the wave theory of light as against Newton's corpuscular theory. They were both right in their own way (...).

I suggest that Ms. S. and perhaps a minority of her pupils also, might be caught in a 'double-bind' viz. on the one hand, accepting the prevalent contemporary philosophic notion that there is no such thing as an absolute fact, yet on the other hand endorsing with the collusion of many aspects of our Western culture, a methodology which is founded upon direct access to reality and the attainability of absolute-knowledge.

Ms. S.' apparent fluctuating stringency in her "enforcement" of empiricist-inductivist epistemology may also be due to her perceived area of competence: her own academic background was in botany rather than in physics.

In project work, pupils from both classes often recorded historical details. I believe, however, that these were either in the cause of an empiricist-inductivist reconstruction, or, more often, merely included as "humanistic tit-bits" without epistemological import, e.g. '[so-and-so], who married four times, did [such-and-such].'

To conclude, this account records the sort of interpretations which I made at the beginning of my field-research. It is not sophisticated, yet is probably over-interpretive. This notwithstanding, such early research contributed to my growing conviction that educands' and educators'
personal epistemologies of science were worthy of being investigated.

Finally, I was able to explore Ms. S.' professional epistemology of science in more detail in an FI at a later date.
Chapter 7. Written Exercises

7.1 Introduction

The work that I describe in this chapter places heavy reliance on class administered 'written exercise' (WE) techniques to investigate students' personal understanding of the following three subjects:

(1) a "scientific job"
(2) a "scientific activity"
(3) a "scientific observation"

I shall describe my use of these investigative instruments in the order that I have just introduced them:

7.2 Written Exercise: Content, Collaboratee Sample and General Method of Administration

I administered WE on four occasions. Between administrations of this exercise I undertook minor revisions to the wording that I characteristically used and also added two questions. This resulted in my administering 3 versions of WE (WE.v1, WE.v2, WE.v3). The final and complete version, i.e. WE.v3, may be summarised as follows:

Q1 List jobs which you would normally put under the headings
of very 'scientific', or very 'non-scientific', or
definitely 'both scientific and non-scientific':

\[
\begin{array}{ccc}
\text{scientific} & & \text{non-scientific} \\
\downarrow & & \downarrow \\
\text{both} & & \\
\end{array}
\]

Q2 Why do you think that I have asked you to answer Q1?

Q3 What is your most usual reason for putting a job in the
scientific column?

Q4 Have you found it useful to answer Q1?
- If "yes", why?
- If "no", why?

Q5 Do a sketch of something that happened outside school in
which you did something scientific.

B Q6 What is happening in your sketch?

Q7 Why is it scientific?

Q8 Carefully observe, then write down what you have observed,

C (Q8a What difficulty had you in trying to answer Q8?).

(Q8b Observations are often very important in experiments. Does the difficulty that you had in trying to
answer Q8 tell you anything about people doing experiments that have never been done before?).

Figure 7.1. Showing Contents of WE.

(N.B. All three versions may be found and compared in Appendix 5).

As I have indicated in my summary above, I consider these questions to
fall into three clusters relating to my three areas of interest, i.e. Q1-4
re. 'A', Q5-7 re. 'B', Q8-8b re. 'C'. I also consider Q1, 5 and 8 to
represent the 'key' questions respective to my three areas of interest.

I administered WE to four Secondary level classes and an abbreviated form
of WE (WE.v4) consisting of Q1 only, to a randomised sample of 37 first
year undergraduate students (see Appendix 5).
Two of the classes to which I administered the WE were ones which I had previously observed in a lesson. Furthermore, I later conducted 'focussed interviews' (FI) with a small group of collaborators from two classes (one class I had observed in a lesson [LO], the other I had not. This was to place my research inferences generated from the written responses to WE in an overall context and to thereby render it as "holistic" as possible.

I selected the two classes that I had not previously observed opportunistically - according to the teacher's willingness to cooperate and within strictures imposed by the timetable.

Entree to each of the classes (for the purpose of administering WE) was effected via the consent of the teacher and a previously granted request made to each class by the teacher.

Basic details of my collaborator sample are given in Figure 7.2 below. In this Table, I present the classes in the order in which I administered WE to them:

<table>
<thead>
<tr>
<th>WE</th>
<th>Educ lev</th>
<th>Cl No</th>
<th>Yr</th>
<th>T</th>
<th>Ac</th>
<th>Sc course</th>
<th>St No</th>
<th>WE &amp; Q1</th>
<th>WE v1</th>
<th>WE v2</th>
<th>WE v3</th>
<th>LO</th>
<th>FI</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
<td>4</td>
<td></td>
<td>Mr.F</td>
<td></td>
<td>Mixed Ability</td>
<td>22</td>
<td>22</td>
<td>22</td>
<td>2</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>2</td>
<td>3</td>
<td></td>
<td>Ms.S</td>
<td></td>
<td>SCISP</td>
<td>24</td>
<td>24</td>
<td>24</td>
<td>1</td>
<td>2</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>3</td>
<td>4</td>
<td></td>
<td>Mr.B</td>
<td></td>
<td>IS</td>
<td>16</td>
<td>16</td>
<td>16</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>4</td>
<td>4</td>
<td></td>
<td>Mr.A</td>
<td></td>
<td>Mixed ability</td>
<td>17</td>
<td>17</td>
<td>17</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Totals/Sub-totals</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>79</td>
<td>79</td>
<td>22</td>
<td>24</td>
<td>33</td>
<td>2</td>
<td>7</td>
</tr>
<tr>
<td></td>
<td>3ry</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>37</td>
<td>37</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Totals</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>116</td>
<td>116</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Figure 7.2. Table Showing WE Data Sources
It should be noted that Ms. S's class constitutes something of a 'special case' in so far as I used all my investigative approaches with them, at least in some form or another.

It may also be worth mentioning at this point that my initial, sole, intention for WE was to obtain a set of representative elements for later use in a 'repertory grid technique' (RGT), i.e. by means of Q1, as, indeed, I did (see section 7.3. below). However, I considered the opportunity to augment the RGT investigation to be an opportunity that was too good to be missed. Questions 1-3, however, may still be considered to be an investigation in their own right, i.e. capable of analysis and interpretation without reference to the RGT investigation. As I shall later describe in more detail, I administered the grid itself to a different collaboratee sample, viz., two fifth form classes. My write-up of questions 1-3 incorporates my RGT investigation.

My desire to elicit views genuinely held by pupils meant that I was anxious lest they perceive WE as a 'test', which, indeed, it was not. With this in mind I characteristically introduced WE as a 'quiz to find out what people in schools think about some aspects of science'. For similar reasons, I also stated that 'there are no right or wrong answers - your set of answers is not something that I can go away with and mark out of 10'. To help gain feedback as to the success, or otherwise, of these intentions between successive administrations of WE included Q2.

With my verbal introduction completed I distributed three, A4 sheets of
to each student. These sheets were stapled together in the top left hand corner. The first and third of these were lined, the second was plain (for response to area 'B'). I then requested each student to record his or her first name at the top of the page. This was in order to render them identifiable to me since responses which I found interesting contributed to my selection of students for interview. This notwithstanding, I guaranteed each student's anonymity both within and without the school (for later reference, see Appendix 6 for an example of a completed WE).

I then wrote WE on the blackboard using the wording that I have already presented in Figure 7.1. I did this one item at a time, adding a verbal explanation for further clarity, answering any queries voiced by members of the class and allowing time for completion of responses by the whole class before continuing (this last was to be methodologically important with respect to area 'C' - section 7.4 below). WE took approximately 35 minutes to administer.

My detailed consideration of areas 'A', 'B' and 'C' of WE correspond to sections 7.3, 7.4 and 7.5 which follow.
7.3 Written Exercise, Part A: Students' Personal Meanings of a "Scientific Job"

7.3.1 Introduction

I infer my principal stimulus to investigating students' personal meanings of a 'scientific job' as a means of exploring their personal meanings of science and scientific method from my experience of early interviews. In those, I had noted that when I asked the collaborators what they personally understood by the terms 'scientific' or 'unscientific' many listed and classified occupations or subjects prior to answering. This appeared to assist them to clarify and formulate their views so I decided that it might be fruitful to mimic this tendency as a research strategy. Moreover, I considered that this approach might render issues of science epistemology more accessible, at least at the linguistic level, to some students.

My investigations in this area began with a version of 'Interview-About-Instances' technique (Osborne and Gilbert, 1980). For reasons given in section 7.4.2.1, however, I found Repertory Grid Techniques (RGT) more suitable.

Before describing my elicitation and analyses of elements and grids, I shall make some general comments concerning RGT in order to make clear what I consider to be their relation to Constructive Alternativeism.
7.3.1.1 Repertory Grid Techniques and Personal Construct Psychology

Repertory Grid Techniques are a set of techniques devised to facilitate a process of inquiry in a manner consistent with the philosophical stance of Constructivist Alternativism, as elaborated by George Kelly (1955) and as I have already discussed in detail (Ch.3 and 4).

Much of the nature, purpose and limitations of RGT may be understood by reference to both the 'ethos' and the formalisms of Kelly's theory since they represent one practical expression of it - in this sense, perhaps, more than mere consistency is implied. At the risk of repeating some of my earlier expressed views, however, I shall argue for these more recent ones rather than assume them. First I shall develop and clarify the main point that I shall argue for.

As we have seen, Kelly formulated his theory whilst employed as a clinical psychologist. He characterised his overarching aim for PCP in the following way:

'We are concerned with finding better ways to help a person reconstrue his life so that he need not be a victim of his past' (Kelly, 1955, p.23).

This expressed aim establishes Kelly's 'humanistic intentions', for no mention is made of externally imposed constraints or norms on the subject-person or 'collaboratee' - it is 'client centered'. Interventions into the life of another person, then, must be "catalytic", "invitational" or "facilitative", rather than "normative". Even these, however, require
that one first acquires an "understanding" of the person:

'If we reach an understanding of how a person behaves, we discover it in the manner in which he represents his circumstances to himself'.
(Kelly, 1955, p.16, my emphasis).

This construal of 'understanding' reflects Kelly's constructivist conception of 'scientist' and of all persons in their scientist like aspects. For Kelly, to know is to construe - and to know another person is to know how they construe. This last part implies that we must somehow elicit and consider that other person's constructs. Kelly developed the original form of RGT as one means of doing this: RGT are a response to his own exhortation that:

'...humanistic psychology needs a technology through which to express its humane intentions. Humanity needs to be implemented, not merely characterised and eulogised'. (Kelly, 1969, p.135).

Of the many investigative techniques whose underlying rationales are compatible with the philosophical stance of Constructive Alternativism, RGT are, perhaps, the best known, most used and most directly related. The closeness of this relation becomes clear in certain of Kelly's views in which he develops his notion of a 'construction matrix':

'By a construction matrix I mean a postulated grid in which events and abstractions are so interlaced that whatever appears to occur independently of one's intention is given meaning in depth by being plotted against whatever coordinate reference axes he has intentionally erected. And in this psychological hyperspace the humanly contrived axes of reference, in turn, acquire whatever objective significance they have through extension - or through "operationalising" if one prefers a term that has more current usage.
This is to say that human constructions derive their objectivity wholly from the way they cast events into varying arrays - or simply from the lines of perspective they provide. Actually it is in terms of such arrays that consensual judgment becomes psychologically possible. Consensus itself, while often cited as the criterion of objectivity, does not properly define the psychological grounds on which objectivity rests. Only sociological grounds are implied.

But now, since we are talking about human experience, including our own particular experience as scientists, it may be more precise, instead of saying that the matrix is a schema in which events and abstractions are interlaced, to say it is man's observations and his constructs that are woven into the fabric of experience - the one ascribing meaning to the other and the other lending palpability to the one. And in this more phenomenological sense the grid might better be characterised as a "repertory grid," since it expresses one's own infinite system of cross-references between the personal observations he has made and the personal constructs he has erected. I suppose it is apparent that all of us must have quite limited repertories, for the events we encounter are experienced only in such depth as our constructions will plumb, and our constructs have only that scope which is provided by the ranges of events to which we undertake to apply them.' (Kelly, 1969, p.290-1: original emphasis).

Kelly is here proposing the notion of 'Repertory Grid' qua meta-theoretical entity, as part of the 'assumptive structure' of his general theory. When elaborated by means of his 11 corollaries, it constitutes what some might call the 'cognitive structure' (I suggest that 'constructive structure' would be a more appropriate expression) that Kelly assumes all persons to develop, albeit to greater or lesser extents. As shall become clear, this notion is intrinsic to that of Repertory Grid techniques, or, Repertory Grid qua humanistic technology.

Easterby-Smith (1981, p.3) argues that RGT exhibit 'three essential
features*, viz., 'elements*, 'constructs' and a 'linking mechanism which shows how each element is described in terms of each construct'.

The expression of links between elements and constructs varies with the format of RGT used (cf. discussion of my application, below). All, however, are principally informed by Kelly's 'Organisation Corollary' without which RGT would be impossible:

"Each person characteristically evolves, for his convenience in anticipating events, a construction system embracing ordinal relationships between constructs"
(Kelly, 1955, p.56: my emphasis).

The Organisation Corollary proclaims Kelly's view that, at any moment in time, a person possesses a repertoire, or set, of personal constructs and that such a repertoire is not to be construed as a mere "aggregate" of their (his/her) personal constructs for each construct is held to be related to every other construct in a predominately (but cf. Fragmentation Corollary: Kelly, 1955, p.87 et seq) coherent and hierarchical manner.

Now, there is no reason in principle why a person's entire system of personal constructs - their "total personal repertory grid" - should not be elicited. In practice, however, this is not possible. Practical constraints have mainly to do with elicitation procedures which are presently too slow, or to put it another way, persons' total repertoires of personal constructs are characteristically too large, though finite. Moreover, a person's construct system is not a static edifice: it would
very likely change during the period of elicitation rendering a "complete" elicitation impossible: a person does not cease to be 'a form of motion' (Kelly, 1955, p. ) merely because they are having their constructs elicited. This points to an important limitation on the inferences that can be drawn from an elicited grid, viz., it constitutes a representation of (an aspect of) a person's ideational world that holds good only at the temporal point of elicitation. Under no circumstances can RGT be used as a 'test'.

More important than practical constraints, however, is the notion that were a complete elicitation possible it would probably not be desirable. This is because it is difficult to imagine a purpose, or a situation, where a person's entire construct system could be pertinent (again cf. Fragmentation Corollary). In using RGT, however,

'Oone is not aiming to encapsulate the whole of an individual's construct system but that part of it which is relevant to the defined purpose'. (Pope and Keen, 1981, p.44-5: original emphasis).

Paraphrasing this comment, we can see that RGT serve to make explicit only those personal constructs, together with their relational structure (defined according to the assumptions of PCP), that are relevant to a previously negotiated purpose.

Pope and Keen argue that 'there is no such creature as "The Grid",' (1981, p.37) applicable for all purposes and propose that five major considerations will have a bearing on the specific format of grid chosen (or developed) and used, viz.:
These authors go on to elaborate two aspects of 'purpose':

(a) What is the topic to be investigated?
(b) What is the intended use of the grid information?

I consider myself to have provided a preliminary answer to '(a)' in my introduction to this section. Pope and Keen (1981) however, then distinguish five uses for which RGT have been developed:

(i) A conversation with one's self;
(ii) Gathering of information about an individual's views on a particular topic;
(iii) A comparison of the viewpoints of two people in terms of either:
    (a) degree of agreement between them, or
    (b) the degree to which either can gauge the other's point of view;
(iv) An exploration of the nature and sharing of construing within a group;
(v) A monitoring of changes in perspective.
I considered my interests (intended use of grid information) to lie with items (ii) and (iv). This corroborated my choice of FOCUS and SOCIOGRIDs programs, developed at Brunel University (see, e.g., Thomas and Shaw, 1976; Pope, McKnight and Thomas, 1978) as the most appropriate existing grid formats to explore my interests. I shall describe briefly the character of each of these programs later.

7.3.1.2 Method of Eliciting and Analysing and Interpreting Grid Information

I elicited data in two stages:

(a) elements
(b) grids

7.3.1.2.1 Elicitation of Elements

Elements in any grid should be judged by elicitor and elicitee as being representative of the 'universe of discourse', i.e. they should hold personal meaning within the problem area to be explored. This requirement creates special problems when elements must be provided, as was the consequence of my decision to explore issues of commonality of construction amongst students, for they must now be regarded as representative by more than one person. There are some who argue that both elements and constructs should be elicited on an entirely individual basis and point to the many abuses of RGT in the other approach.
For my part, I consider that providing elements or constructs (or both) does represent a departure from the "ethos" of Constructive Alternativism, at least, in its "purest" form. But I also believe that, under certain circumstances, such departures may be justifiable. Exactly what these circumstances are, however, have to be considered with each such application and responsibility resides, very largely, with the applier. This notwithstanding, it would seem that, in such cases, extra care has to be taken to ensure that the universe of discourse is adequately negotiated with the persons from which the "source-set" of elements are initially elicited and that the set of provided elements derived, derived from the source set, exhibit contrast.

I obtained my source-set of elements by means of Q1. My verbal instructions for eliciting elements were very similar to those which I wrote on the blackboard and which I have already presented. I did, however, particularly emphasise that there was no virtue in listing large numbers of occupations unless the respondent considered each to be a very good exemplar of the relevant category, i.e. 'scientific' or 'non-scientific' or 'both scientific and non-scientific'. As with each WE question, I also re-emphasised that there were 'no right or wrong answers'.

I initially derived what I judge to be representative sets of elements from the Secondary and Tertiary students separately. This was in order to establish whether there were large differences between the trends of responses from these populations.
I began by taking the first script from the pile and then, in the order presented, noted down the occupations that the respondent had listed for each category on a 'data analysis sheet'. I entered a dash (tallymark) to the right of each occupation to signify its occurrence. I repeated this procedure with the remaining scripts. Where an occupation recurred I only entered a tallymark. I added "novel" occupations, together with a tallymark, to the appropriate list and numbered the occupations in the left hand margin.

In the early stages of analysis I wished there to be minimal data degradation so I corrected only spelling when adding occupations to the lists, see Figure 7.3 below:

<table>
<thead>
<tr>
<th>Secondary</th>
<th>Scientific</th>
</tr>
</thead>
<tbody>
<tr>
<td>Scanner, Teacher</td>
<td>Lab. Tech.</td>
</tr>
<tr>
<td>Pharmacist</td>
<td>Doctors</td>
</tr>
<tr>
<td>Electrician</td>
<td>Scientist</td>
</tr>
<tr>
<td>Surgeon</td>
<td>Computer programmer</td>
</tr>
<tr>
<td>Chemist</td>
<td>Photographer</td>
</tr>
<tr>
<td>Astronomer</td>
<td>Physicist</td>
</tr>
<tr>
<td>Professor of science</td>
<td>Drama student</td>
</tr>
<tr>
<td>Professor</td>
<td>Technician</td>
</tr>
<tr>
<td>Computer</td>
<td>&quot;Program&quot;</td>
</tr>
<tr>
<td>&quot;Program&quot;</td>
<td>&quot;Experimental&quot;</td>
</tr>
<tr>
<td>TV Engineer</td>
<td>&quot;Audio Engineer&quot;</td>
</tr>
<tr>
<td>&quot;Audio Engineer&quot;</td>
<td>Researcher</td>
</tr>
</tbody>
</table>

Figure 7.3. Showing an excerpt from a data analysis sheet
I soon judged, however, that some 'clustering' of the data prior to recording was desirable to avoid unnecessary repetition and labour. This 'first order clustering' was done with 'novel' occupations which I judged to be synonymous with one already included in the list. Most trivial and unproblematic was the use of singular and plural forms. In certain other cases more judgement was needed but I feel confident that this has not distorted the data more than the following examples:

- e.g. 'brain surgeon' = 'neurosurgeon',
- e.g. 'secretarial work' = 'secretary',
- e.g. 'teaching' = 'teacher'.

Where occupations appeared to be different but related (examples respective to the last 3, above, include 'surgeon', 'typist', and 'lecturer') I made a note to myself in the left hand margin - 'see 5, 12' etc. This was in anticipation of later, 'second order clustering'.

Since second order clustering would further degrade the data, albeit in the cause of improved representation, I first obtained a set of 12 occupations from the first order data. I felt that this number would be adequate to function as elements giving rise to grids of sufficient depth and detail. I also judged that 12 elements was the maximum that could be used within the known time limit for construct elicitation (approximately 1 hour) and the nature of the respondents (conscripted pupils and students). I selected these occupations - elements - by identifying those four which occurred most frequently in each category. I then ranked each
set of four occupations in order of frequency (highest or lowest) prior to presenting them in tabular form. The results of this procedure are shown in Figure 7.4 below:

<table>
<thead>
<tr>
<th>Secondary (n=79)</th>
<th>Tertiary (n=37)</th>
<th>Category</th>
</tr>
</thead>
<tbody>
<tr>
<td>Scientist</td>
<td>Engineer</td>
<td>'scientific'</td>
</tr>
<tr>
<td>Doctor</td>
<td>Doctor</td>
<td></td>
</tr>
<tr>
<td>Science teacher</td>
<td>Physicist</td>
<td></td>
</tr>
<tr>
<td>Chemist</td>
<td>Chemist</td>
<td></td>
</tr>
<tr>
<td>Dustman</td>
<td>Musician</td>
<td>'non-scientific'</td>
</tr>
<tr>
<td>Shop assistant</td>
<td>Shop keeper</td>
<td></td>
</tr>
<tr>
<td>Secretary</td>
<td>Politician</td>
<td></td>
</tr>
<tr>
<td>Milkman</td>
<td>Accounting</td>
<td></td>
</tr>
<tr>
<td>Teacher</td>
<td>Teacher</td>
<td>'both'</td>
</tr>
<tr>
<td>Doctor</td>
<td>Nurse</td>
<td></td>
</tr>
<tr>
<td>Engineer</td>
<td>Farmer</td>
<td></td>
</tr>
<tr>
<td>Dentist</td>
<td>Policeman</td>
<td></td>
</tr>
</tbody>
</table>

The frequency of each occupation is given in brackets.

Figure 7.4. Table showing the four most frequent occupations for each category drawn from FIRST order clustered data for secondary and tertiary populations.

Second order clustering appeared desirable due to the high degree of overlap between, and equal frequency of, many of the occupations.

To derive second order clusters I collected occupations which I judged to be highly related and added their frequencies together. I then adopted one of the occupations (usually the most frequent) or invented a new generic expression to identify the cluster. The following two examples, drawn from the 'scientific' category of the Secondary education population, illustrate this procedure:
<table>
<thead>
<tr>
<th>Second order cluster</th>
<th>First order cluster</th>
</tr>
</thead>
<tbody>
<tr>
<td>E.g. Medical work (76)</td>
<td>Doctor (35),</td>
</tr>
<tr>
<td></td>
<td>Surgeon (7),</td>
</tr>
<tr>
<td></td>
<td>Neurosurgeon (5),</td>
</tr>
<tr>
<td></td>
<td>Medical officer (1),</td>
</tr>
<tr>
<td></td>
<td>Optician (1),</td>
</tr>
<tr>
<td></td>
<td>Nurse (10),</td>
</tr>
<tr>
<td></td>
<td>Vet (6),</td>
</tr>
<tr>
<td></td>
<td>Dentist (10),</td>
</tr>
<tr>
<td></td>
<td>Radiographer (1).</td>
</tr>
</tbody>
</table>

| E.g. Technician (12)         | Technician (6),     |
|                             | Lab. technician (6). |

Where I judged myself to be using the same criteria for inclusion in second order clusters I kept the generic name the same across Secondary and Tertiary populations.

When I had identified and named the second order clusters I selected the four most frequent such clusters from each category of each population and ranked them as before. The results of this procedure are shown in Figure 7.5:
The frequency of each occupation is given in brackets.

**Figure 7.5. Table Showing the four most frequent occupations for each category drawn from SECOND order clustered data for secondary and tertiary populations**

Even after second order clustering I still had to make some "arbitrary" choices for inclusion of occupations in Figure 7.5. For example, 'pilot' shared equal frequencies with 'farmer', 'psychologist' and 'policeman'. I do not feel, however, that this prejudiced my overall approach and intentions. From an impressionistic interpretation of the data analysis sheets and from comparisons within Figures 7.4 and 7.5, the responses from each population to Q1 of WE appeared relatively homogenous. On the basis of this judgement I felt it reasonable to select occupations from Figure 7.5 to function as a common set of supplied elements in a SOCIOGRID to be administered to either a Secondary or Tertiary population (although, in the event, only to the former).

My selection of occupations was guided by the need to represent each category and the minimum number that has been recommended as being

I straightforwardly included all occupations which were common to each population (i.e. 'Medical work', 'Engineer', 'Driver', 'Shopwork', and 'Teacher'). I excluded some. For example, although 'scientist' was the most frequent response in the 'scientific' category of the secondary education population, I felt that this response was tautologous and would do little to identify the 'universe of discourse' during construct elicitation later. I tend to regard this response as an "unthinking" one - spurred by the novelty of the question being asked. Of the 40 who included this response, 28 recorded it as their first occupation. All but two of these 28 went on to elaborate other more specific occupations in this category. Incidentally, four undergraduates included this response in the same category! I also excluded 'science teacher'. This was because I had already selected the more generic 'teacher' (see above).

Drawing on previous research experience I chose four more occupations which I felt would be useful for the purpose of my sociogrid, viz. the investigation of commonality of students' personal meanings of a scientific job. Accordingly, I selected three out of these four occupations from the 'scientific' category.
My final selection of occupations was as follows:

<p>| | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Engineer</td>
</tr>
<tr>
<td>2</td>
<td>Shop worker</td>
</tr>
<tr>
<td>3</td>
<td>Computer work</td>
</tr>
<tr>
<td>4</td>
<td>Physicist</td>
</tr>
<tr>
<td>5</td>
<td>Secretary</td>
</tr>
<tr>
<td>6</td>
<td>Medical work</td>
</tr>
<tr>
<td>7</td>
<td>Driver</td>
</tr>
<tr>
<td>8</td>
<td>Biologist</td>
</tr>
<tr>
<td>9</td>
<td>Teacher</td>
</tr>
</tbody>
</table>

Figure 7.6. List of occupations selected for SOCIOGRID elements (presented in randomised order)

I randomised the sequence by first numbering the occupations and then asking a colleague, who had not seen the list, to call out all numbers one to nine in any order.

7.3.1.2.2. Elicitation of Constructs

Having selected a set of elements to be supplied which I felt confident and justified would be representative, I set about eliciting repertory grids to explore the matter further.

My collaboratee sample for the grids comprised a total of 21 persons who constituted the entire members of two 5th/lower 6th form classes (average student age of 16 years) from school 1. I refer to these classes as '5th/lower 6th' since these students had completed their 5th year studies, including their GCE examinations, but were, at the time I elicited grids from them, being "introduced" to the 'A' level of their subject (Biology) during the last three weeks of term. This period was described by one teacher (Mr. F., C1RG1) as being 'mainly a sort of
P.R. job to see if they might be interested in taking the 'A' level.'

As with my elicitation of elements, I achieved entree to each of these classes (for the specific purpose of eliciting grids) via the prior consent of both the teacher and a previously granted request made to each class by the teacher.

At the time which I elicited the grids, I had previously observed one of the classes and, to further assist my interpretation of the grid information once elicited, I conducted a "group interview" (GI) with a sample of students from each of the classes.

Basic details of my RG collaboratee sample are summarised in Table 7.7 below:

<table>
<thead>
<tr>
<th>Cl.RG</th>
<th>T</th>
<th>Student Sub-totals</th>
<th>GI</th>
<th>LO</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cl.RG1</td>
<td>Mr. F</td>
<td>9</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Cl.RG2</td>
<td>Dr. O</td>
<td>12</td>
<td>(2)</td>
<td></td>
</tr>
<tr>
<td>Totals</td>
<td></td>
<td>21</td>
<td>7</td>
<td>6</td>
</tr>
</tbody>
</table>

Figure 7.7. Table Showing Class Repertory Grid Data Sources

My elicitation of the grids from each class was conducted in two stages.

In the first stage, which occupied one lesson period (1 hr), I met the students and discussed Kelly's ideas concerning PCP and the specific procedures I intended to use to elicit constructs from them.

I requested students to complete a "practice grid" in order to give them
personal experience in the processes of negotiation and elicitation. From each class I elicited and negotiated a class set of elements as well as eliciting constructs from each individual. I felt it worthwhile to elicit a "class" set of elements for the practice grid since I wished each student to understand the way in which the set of supplied elements in the "study grid" had been obtained (with each class I negotiated a purpose for the practice grid).

I distributed blank grids to each individual and elicited constructs by means of the triadic technique, viz., by offering the collaboratee a randomly derived list of triads (of elements), presenting them with a triad from that list and asking them to say in which way two of the elements were alike and, thereby, different from the third element. I asked each student to name both the emergent pole, i.e. the way in which two of the elements were alike, and also the implicit or contrast pole, i.e. what made the single element different from the pair. This they recorded in their grid form. Next, I asked each student to order the elements on a construct rating scale of 1...5 and to record their allotted values in their grid form.

In my explanation of the above points, and some others, I simplified some aspects of Kelly's terminology: for example, I referred to the 'triadic elicitation technique' as the 'splitting principle'. I wrote up terms and their definitions on the blackboard. By this means I tried to make the meanings and intentions that I had for the following terms, ideas and procedures especially clear:
(1) 'element'

(2) 'construct' (and triadic elicitation technique) - defined as per Kelly, 1955, p.61. Also the notion of 'opposite contrast' (this to reduce the tendency I had found during early pilot testing of the technique, to use 'X vs. Not X' formulations),

(3) 'rating scale', especially the notion that the two elements of the emergent pole need not necessarily both be rated '1',

(4) can re-rate a construct (hence I asked collaboratees to complete their grids in pencil),

(5) can re-name either or both of the poles of a construct(s),

(6) can add another construal of any triad,

(7) 'full context'

Before collaboratees began completing their practice grid, I first created and rated a construct using a triad from the elements we had negotiated (this in order to provide them with a demonstrated example). I then asked them to start eliciting their grid by re-construing the elements I had
chosen (i.e. the first triad from my list) for their first construct.

In the first class (i.e. as per Mr.F.- see Figure 7.7 above), the students construed makes of motor car for the following purpose: "what makes a motor car suitable for the family?". For an example of a raw practice grid, completed by a student (Heather, C1RG1), see Appendix 7.

The day after each class had completed practice grids I elicited the grids proper. An example of a completed raw grid (again for Heather, C1RG1) is given in Appendix 8. Figure 7.8, below, however, represents an illustration of part of that raw grid (elements used in each triad are referred to as E1...E9; constructs as C1...Cn):

7.3.1.2.3: Analysis of Grid Information

As I mentioned earlier, I analysed the grids using the Focus and Sociogrids computer programs.

* Shaw (e.g. 1980, p.26) describes FOCUS as a program which used a two-way cluster analytic technique to systematically re-order rows of constructs and columns of elements to produce a "focussed" grid which shows the least variation between adjacent constructs and between adjacent elements. It is important to stress that focussing is done with respect to the way constructs order elements rather than to the verbal labels given to the poles of the constructs. Thus 'similarity' between constructs is not based upon "literal similarity" but upon a notion of similarity which is
defined "operationally" in terms of the ordering of the elements.

Clearly, this particular notion of similarity is rooted in the 'assumptive structure' of PGP, viz., bipolarity of all constructs.

The relationships between constructs and between elements are represented in the form of 'tree' diagrams. This is a particularly clear mode of representation and which lends itself to conversational feedback with the elicitee as part of the process of interpreting the completed and analysed (in this case, focussed) grid. An example of a FOCUS-ed grid is given in Figure 7.9. (Student: Heather, C1RG1, G1 - as per the raw practice and study grids examples given earlier).

I discussed with a sample of students from each class their FOCUS-ed grids. This I initiated by first providing each student with a "grid feedback package" consisting of:

(1) A reminder of both the purpose of, and methodological rationale implicit within, elicitation and creation of their grid.

(2) A summary in which I listed the supplied elements, that student's constructs and the elements used to form them.

(3) That student's FOCUS-ed grid

(4) That student's raw grid
NAME: Heather.  C1RG1, GRID No.1.

<table>
<thead>
<tr>
<th>CONSTRUCTS</th>
<th>ENGINEER</th>
<th>WORK</th>
<th>WORK</th>
<th>PHYSICIAN</th>
<th>DEVELOPER</th>
<th>TREATMENT</th>
<th>BIOLOGICIAN</th>
<th>PHYSICIAN</th>
<th>CONSTRUCTS</th>
</tr>
</thead>
</table>

<table>
<thead>
<tr>
<th>C. No.</th>
<th>EMERGENT POLE</th>
<th>IMPLICIT POLE</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Need some kind of science qualification (E1, E3)</td>
<td>Not necessary (E2)</td>
</tr>
<tr>
<td>2</td>
<td>Would work in a lab. (E4, E6)</td>
<td>Not likely to work in a lab (E5)</td>
</tr>
<tr>
<td>3</td>
<td>Would need some knowledge of biology</td>
<td>Not necessary (E7)</td>
</tr>
</tbody>
</table>

10* (* This student had 10 constructs)

Figure 7.8. An illustration of an Example of a Completed Raw Grid (Student: Heather, C1RG1, G.1).
Figure 7.9: Showing an Example of a FOCUS-ed Grid (Student - Heather, 01RG1, G1)

1. Would need some knowledge of biology.
2. Would have to be able to operate a computer.
3. Would have to be able to convey scientific results to others.
4. It is essential to have studied science at least to 'A' level.
5. Need some kind of science qualification.
6. Ability to carry out scientific practices [e.g.] is essential.
7. Understanding of chemistry essential.
8. Would work in a lab.
9. Work involves handling dangerous substances.
10. Maths is an essential qualification.

- Not necessary.
- Doesn't need any understanding of computers.
- Not necessary.
- An 'O' level 'CSE' or no science qualification is necessary.
- Not necessary.
- Not necessary.
- Not necessary.
- Not likely to work in a lab.
- Never have to do so.
- Not necessary.
I include an example of a completed such package in Appendix 9 (again, for Heather, C1RG1,G1). I include FOCUS-ed grids for each collaboratee in Appendix 10.

I then invited each student to come and discuss their grids with me at a time convenient to them. I offered to see each of them individually for this purpose, however, they expressed a preference for a group discussion. I held such discussions with a sample of students from each class but at different times. With the prior consent of the students I audio recorded both these discussions. During these discussions I followed Pope and Keen's (1981) advice to encourage each individual to:

(a) Note high relationships between pairs or groups of elements,

(b) Consider personal reasons why pairs or groups within the total set may be alike or dissimilar,

(c) Consider the clusters formed in order to ascertain possible superordinate constructs (see later).

In addition I invited each student to consider whether, or to what extent, the grid exercise had facilitated alteration or development of their prior views. I did this in order to explore the possibility of using RGT as a teaching technique. However, one of my main reasons for presenting
these collaboratees with the feedback packages and for inviting them to
discuss their grids with me was because I was keen to maximise their
gains from the exercise.

I further analysed the FOCUS-ed grids by means of the SOCIOGRIDS program
since I was interested to explore possible commonalities between students' constructs.

The meta-theoretical rationale behind the SOCIOGRIDS program, in
particular, augments the intimate relation of RGT, in general, to Kelly's
Organisation Corollary by also emphasising his Commonality Corollary. In
Kelly's words, the latter states

'To the extent that one person employs a construction
of experience which is similar to that employed by
another, his psychological processes are similar to
those of the other person.' (Kelly, 1955, p.90).

This additional emphasis underlies the judgement that

'Elements are more easily shared than constructs,
since they are representatives of the universe of
discourse. If they are physical entities or shared
experience both participants are likely to be able to
construe them without difficulty'. (Shaw, 1980, p.88).

This, in effect, supports the view that a set of elements may be
provided for construal (as is the case with SOCIOGRIDS) provided that
grounds, judged "sufficient" by all concerned (i.e. elicitor,
elicitee(s), and, ultimately, readers and critics of the reported study),
are given for the "representativeness" of the set. I hope to have
rendered plausible the adequacy of my provided set of elements in this study (cf. section 7.3.1.2.1., above).

The SOCIOPRIDS program analyses the set of repertory grids elicited from a group, but each person is free to use his or her own personal constructs. As with the FOCUS program, analysis by SOCIOPRIDS of similarities and differences of construing is conducted in terms of ordering of elements.

The SOCIOPRIDS program may be said to entail that of FOCUS. SOCIOPRIDS allows each person in the group to have feedback on their own mapping of the area from a FOCUS-ed grid (i.e. as per the RG feedback package, described earlier). In addition, however, the SOCIOPRIDS program extracts and focusses a "mode" grid which provides a representation of the most commonly used constructs by all members of the group and thereby allows additional, qualitatively different, feedback to both the elicitator and elicitees (though, sadly, I did not take up this possibility with elicitees in this application).

The character, possibilities and limitations of SOCIOPRIDS can best be understood through a brief consideration of how the mode grid is created.

What makes SOCIOPRIDS different to FOCUS is that the rating values which, for purposes of analysis, comprise each grid are mapped on to those comprising each and every other grid (rather than just within a grid). This is done two grids at a time by means of the PAIRS program which, with respect to SOCIOPRIDS, may be considered to be a sub-program. Shaw has described the mode grid thus:
The "mode" constructs of the group can be extracted from the maximum values obtained in the PAIRS algorithm. These are the constructs most often used by all members of the group, found by listing in descending order of average match values all the constructs from every grid. To find these values, each construct in turn is considered, the total of the maximum match values of this construct with every other construct, scaled over the number of constructs with which it is matched, being computed. A cut-off point on this list may then be taken at a place appropriate to the purpose of the exercise, identifying those constructs which are highly matched with some construct from each of the other grids.

These constructs chosen from the list then make up the "mode grid". Each construct in the mode grid has been obtained from one individual in the group and is in no way changed when used in the mode. This grid then is not a consensus grid which averages out the individualities to produce a pale imitation of the group, but is strongly weighted towards the commonality or intersection of construing within the group. (Shaw, 1980, p.92: my emphasis).

One aspect of SOCIOGRIDS that may constitute a constraint in some applications is that the program is presently able to deal with a total of only 15 constructs in the mode grid.

With C1RG1 I took my "cut off" point at 13 constructs, whilst with C1RG2 I used the full 15 construct capability. I present the mode grid for C1RG1 (C1MG1) in Figure 7.10 as an example. (I shall discuss my interpretation of both C1MG1 and 2 in section 7.3.1.3 below).

One further feature of SOCIOGRIDS is that it produces a table (see Figure 7.11) showing the ranked sequence of rating similarities between pairs of individual grids. This sequence of "socionets" may also and, for purposes
of interpretation and conversational feedback with elicitees, more usefully be presented in the form of diagrams. (I employ such diagrams in my later discussion, in section 7.3.1.3., below). Socionets facilitate the identification of sub-groups where the highest degree of commonality of construing occurs. They also direct attention to those persons who, in relation to the group's identified sub-groups, have strong individual viewpoints, indeed, the "individuality" of such persons may operationally be defined by their rank position in the ordering of similar measures.

This concludes my description of the methods of grid analysis that I used. I have hitherto referred to FOCUS and SOCIOGRIDS as methods of analysis, as opposed to "methods" of interpretation, because I contend that the mere focussing of a grid (by whatever means) actually provides no meaning for a grid - though by so structuring a person's expressed views on a chosen subject it (may) facilitate developing one, i.e. it facilitates interpretation of the grid elicited information.

Personal utility(s) is intrinsic to the interpretation of any grid. In interpreting another person's grid there should, ideally, be a re-visiting of that person's personal utility(s) informing their created constructs and considered in the light of the elicitor's original intended and negotiated purpose for elicitation of the grid(s). But there should also be a negotiated elaboration of the elicitor's personal utility(s), as well as of those of the elicitee, during interpretation of the elicited grid.
Figure 7.10 Showing the FOCUS-ed Mode Grid for C1RG1

Q7C1: Get to wear a white lab coat
Q7C4: Involved in some kind of research work
Q5C2: Needs at least one 'A' level in one of the science subjects
Q8C5: Claims that he/she is a scientist.
Q7C1: Complicated mathematical calculations involved.
Q7C6: Science 'A' level not required.
G1C9: It is essential to have studied science at least to 'A' level.
G1C1: Need some kind of science qualification
G7C10: Get these initials after your name
Q9C2: Job requires high qualification.
R] Q5C4: Had a science-based education.
R] Q7C4: Job in high-wage bracket
Q5C1: Needs mathematical skills

Don't very often
Some kind of work involved everyday.
Doesn't need science 'A' level.
Does not think of himself/herself a scientist
Uncomplicated
Required.
An 'A' level, 'CSE' or no science qualification necessary.
Not necessary.
Don't
Job requires low qualification.
Doesn't need a science-based education
Job in low-wage bracket.
Doesn't need a particularly good knowledge of mathematics.
<table>
<thead>
<tr>
<th>Grid No.</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>Link No.</td>
<td>7</td>
<td>7</td>
<td>7</td>
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<td>1</td>
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<td>3</td>
<td>4</td>
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<td>6</td>
<td>7</td>
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</tbody>
</table>

Figure 7.11 Table Showing Rank Ordering of Similarity between Pairs of Individual Grids (Socionets) for C1RG1
In practice it is frequently the case, as it is my present application, that the interpretation of the grids is skewed in favour of the personal utility(s) of the elicitor. This means that the research inferences that I later present should be considered less 'authentic' (cf. discussion, Ch.5) than where I have used more developed processes of negotiated interpretation. This notwithstanding, I believe that the inferences that I draw in this part of my study deserve serious consideration since I not only engaged in some negotiation of interpretation of the grids (the group interviews) but also the inferences I create may be considered by the reader, as indeed they were by me, in the light of certain of my other investigations on highly related topics and in which I did employ more developed techniques of negotiation (see especially the FI material, specifically used to augment this present investigation in section 7.3.1.4. below, and the FIs, discussed in Ch.8).

My interpretation of the grid information has been guided by my meta-theoretical commitments as elaborated in (especially) Ch.3 and 4: these commitments are, after all, the subject of my study. More specifically, I have judged McClelland's (1984) distinction between 'strong' and 'weak' senses of 'science', 'scientific', and my distinction between 'strong' and 'weak' senses of of 'method', 'methodology' (both discussed in Ch.8) to be of an appropriate level of sophistication for purposes of interpreting responses to WE.
7.3.1.3. Interpretation of Grid Information

Following an "impressionistic" appraisal of the students' individual and mode grids, my detailed interpretation of the grid information began with "group interviews" (GI) with a sample of collaboratees from each class. For ease of reference to the appropriate appendices, I present basic details of these samples in Figure 7.12 below:

<table>
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<tr>
<th>GI No</th>
<th>ClRG No</th>
<th>T</th>
<th>Student's Name</th>
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<td></td>
<td>Sue *</td>
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<td>Lee T</td>
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<td>Tim</td>
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<td>ClRG2</td>
<td>Dr.O</td>
<td>Stephen</td>
<td>9</td>
<td>A.10(b)</td>
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<tr>
<td></td>
<td></td>
<td></td>
<td>Lester</td>
<td>12</td>
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</table>

(* = I conducted in depth FIs individually with these students approximately one year later: cf. Ch.8).

Figure 7.12: Table showing details of Collaboratee Sample for Group Interviews
Figure 7.13: Showing the FOCUS-O Grid of Heather (C1RG1, G1)

- Would need some knowledge of biology
- Would have to be able to operate a computer
- Would have to be able to convey scientific results to others
- It is essential to have studied science at least to 'A' level
- Need some kind of science qualification
- Ability to carry out scientific practices is essential
- Understanding of chemistry essential
- Would work in a lab
- Work involves handling dangerous substances
- Maths is an essential qualification

- Not necessary
- Doesn't need any understanding of computers
- Not necessary
- An 'O' level, 'CSE' or no science qualification is necessary
- Not necessary
- Not necessary
- Not likely to work in a lab
- Never have to do so
- Not necessary
The first part of each GI was spent trying to sort out students' queries and confusions concerning the nature and limits of the analytic aspects of the Focus program. For example, one student, Stephen (C1RG2, G9), expressed concern that whilst he had elaborated only four constructs in his raw grid, the computer appeared to refer to a total of seven constructs in his construct tree... I found these preliminary aspects of the GIs very useful in helping me improve the clarity of my exposition of RGT to collaborators. This notwithstanding, there was an enduring tendency for students to anthropomorphise the numerical manipulations of the computer. GI1 lasted approximately 35 minutes, GI2 approximately 10 minutes.

When I had judged a student's understanding of the rationale behind their grid analysis to be appropriate (or sufficiently appropriate), I encouraged them to explore their personal interpretations of the grid information with me.

To give an idea of the sort of discussion which took place between individual students and myself with GIs, I shall present excerpts from that which I had with Heather (C1RG1, G1) and whose FOCUS-ed grid I re-present in Figure 7.13.

My discussion with Heather began in the following way:

I: [...] can you see any patterns in [...] your construct tree [...]?

H: Well that's obviously the best one [indicates node between C7,8] because its under[neath] a 100 [%construct matching scores].

At this point there followed a brief discussion in which I clarified some...
confusions that I felt Heather had concerning what this meant in RGT terms. This culminated with me asking her

(074) I: [...] so what is it you think is so similar about constructs 7 and 8?

(076) H: chemistry virtually is all practicals and the practicals we do in biology... they're mainly chemistry [...] what I'm getting at is biology's not like it used to be. modern biology is done mainly in the lab using all the latest equipment and experimental techniques...

(085) I: I see [...] is that why a knowledge of biology [C3] is out here?

(093) H: yes all that classification of plants and animals... you've still got to know it roughly but these days its more important for a biologist to know the periodic table that's what modern biology's about..

(098) I: uhhuh [...] can you say a bit more about what you meant by experimental techniques?

(101) H: set ways of measuring and testing things to do with the living world using incubators microscopes and equipment like that...

(103) I: I see [...] is that what makes a job scientific?

(107) H: yes except for if you're a physicist obviously you'd be more involved with the non living world so the methods and equipment would be slightly different.

I was able to find out more about Heather's personal meaning of an 'experimental technique' when I asked her to elaborate upon why she thought qualifications in science are important in/for a 'scientific job':

(121) I: according to this [indicate H's construct tree] you think studying science and being qualified in science is important for being a scientist. for doing a scientific job?

(122) H: yes they wouldn't even look at you if you weren't qualified
I: what. interview do you mean?

H: yeah [...]

I: is that the only reason...getting an interview?

H: oh no no. what a science qualification shows is that you're competent in all the relevant knowledge and techniques of your science according to your [academic?] level. that's what science qualifications show and that's what you'll also need as a scientist [...] if you go for a job in forensics your employer won't expect to have to explain how to do a chromatogram or that keratin is a protein hell just assume you know because you've applied as a biologist...

I: I see. so having a qualification in science shows that you're competent in the knowledge and techniques relevant to whatever science your qualification is in?

H: well...I suppose the actual exam itself. it's more 6 [indicates C6] because they'll often give you just the basic results and want you to make something of them...that's where your knowledge [of science?] comes in...or they might want you to look for sources of experimental error. was there a draught? were the chemicals pure? was the apparatus clean? you know how to answer these things from having done practicals I mean the exams are not practical enough. not for me but it's not practical for science exams to be all practicals! [laughs]

I judged Heather to place pre-eminent emphasis on 'practicals', a term she seemed to use interchangeably with 'experimental techniques' and 'methods', as a criterion for a 'scientific job'. She was somewhat atypical in this, relative to the other GI students who tended to place greater stress on knowledge of discrete aspects of science theory (e.g. theory of evolution by natural selection, Mendelian laws of heredity) in their elaboration of
the requirement of "qualifications in science". This notwithstanding, my overall inferences concerning Heather's views on this subject were in sympathy with those that I made in similar respect to the other students (see G1, C1MG1), viz., she seemed to construe both 'science' and 'method' in 'weak' senses. This I shall try to explain through a consideration of the protocol excerpts I have given earlier.

The main reason for my having drawn these inferences is that Heather did not, at any stage of the interview, appeal to a meta-theoretical criterion for a scientific job (e.g. "theorising involved"), nor did she posit a formative or substantive relation between theories in science and the methods in science. Thus her citation of various parts of the theoretical 'disciplinary matrix' (Kuhn, 1970) of (biological) science, e.g. 'classification of plants and animals' (093), 'periodical table' (093), 'keratin is a protein' (126), served only to locate the subject matter of biological science; she did not appear to perceive them as its (immaterial) products. Her only other reference to science theory, albeit in the context of an examination, cast it in the role of "neutral arbiter":'[examiners] often give you just the basic results and want you to make something of them. that's where your knowledge comes in..' (134).

Heather's references to 'method' also seemed to display a commitment to 'weak' senses since they stressed equipment and "means-end" procedures rather than epistemic considerations: e.g. '[experimental techniques are] set ways of measuring and testing things to do with the living world using incubators microscopes and equipment like that' (101), 'do[ing] a chromatogram' (126).
Figure 7.14 Showing the FOCUS-ed Mode Grid for G1RG1

Q7C7: Get to wear a white lab coat
Q4C4: Involved in some kind of research work
G5C2: Needs at least one 'A' level in one of the science subjects
G8C5: Claims that he/she is a scientist
G7C1: Complexed mathematical calculations involved
G7C6: Science 'A' levels NOT required
G1C9: It is essential to have studied science at least to 'A' level
G1C1: Need some kind of science qualification
G7C10: Get first initial after your name
G9C5: Job requires high qualification

[K] Q5C4: Must have a science based education.
[K] Q9C4: Job in high wage bracket
G5C1: Needs mathematical skills

Don't very often
Same kind of work involved everyday
Doesn't need science 'A' levels
Doesn't think of himself/herself a scientist
Uncomplicated
Required
An 'O' level, 'CSE' or no relevant qualification necessary
Not necessary
Don't
This requires low qualification
Doesn't need a science based education
Job in low wage bracket
Doesn't need a particularly good knowledge of mathematics
Figure 7.15 Showing the FOCUS-ed Mode Grid for C1RG2

q5c5: Interesting
q5c2: Lengthy training
q5c1: A lot of qualifications.
q5c3: Intelligence
q1c5: High qualifications.
q1c2: Long apprenticeships
q2c6: Non-scientific
q5c1: Qualifications (high)
q5c5: Scientific
q5c4: Research work
q2c1: Scientific qualifications
q4c3: Long hours per week.
q4c2: Trying to understand phenomena
q6c2: Large amount of research work
q4c3: Lab work

Boring
Short training
No qualifications
No need of intelligence
Low qualifications
Short
Scientific
Low qualifications
Non-scientific
Mundane work
Other qualifications
Short working week
Accepting the world as it is
No research work
Office work.
I used a similar process of discussion to interpret the grids of the other GI students. Clearly, constraints of time and method (i.e. GI) meant that I was not able to explore their personal meanings in depth, but my interpretations of their responses (and of CIMGs, below) were cast in the light of (precursors to) my more detailed FI investigations, described in Chapter 8.

In the constructs of both mode grids (CIMG1 re-presented in Figure 7.14; CIMG2 presented in Figure 7.15) a similar emphasis to that shown in the grids of GIs on qualifications and training in science is apparent: CIMG1 node 22; CIMG2 nodes 18, 20, 22 with only G7C2 (CIMG2) approaching an explicit "meta-theoretical" criterion: 'Trying to understand phenomena vs. Accepting the world as it is'.

An interesting difference between CIMG1 and 2 may be seen in their configuration of elements (E). In CIMG1, these fall roughly into 2 groups: E(6, 8, 4, 1, 3, 9) and E(5, 7, 2). In the first group, 'Medical Work' (E.6) and Biologist (E.8) are the most highly matched. In CIMG2, by contrast, the elements fall into 4 groups: E(8, 4), E(1, 6), E(3, 9), E(5, 7, 2).

(The fourth group is identical to the second group of CIMG1). The first, most highly matched, pair of elements comprising group 1 of CIMG2, however, differ from those of CIMG1: 'Biologist' (E.8) and 'Physicist' (E.4). I suggest that this difference may be due to the fact that CIMG1 was predominately an 'A' level biology class, whilst CIMG2 was predominately an 'A' level chemistry class: members of the latter may have been using 'Biologist' as their nearest label for 'Chemist', this due, in turn, to the high societal status afforded to
science (see Ch.2).

As I mentioned earlier, the SOCIOMAPS program also produces a series of 'socionets' from the matrix of similarity measured between pairs of individual grids, presented in tabular form, and with which a sequence of socionet diagrams may be created.

In my earlier discussion of socionets I have already outlined the general benefits that they afford to analysis and interpretation of mode grid information. Now, however, I shall discuss the specific nature and benefits of socionet diagrams in these respects. To assist me, both in this task and for later reference in my interpretation of the mode grid information, I re-present the tabular form of socionets for C1RG1 (Figure 7.16) and present selected excerpts from the sequence of socionet diagrams, also for C1RG1 (Figure 7.17).

A diagrammatic representation of a completed sequence of socionets consists of a number of labelled points, each of which represents a grid, inter-connected by straight lines, each of which represents a 'link', i.e. similarity of construct ratings between grids. In such a diagram, the ratings of constructs comprising each grid have been compared with those comprising every other grid within the group. Every grid, therefore, has the same number of links, viz., one less than the total number of grids. Thus, in the case of C1RG1, this number was 8, as may be seen by reference to both Figure 7.16 and Figure 7.17(g).

For purposes of analysis and interpretation, however, the diagrammatic
representation of the completed sequence of socionets is of little use (again, see Figure 7.17(g)). Rather, what is of use is the enhanced feedback that the sequence of socionet diagrams facilitate (over and above their tabular counterpart) in the course of their construction - a process that may usefully be represented in a "linear-cumulative" manner. This enhanced feedback consists of the visual way in which they facilitate attention being drawn to the rank order in which individual grids acquire multiple links, viz. each new (latest) link in the sequence is represented by a dotted, arrowed, line - the dots proclaiming its "newness", the arrow indicating the direction in which the similarity comparison has been made. More importantly, construction of successive socionet diagrams allows for the qualitative identification of 'stars' (i.e. grids with which multiple links are formed early within the total sequence) and 'isolates' (i.e. grids with which few, or no, links are formed until late in the total sequence) (Moreno, 1953). 'Stars' and 'isolates' provide an indication of the distribution of consensual construing within the group. Notwithstanding more popular connotations of these terms, positive valuations of 'star' construers cannot be assumed on the part of other members of the group any more than can negative valuations of 'isolate' construers, for, as Shaw cautions:

'It sometimes happens that the "isolate" turns out to be the creative thinker, and the "star" the muddled compromiser in the group'. (Shaw, 1980, p. ).

Figures 7.17(a)-(d) inclusive, represent the formation of a 'star' around Heather, whilst Figure 7.17(f) suggests that Lee was an 'isolate' construer within the group (n.b. only 36 links in the total set of
I shall interpret my analyses of these two students' grids later.

To facilitate comparison between socionet diagrams, each earlier diagram should constitute an excerpt from the exact geometry of the last diagram in the sequence which is considered. This "last" diagram may or may not be that representing the total (completed) set of links. Whichever is the case, however, clear presentation is usually best effected by "working backwards" - excerpting - from the diagram representing the total set of links, for it may easily be drawn as polygon with sides of equal length and with equal angles subtending between them: herein lies the principal use of such a diagram. For purposes of a quick, initial analysis, however, a sequence may be construed in a "true" linear fashion.

I shall now present my interpretation of the socionets for ClRG1 and 2, and in that order.

I judged Heather (ClRG1, G1: her grid re-presented, once again, in Figure 7.18) to quickly emerge as the first 'star' construer since the first four links in the total sequence were made to her grid (Figure 7.17(a)-(d)). This did not altogether surprise me for two reasons. Firstly, I judged Heather to be a socially dynamic and influential member of the group. Secondly, I considered the character of her constructs, especially in the light of our discussion of them in the GI, to reflect views and attitudes that I have often encountered with young student scientists.
### Table 7.16

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**Figure 7.16** Table Showing Rank Ordering of Similarity Between Pairs of Individual Grids (Socionets) for G1RG1 (Re-presentation of Figure 7.11)
Figure 7.17(a)-(f) Showing Selected Socionet Diagrams for GlRG1
Figure 7.17(g) Showing Selected Socionet Diagram for C1RG1
I also consider Lee T. (C1RG1, G6: his grid presented in Figure 7.19) to be a 'star' construer because there were already 3 links to his grid by link(s) 7 (see Table 7.16) and by link(s) 12 there were no fewer than 5 links (see Figure 7.17(c)).

Lee T. had a very much less extrovert personality than Heather. I attribute his 'star' status to the character of his constructs which might be said to represent an even more narrowly stereotypic image of the scientist than is usual amongst students: he elaborated only 4 constructs, of which 3 formed an equally matched cluster at node 6 (C1,3,4).

Although Matthew's grid (C1RG1, G1: Figure 7.20) was the most highly matched to Heather's (link 1), I do not consider him to be either a 'star' or an 'isolate' construer within the group. A perusal of Figure 7.16 shows that there were only 2 links to his grid by link(s) 15, yet links made thereafter are at roughly equal intervals.

I interpret this analysis by reference both to the large number of constructs that Matthew elaborated, viz. 14, more than any other student in either C1RG1 or 2, and to the range of character of the constructs he elaborated, viz. his constructs varied from those which I consider to be narrow, stereotypic and superficial, e.g. G7, to those which I judge to be atypical but mysterious (because unelaborated), e.g. C6.
Figure 7.18 Showing the FOCUS-ed Grid of Heather (ClRG1t G1)

1. Would need some knowledge of biology
2. Would have to be able to operate a computer.
3. Would have to be able to convey scientific facts to others.
4. It is essential to have studied science at least to 'A' level.
5. Need some kind of science qualification
6. Ability to carry out scientific practices [sic] is essential.
7. Understanding of chemistry is essential.
8. Would work in a lab.
9. Work involves handling dangerous substances.
10. Maths is an essential qualification.
11. Not necessary.
12. Doesn't need any understanding of computers.
13. Not necessary.
14. An 'O' level, 'CSE' or no science qualification is necessary.
15. Not necessary.
17. Not likely to work in a lab.
18. Never have to do so.

- DRIVER
- SHOPWORK
- SECRETARY
- TECHNICIAN
- COMPUTER WORKER
- ENGINER
- MEDICAL WORKER
- BIOLOGIST
- PHYSICIST
Specific scientific qualifications needed.

Scientific (well qualified). Normal high IQ.

Mathematically scientific (intellectual).

Scientific (not technical) (narrow range intellect).

Scientific qualifications not essential.

(No need to be well qualified intellectually).

Experience needed in background.

Unscientific (unintellectual).

Literature based.

---
Figure 7.20 Showing the FOCUS-ed Grid of Matthew (C1RG1, G7)

- Militant unions.
- Involves cutting up organisms (alive or dead).
- Do I want to do it.
- Involved.
- Travel prospects (Good).
- Don't
- Computers used daily.
- Logical mathematical mind involved.
- Good pay
- Complicated mathematical calculations involved.
- Science 'A' levels NOT required.
- Get flush initials after your name.
- Get to wear a white lab coat.
- Get salaries.
- Moderate unions
- 'No real fun in it.'
- I don't want to do it.
- Experimentation involved.
- (Bad).
- Work closely with a lot of people.
- Not.
- 'Ordinary' mind.
- Bad pay.
- Uncomplicated.
- Required.
- Don't.
- Don't very often.
- Paid weekly, monthly, piece work etc.
Figure 7.21 Showing the FOCUS-ed Grid of Lee (CLRG1, G3)
Figure 7.23(a)-(c) Showing Selected Socionet Diagrams for O1RG2
Figure 7.24: Showing the FOCUS-ed Grid of Frances (G/BU4, 08)

- Long hours per week
- Qualifications (high)
- High pay

- Short working week
- Low qualifications
- Low pay

<table>
<thead>
<tr>
<th></th>
<th>1</th>
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<td>12</td>
<td>13</td>
<td></td>
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<td></td>
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</tr>
</tbody>
</table>

- Secretary
- Driver
- Shop work
- Computer work
- Teacher
- Medical work
- Engineer
- Physicist
- Biologist
Figure 7.25: Showing the FOCUS-ed Grid of Robert (C1RG2.G/Q)
Figure 7.26. Showing the FOCUS Grid of Richard II. (CIRG 2, 98.)

- 3 - 1 - 2 - 4 - 6 - 8 - 9 - 3 - 5 - 7 - 2 - 1
- 11-10 - 15-16 - 14 - 13-12

<table>
<thead>
<tr>
<th>Less repetition</th>
<th>Large amount of research work</th>
<th>Lot of intellectual work</th>
<th>A lot of qualifications</th>
</tr>
</thead>
<tbody>
<tr>
<td>2</td>
<td>1 1 1 1 3 4 5 5 5</td>
<td>4</td>
<td>1 1 1 3 2 2 3 4 5</td>
</tr>
</tbody>
</table>

- No research work.  
- No intellectual work.  
- No qualification.

- Shop work  
- Driver  
- Secretary  
- Teacher  
- Computer work  
- Medical work  
- Physicist  
- Engineer  
- Biologist
Figure 7.27 Showing the FOCUS-ed Grid of Irrel. (C1RG2, G3)
Matthew was considered by his peers, his teacher and myself to be the "brightest" in the class; he seemed to be the most innovative and creative thinker and was often given to cogently arguing for radical "offbeat" views (both with respect to biology and elsewhere). His undoubted influence and status within the class, however, did not extend to commonality of construction – he might be considered to be a "free thinker", indeed, he left school in favour of the local Technical College because he could not study the combination of 'A' levels that he desired. I consider it unfortunate that I did not try more strenuously to secure an interview with him in particular.

Lee (C1RG1, G3: his grid presented in Figure 7.21) was the only collaboratee whom I considered to be an 'isolate' construer within the group. Lee's grid was the last with which a link was made (link(s) 14) and additional ones were not made until near the end of the sequence.

I interpret this by reference to his atypical preference for elaborating, almost exclusively, constructs which might be termed "socio-environmental" in character (see especially C1,2,3,5).

Turning now to C1RG2, I present the tabular form of socionets in Figure 7.22, and selected excerpts from the sequence of socionet diagrams in Figure 7.23(a)-(b).

In Figure 7.23(a), 'stars' may be seen to have formed around the grids of Frances (C1RG2., G8: 8 links), Robert (C1RG2., G4: 7 links) and Richard (C1RG2., G6: 6 links) at a point roughly one third of the way
through the total sequence of socionet diagrams (links 23/66).

If the grids for these students (Figure 7.24, Figure 7.25, Figure 7.26) are compared, then it may be seen that family themes emerge, e.g. "qualifications", "lab work". However, there is also the idea that "high pay" is a criterion for a scientific job (Frances, see G8, C2; Robert, see G4, C2) and this may serve to contrast the commonality of construing in C1RG2 from that of C1RG1 (in the former, there were 10 grids featuring constructs relating to pay, whilst in the latter, there were only 2). I speculate that this reflects a greater "vocational" orientation with respect to C1RG2 as compared with C1RG1 and further suggest that this is due, in part, to the predominance of 'A' level chemistry candidates in C1RG2, viz. a career, and career structure, would seem to be more assured, and more defined, in chemistry as compared with biology: this seemed to be considered an important criterion for C1RG2 students - an interpretation given further support from the comments expressed by Stephen (G9) and Lester (G12) in the GI: Lester, for example, claimed that he ' [...] usually think(s) of an industrial chemist working for ICI as a scientific job'. (See Appendix 10(b) for these students' grids).

The student who gave his name as 'Irrel[evant]' (C1RG2, G3) emerged as the most isolated construer within C1RG2: in Figure 7.23(b) it may be seen that only one link had been made to their grid at a point nearly 3/4 of the way through the total sequence of socionets (links 48/66).

If the element tree of Irrel's grid is examined (Figure 7.27), then it may
be seen that their constructs do, indeed, result in a highly atypical clustering of elements. For example, in Irrel's first construct (C.1: 'non-creativity vs. Creativity') the elements 'Biologist', 'Medical Work', and 'Shop Work' are all rated equally (2), whilst the element 'Engineer' receives the highest (and only) rating for the contrast pole (4). The only (highly speculative) interpretation that I can place on this is that Irrel construed the contrast pole, i.e. 'Creativity', to mean something along the lines of 'Construction of artefacts'. Again, in Irrel's third construct (C.3: 'Salary (good) vs. (Bad)'), the element 'Medical Work' receives the highest rating (5) for the contrast pole, whilst the elements 'Secretary' and 'Shop Work' are each rated 4. I interpret this to mean that Irrel construed 'Medical Work' to mean nursing and auxiliary medical servicing by contrast with most students who seemed to interpret that element to mean the work of medical doctors.

7.3.1.4. Comparative Interpretation of Responses to Written Exercises

Question 3 'What is your most usual reason for putting a job in the Scientific Column?'

I included Q3 in WE.v2 and 3 (n=57) as an adjunct to the grid investigation.

I verbally introduced this question to the students by suggesting that they 'might like to tackle this question by looking over the jobs you have listed in the 'scientific' column and then see if there is anything in common between them which might have made you label them scientific'.

- 7.65 -
In analysing responses to Q3, and, indeed, for all the remaining items in WE, I used essentially the same clustering technique to derive categories as I used for Q1, (cf. section 7.3.1.2.1, above).

Where students gave what I judged to be a "compound" response to any of the WE questions, I classified their answer under the two, or more, categories which I felt best characterised their answer. This did not occur in my analysis of responses to every WE item, but where it did, there were no more than 3 instances. In categorising such responses I did not take account of the order in which the components were expressed, though in my overall interpretation and discussion I have tended to assume that priority of order implies priority of (student perceived) importance.

Throughout this chapter I shall not make more than passing references to my analyses of responses to Q2 and Q4 since I included these for purposes other than the present investigation (see Appendices 11 and 12 respectively).

I present a summary of the categories I derived from responses to Q3 in Figure 7.28 below:
WE. Q3: 'What is your most usual reason for putting a job in the scientific column?'

<table>
<thead>
<tr>
<th>Cat.No</th>
<th>Category of Response</th>
<th>Freq. of response</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Requires qualifications/special skills</td>
<td>17</td>
</tr>
<tr>
<td>2</td>
<td>Tautologous</td>
<td>11</td>
</tr>
<tr>
<td>3</td>
<td>Mere use of sophisticated/lab. equipment</td>
<td>8</td>
</tr>
<tr>
<td>4</td>
<td>Mere involvement with Natural Phenomena</td>
<td>7</td>
</tr>
<tr>
<td>5</td>
<td>Is complex or difficult</td>
<td>6</td>
</tr>
<tr>
<td>6</td>
<td>Requires mental activity</td>
<td>6</td>
</tr>
<tr>
<td>7</td>
<td>Related to school science</td>
<td>6</td>
</tr>
<tr>
<td>8</td>
<td>No response</td>
<td>2</td>
</tr>
<tr>
<td>9</td>
<td>Uncategorised data</td>
<td>6</td>
</tr>
</tbody>
</table>

Figure 7.28: Table Showing Categories of Response for WE, Q3

My discussion of these categories which follows, is highly selective. However, in Appendix 12 I present a more detailed summary table, my rationale for each and every category together with further examples from students' responses (I also include these features in appendices for WE, Q2, 4-86).

Amongst responses to Q3, easily the most prevalent criterion for an occupation's scientific status was whether or not, or whether to a sufficient extent, it "required qualifications":

**Category 1: 'Requires Qualifications/Special Skills (n=17)'**

e.g. Class 3, r.1

'Because you have to have the qualifications to have a scientific job so you could be classed as a scientist'
(n.b. only 3 students specified the requirement of 'special skills' and none elaborated the nature of such skills).

Now I contend that mere possession of a present day qualification in school-science (and even in university science, with the exception of some research degrees) can only be used to confer the 'scientific' character of an individual in the 'weak' sense. This is because such qualifications presently represent only a hierarchical recognition by society of an individual's grasp of the content of science, i.e. of the theories of science. They do not, however, provide a demonstration, still less a measurement, of an individual's ability to theorise. Whilst theories are the vital content of science they are also its most important, albeit immaterial, products. I contend that qualifications in school-science ignore the process(es) by which such products are created. At most they show the individual's ability to reiterate, select or apply theories, not to create one. (For a more detailed discussion, see Ch.5).

If I exclude 'tautologous' responses (e.g. Class 2, r.5: 'Whether or not science is yoused to do the job'), then the next two most frequent types of response also suggested 'weak' senses of science.

Respondents in the first of these categories saw the mere use of certain types of equipment as a criterion for classifying a job as being scientific. Responses in this category referred either to sophisticated or complicated equipment (e.g. "high technology") or to equipment which I judged to be normally - and stereotypically - associated with the conduct of science (e.g. "microscopes", "computers").
Category 3: 'Mere Use of Sophisticated/Laboratory Equipment' (n=8)

e.g. Class 2, r.15

'I think the most usual reason for a scientific job is whether or not the job involves high technology or using scientific implements'.

These responses discussed neither the *way* in which such equipment was used nor *why* it was required: mere involvement with, or use of, it was enough. Mere use of laboratory equipment does not make the user a scientist any more than the mere use of paints and brushes makes the user an artist. By this criterion an actor portraying a scientist, and whose portrayal required him to look through microscopes etc, would become one. Nor is a judgement of the relative sophistication of equipment used appropriate for inferring the degree of scientificness of the user. This would make the user of a computer more scientific than the user of an abacus in the solution of a common problem. Equipment represents only a physical tool of science (as opposed to an abstract tool, such as method). Finally, for reasons that should be apparent, appeals to what is arguably scientists' most common workplace, certainly in western lore, namely: the laboratory, demonstrate only a 'weak' sense of scientific.

The second category consisted of responses in which the pupil classified an occupation as being scientific if it required mere use of, or involvement with, materials, physical properties or phenomena normally associated with school-science. In these responses, little or no attempt was made to elaborate any *way* in which they were used and so reflect only a 'weak' sense of 'scientific'.
Category 4: 'Mere Involvement with Natural Phenomena' (n=7)

e.g. Class 2, r.10

'My usual reason is when chemistry gases or electricity or biology are involved'

My impression, corroborated by many sources of experience from this investigation, was that there was a very prevalent initial assumption amongst students that the notion of 'scientific' was 'commonsense'. Where elaborations were made, they seemed to reflect predominately 'weak' senses of scientific. Where elaborations were not made, there seemed to be an assumption that the character of 'science' was known or knowable in some absolute way, albeit presently not by them (cf. 'inexplicable explicative device' strategy, described in section 7.4. below).

Strong support for these interpretations come from the FIs where students' responses were roughly co-extensive with the categories of responses to Q3 and to the inferences I drew from the grid investigation (cf. Ch.8). In interviews, many students seemed surprised and perplexed by my confrontations to their criteria: for many it appeared to be their first experience that their 'commonsensical' understanding of 'scientific' might, in some ways, be inadequate or inappropriate.

Again, support may be inferred from the categories of response to Q2 and Q4.

With Q2 ('why do you think that I have asked you to answer Q1?'), no
student considered Q1 to constitute an investigation of their views on the nature (character) of the conduct of science. Most students' responses to this item fell into one of two 'superordinate' categories of response, viz. 'normative' responses (Cats. 2, 4) or 'didactic' responses (Cats. 3, 6, 7). I was either assessing them against some absolute criterion or I was teaching them (the latter primarily in the sense of career guidance).

Complementary inferences can be made with respect to responses to Q4 ('Have you found it useful to answer Q1?'). The minority of students who did find answering Q1 useful, again saw this primarily in terms of career guidance, whilst the majority, who did consider themselves to have benefitted, claimed that this was because it was 'commonsense'.

(n.b. in my appendices for Q2 and Q4, I include a brief interpretive discussion of the two categories).
7.4. Written Exercise, Part B: Students' Personal Meanings of a 'Scientific Activity'

7.4.1. Introduction

In this section I describe my attempts to investigate students' personal meanings of a 'scientific activity' by means of pictorial methods. Before presenting this account, however, I would first like to advance a general case for eliciting personal constructions by such methods and to indicate how I consider them to be compatible with, and complementary to, more established PCP investigative methods such as RGT. My advancement of this general case is heavily indebted to the generative discussions I had with Mr. (later Dr.) Mike Watts and Dr. Maureen Pope, as expressed in Swift, Watts and Pope (1983).

7.4.2. Pictorial Methods of Eliciting Personal Constructions

'All art is a kind of confession, more or less oblique.'
(James Baldwin)

The use of pictorial methods for "capturing" a representation of persons' personal understandings now has a reasonably long and established tradition in investigations conducted by and with my immediate colleagues. In some cases the pictures are provided whilst in other cases collaboratees are asked to produce their own. In our work, the pictures have tended to find optimum use as a focus for recorded interviews (though other uses may profitably be made as, indeed, was mainly the case with my application described in this chapter).

Our experiences show that they provide an excellent facility for focussing
attention, stimulating recollection of experimental details, refining and
directing previous statements, allowing protracted and varied discussions,
and exploring the extent to which the meanings received by the interviewer
are similar to the intended meanings of the interviewee.

It is this last feature that we find to be of particular interest: the
role of pictures as semiotics within a meaning system. Before describing
in detail those pictorial methods which I have personally used/developed,
I shall make reference to other works which advocate pictorial methods for
similar purposes.

The term "picture" can be both a verb and a noun: the act of making a
representation of some form ("to picture") and the form itself ("a
picture"). Common usage often tends to hide the distinction between the
two. Novitz argues that anyone producing a picture of any sort is
engaged in an act of communication:

'Clearly, then, pictures are like sentences... for they can
be used to express what I have called propositions: they
can be used to indicate a subject and to attribute certain
properties, dispositions, states, actions etcetera to it'
(Novitz, 1977)

Part of Novitz's case is that pictures are produced with an audience in
mind - even if that audience is the picture producer himself. In this way
pictures can be seen to act as a focus for both an 'author message' and an
'audience message' (Sless, 1981). Sless points out that both such messages
operate within a 'framework of assumptions and expectations' and that
there is no logical basis for presuming that there is necessarily a nexus
'They represent two worlds in which the only unconditional similarities are the physical form of the message and the humanity of the participants'.
(Sless, 1981)

My own interests lie in exploring the personally constructed worlds of participants. There is a long tradition of using pictures in psychological research. Partly for reasons of brevity, and partly for emphasis, I shall not present an exhaustive or detailed review of such uses. Much of the work, however, lies within a strictly-bound psychometric paradigm concerned with memory recall, face recognition, and stimulus-response contingencies. Little, if any, of this work has attempted to explore the "internal world" of the participants.

Where the internal world of the individual has been explored using pictorial methods the psychological approaches used tended to depict the person as the victim of internal forces - as is the case of the extensive psychoanalytic literature. Either way the approaches have denied reflexivity, the participants are victims either internally or externally determined (Bannister, 1979). This runs counter to the spirit of PCP, for, as Kelly has said, we are

'[ ] very sceptical of any piece of human research in which the subject's questions and contributions have not been elicited, or have been ignored in the final analysis of results'.
(Kelly, 1969, p.132)
Marton (1981) refers to this as a 'second-order perspective' which describes the imposition of meaning on the outside world as it appears to the actor himself. The central feature of the uses of pictures described in this paper is their emphasis upon a second-order enquiry into students' own statements. Such use can be seen as an attempt to

'compensate for the traditional emphasis on behavioural data by building a body of experimental data'
(Colaizzi, 1971)

The studies that I have referred to so far have used pictures as a method but have not been conducted within any explicit reference to Kelly's meta-theoretical methodological stance. In contrast, the work of my colleagues and myself draws extensively on the spirit of Kelly's work. Our use of pictures falls into two categories - the use of pictures in a way that is akin to "supplying elements" which is called the Interview about Instances (IAI) approach, and a second approach, 'Responding-with Pictures' (RWP), (Swift, Watts and Pope, 1983), where "elements" are elicited.

My development and use of pictorial investigative methods has focussed on RWP, whilst my use of IAI has been only "peripheral". This notwithstanding, I shall present a brief account of IAI approach (see below), prior to that of RWP (see below), since it represents a worthy addition to PCP (and ACM) methods and it provided an influential methodologico-ideational context in which I developed my application of RWP.

7.4.2.1.: Interview-about-Instances (IAI) Approach:
Research using this approach was initiated by the study made by Osborne and Gilbert (1980) and, to date, most investigations which have used it have been concerned with exploring young persons' understanding of ideas in (school) science.

In the IAI approach, a series of line drawings that have been prepared by the researcher play an important role. These drawings (known as a "deck" of IAI "cards") depict various situations which embody elements of a particular concept in science. That is to say, from a science teacher's point of view, the drawings on the cards of an IAI deck provide a representation of a variety of clear-cut examples of the concept (e.g. 'force') under consideration. Some non-examples and a number of 'borderline cases' (being unusual or unorthodox applications of the concept) are also included.

The collaboratees are asked to respond to the pictured situations in terms of their personal explanatory concept. In this way the cards can be seen to act as a focus for both an 'author-message' and an 'audience-message' (Sless, 1981, discussed earlier).

More specifically, collaboratees are asked to consider whether the pictured situations are examples or not of their concept and to give a reason why.

Bannister and Mair (1968) argue that the essential purpose for developing personal constructions is for anticipation, for the better understanding
of future events. Accordingly, many of the pictures on IAI cards contain a "dynamic element" that require the pupil to make some comment as to the outcome – to predict and hypothesise.

The interviews are conducted one-to-one and are audio-recorded. They are semi-structured in that the collaborators are encouraged to sort the pictures as they see fit and an informal discussion is allowed to develop concerning the meanings for the concept.

On occasions when later "analysis" of collaborators' responses yield an inference about their understandings that is judged to be incompatible with, or very different from, that negotiated within the interview, then an attempt is made to re-find that person and ask them to respond to the new inference.

In studies using IAI, interviews have been conducted in two phases. The first phase is the pilot stage for a particular set of cards, the second a series of interviews with a settled deck.

During the pilot stage a considerable amount of feedback is used to make adjustments to the drawings, to the selection and to the order of the instance cards. It allows for the use of the students' own examples and counter-examples where appropriate. More importantly it allows for a selection of cards that "work": that provide lengthy and varied discussion. These represent the negotiated range of instances which can be construed by each individual. The research outcomes have been reported in terms of students' alternative conceptual frameworks of, for
instance, potential difference (Gilbert), force (Watts and Zylbersztajn, 1981), gravity (Watts, 1982a), colour (Zylbersztajn and Watts, 1982), energy (Watts, 1982b; Gilbert and Pope, 1982).

To illustrate the approach I shall consider an example from a study conducted by Watts (1983):

'Michael, a first year 'A' level pupil aged 17 years, discusses a picture of a potted plant (Figure 7.29 below) in the context of his meaning of heat:

---

'M ..... well in the plant cell. it absorbs energy from the sun in order for it to make its own food.

I how does the sun do that?

M (laughs) Its got packets of sunlight. we were told (laughs) to think about packets of sunlight thats it. Thats what we were told to think except that it doesn't really help here...
you think... that it has something to do with packets of sunshine?

yes but it doesn't really help... that's what we were told to think of but I don't think that really helps uh its photons or something that's what springs to my mind. I don't think that really helps... we were told to think about just to go on thinking about packets but never really helped me.

can you tell me anything else about it?

... I was wondering about the sun I don't really know I know its got something to do with the sun's energy. um the plant can absorb the heat its got an ability. it could possible be because um... heat is used as a catalyst not a reaction. and so for a plant to make its own food the heat could be used... as a catalyst or is it light? um oh I'm not sure (laughs) um....I think it'd be heat because. from light you get heat. so it'd be the heat.

In his commentary on this excerpt, Watts speculates about this pupil's derivation of personal meaning from school science lessons:

'In this extract there is evidence of considerable confusion, of unease and difficulty. During some point in his 'A' level work he has clearly been told to regard photons as 'packets' of sunlight. It is the kind of statement that occurs many times in the course of A-level physics, chemistry and biology. Wenham et al [ ] for instance attributes the metaphor to Planck's proposal of quantum theory in 1903. In this case, and in Michael's own words, the metaphor has been of no help in either clarifying his own thoughts on sunlight or in explaining what a photon is or does. There are a range of possibilities as to why this might have been the case. The teacher may have established the notion of 'photons are packets of light' uncritically, without being aware that it is a metaphor. Or it may be that, although aware of its metaphorical status s/he failed to draw out its implications and the purpose and the limits of its transferability. Perhaps, too, in the face of the most detailed and complete of analyses, it would simply have been an inappropriate discomfiting metaphor to draw (between sunlight and packets) for Michael's own approach. Whatever the circumstances, the argument here [ ] is that the teacher is only in a position to highlight and ameliorate such conflicts if they know about them.'

(Watts, 1983, pp.15-15: original emphasis)
I believe that Watts' suggestions can be complemented by reference to a 'model for conceptual exchange' proposed by Hewson (1981) and which, in Gilbert and Swift (1985), we suggest represents a meta-theoretical framework that is both compatible and useful within the ACM. This model incorporates four necessary conditions for 'conceptual exchange' which complement the notion of 'personal utility':

\[ \text{(D) Dissatisfaction with } C_1 \rightarrow \text{(F) Fruitfulness of } C_2 \rightarrow \text{Conceptual exchange, } C_1 \text{ to } C_2 \]

\[ \text{(I) Intelligibility of } C_1, C_2 \rightarrow \text{(P) Initial plausibility of } C_2 \]

Where \( C_1, C_2 = \) Concept 1, Concept 2.

**Figure 7.30: Showing a Model of a Conceptual Exchange (after Hewson, 1981)**

With this model in mind, the pupil discussed above can tentatively be construed as having appraised the notion of 'quantum', as instantiated by that 'photon' and as portrayed by the teacher, with respect to (I), (P) and (F) — and found it to be "lacking" on all counts!

I hope that my recent account of IAI approach has made it clear that the persons whose ideas are investigated are substantially involved at all stages in its development and use. In IAI approach, as in all ACM
investigative methods, there is an especial reliance on such persons expressed views ("content") in the making of inferences with, and about, their ideas. In other words, there is genuine collaboration between the researcher and the person whose ideas are researched (i.e. and hence, 'collaboratee', cf. discussion Ch.5). This quality of collaboration is not only the main way in which ACM investigative methods implement the core philosophical commitments of ACM meta-theory but is also the principal means by which they may be judged to be doing so.

In striving to create a mutually agreed understanding about another person's understanding of a selected topic, ACM researchers adopt a 'credulous approach' (Kelly, 1955, p.586) prior to making negotiated inferences. This contrasts with strict-Piagetians who, it seems to me, adopt some "principle of charity" prior to making normative, un-directed, inferences. Thus, in the 'photon' example described earlier, the ACM researcher seriously discussed and appraised the pupil's personal meaning of the concept largely in terms of that pupil's personal purposes and anticipations for it. For the ACM researcher in such a context, a consideration of the collaboratee's personal purposes and anticipations for a concept is the most important way in which they (the researcher) may "come to know" the collaboratee's personal meaning of a concept and it is upon these that personal relevance for the collaboratee depends.

Kelly comments on this issue:

'Relevance must come to light, and not be dismissed as a "value" beyond the concern of scientific psychology. It can come to light if the discusssional structure of psychological space is explored, and the course of events, behavioural and otherwise, is plotted within personal construct systems that hopefully are cognate to each
ACM workers' high positive valuation of 'relevance' in investigation situations may be further demonstrated by considering how, say, a 'Piagetian technologist' (cf. Ch.3) might consider the student's expressed personal utility for the notion of 'photon', recently cited. I suggest that their main concern would be to relate the pupil's remarks to, and to judge them against, a postulated "stage level of cognitive demand" allegedly required for the meaningful learning of the concept. The Piagetian technologist, like the Empirical-Critical-Rationalist, would not be concerned with the respondent's personal meaning of the concept except in a "diagnostic" sense (i.e. according to the standards of "stage level of cognitive demand" and "objectivity" respectively): their ultimate preoccupation would be with the form of the respondent's utterances. In such approaches, personal relevance for the researched person is ignored, and may be denied, through being assumed.

The open-ended and collaboratee originated qualities of ACM research methods render inferences made with them less open to the charge of "context dependence" than classical Piagetian task-analytic methods (see especially Donaldson's (1978) contributions to, and survey of, such criticisms of the Piagetian school). These qualities afford ACM research methods a flexibility which allows inferences made with them to reduce and even transcend physical and environmental constraints on the investigation-situation. With these claims in mind, it might be useful to consider the following example of an IAI card which is from a deck concerned with the concept of 'heat':

- 7.82 -
Finally, the compatibility of the IAI approach with the core philosophical commitments of PCP and ACM (which I hope to have demonstrated through prior argument and discussion) coupled with its normal incorporation of other investigative methods (mainly interviews) render it an example of 'methodological triangulation' (Denzin, 1978) or '(m)ethodological pluralism' (Swift, Watts and Pope, 1983) - cf. my discussion, Chapter 5.

As I mentioned earlier, I have not strongly featured IAI approach in my own investigations. My "peripheral" involvement with IAI began when, in an early form of FI (my '3 questions approach', discussed in Chapter 8) I noticed a marked tendency for collaboratees to list what they considered to be a 'scientific' and 'un-scientific' subject-disciplines and occupations prior to expressing their personal meanings of the
"unembodied" term 'scientific'.

This practice seemed to help collaborators to formulate their views and so I quickly decided that it would be worth trying to mimic this tactic in my investigative methods. I began by explicitly requesting the next few whom I interviewed to supply exemplar occupations for each category ('scientific' and 'un-scientific') and, additionally, some about which they felt unsure. From interview protocols I then compiled a list of some 24 occupations, with each category represented (see Appendix 14 for complete list). I then constructed a pilot IAI card-deck which consisted of a line drawing, drawn by myself, for each of the 24 occupations and the occupation title on each appropriate card. In these drawings I attempted to portray each occupation as "mimicably" and as stereotypically as possible:

![A Doctor]

Figure 7.32: Showing an 'Interview-about-Instance' IAI Card: 'A Doctor'
I then conducted a series of interviews using the deck. In these interviews I intimated the content of the deck prior to presenting the cards. When I did present the cards, I did so one at a time and in alphabetical order (I also advised each student of this latter fact prior to my presentation). With each card I asked the student two questions:

Q1 Is this person a scientist?
Q2 What tells you that?

I asked the first question in order to find out whether the student categorised the occupation as an exemplar or non-exemplar of a scientific occupation. I allowed, but did not encourage, a "don't know" category.

I asked the second question in an attempt to investigate their reasoning behind their categorisation. By this means I hoped to elicit from each student a set of 'criterial attributes' for a 'scientific occupation'.

I used this approach in only three interviews and then abandoned it. The main problem that I encountered was that each occupation may be subject to many classifications - some falling into each of my categories - yet my drawings tended to "fix" the classification and category and in a way which each student did not necessarily approve in every instance. The sort of objection that I encountered may be exemplified by the following remark made with respect to my 'doctor' card, recently illustrated in Figure 7.32 above:
It depends whether you mean a research doctor [...] like Fleming or an ordinary family doctor.

Whilst the ensuing discussion to such objections often proved insightful, I felt that the means by which I had generated the deck had 'short circuited' the process described by the initiators of the approach and so would cast doubt on any research inferences that I might make using my deck. I suspect that the inadequacy of my deck was also heightened by the absence, or near absence, of a 'dynamic element' to my instances.

In the event, I decided to pursue my investigation of students' personal meanings of a 'scientific job' by other means (RGT and Q1-4 of WE., cf. section 7.3 above). My experience with IAI approach was, nevertheless, useful since students had expressed enjoyment and interest in using illustrations.

7.4.2.2: 'Responding-with-Pictures' (RWP) Technique

My development and use of RWP was prompted by Pope's (1981) suggestion that Cortazzi and Roote's (1975) 'Illuminative Incident Analysis (IIA)' represents a technique that is compatible with the tenets of Constructive Alternativism.

Cortazzi and Roote developed IIA primarily to explore the thoughts and feelings of members of teams working in health and social services. It is based upon the idea that team harmony and development can be encouraged by a frank exchange of ideas and feelings about experiences involving the team as they have worked together. This exchange is initiated and
facilitated by illustrations of incidents from the team's working history drawn by team members.

The IIA technique appears to be especially useful in investigations where there is some difficulty on the part of the collaboratee(s) in verbalising their thoughts and feelings. In Cortazzi and Roote's study, collaboratees' difficulties in verbalising their views were apparently due to the considerable emotion connected with incidents involved in the working of the team, indeed, in that study, drawings were used instead of words. These authors argue that:

' [...] verbal discussion of an Illuminative Incident is likely to be abortive [...] Reality is easily hidden behind words, but difficult to disguise in pencil lines. certainly feelings will be more accurately indicated in a drawing, as every art therapist knows; and ultimately, it is feelings that interest us'.

They concede, however, that an IIA investigation may profitably move from the non-verbal to the verbal mode.

As we have seen (Ch.3), Kelly was emphatic that 'cognition' and 'affect' could not be separated within PCP. I suggest, however, that other difficulties in verbalisation may have to do with interrelations between the collaboratees and their (young/old) age, and/or their competence within a linguistic tradition for discussing a certain subject, and/or the traditional degree of complexity of the syntax and vocabulary associated with ways of discussing a certain subject. In researching into students' personal, scientific, epistemologies, I have encountered collaboratees with difficulties of verbalisation of particularly these last sorts -
hence the suitability of IIA for me. IIA approach may be construed to be a methodological response to Kelly's caution that 'Construing is not to be confounded with verbal formulation' (1955; p.51), a caution that is less heedable with RGT due to the reliance on verbal labels in that technique.

'Responding-with-Pictures' is the name that my colleagues and I have given to our variants of IIA, my personal endeavours with which are described in the account which follows:

7.4.3 Investigation of Students' Personal Meanings of a 'Scientific Activity' using RWP Approach

Science epistemology is a notoriously linguistic enterprise and, furthermore, is one which has little or no (explicit) tradition in science education.

In my experience, investigating pupils' personal meanings of "scientific-method" lessens difficulties of verbalisation but at the risk of encouraging mere recitation of specific experimental procedures as had been carried out or described in school science lessons. Furthermore, in enquiring into pupils' personal meanings of scientific-method as a means of initiating exploration of such personal science epistemologies as they might have, I did not wish to "beg the question", i.e. pupils might not perceive that scientists refer to method(s) as a means of acquiring knowledge.

I have found that asking young persons to produce pictures of a
"scientific" activity has helped to overcome these difficulties: RWP.

7.4.3.1 Method for Eliciting Pictures

Within WE this part of my investigation was effected by means of 3 questions:

Q5 Do a sketch of something that happened outside school in which you did something scientific

Q6 What is happening in your sketch?

Q7 Why is it scientific?

I shall now describe my purpose and development of each of these questions.

(a) Q5 Do a sketch of something that happened outside school in which you did something scientific

I included this question in all three versions of the WE., however I modified its wording and format in the first two. I will discuss these modifications later.
My purpose for this question was similar to that of Q3, namely, to attempt to elicit what pupils considered to be the 'essence' of scientific activity.

In my verbal presentation of the question I stressed that I did not require a "work of art". I also advised them that if they included people in their sketch then "pin-persons" would be perfectly acceptable. I said these things to allay the fears of those who claimed not to be 'able to draw' when they first saw the question on the blackboard. I also requested pupils to draw their sketch on the plain sheet of paper I provided.

I 'pre-piloted' this question on six fourth-year pupils from a class which I did not use for the full WE.

I shall now describe the modifications I made to the wording of this question. My first version (i.e. WE.v1) was as follows:

```
Think of something that you have done which you think was scientific and do a sketch of it
- what is happening in your sketch?
- why is it scientific?
```

This wording resulted in many pupils drawing a sketch of an experiment
which they had conducted in a school science lesson but which could have been conducted out of school. On the whole I did not regard this as the most fruitful sort of response in pursuit of my purpose for the question. For example, it begged quasi-tautologous responses to Q7 (see below): 'it was a scientific activity] because I did it in a science lesson'. I therefore reworded the question and added emphases for clarity (i.e. WE.1.v2):

Do a sketch of something that happened outside school in which you did something scientific
- what is happening in your sketch?
- why is it scientific?

In this version, another problem which had also applied to the first version, came to my notice. This was merely that some pupils were unsure of the status of the questions following the dashes. Accordingly I numbered them separately as Q6 and Q7.

(b) Q6 What is happening in your sketch?

My purpose for this question was twofold. Firstly I included it to aid my
interpretation of the sketch with those respondents whom I did not interview. Secondly, I intended its inclusion to reinforce my earlier verbal claim that the artistic quality of the sketch was irrelevant.

In my verbal introduction to this question, I characteristically asked students to 'write down what is happening in your sketch in a word or two'.

(c) Q7 Why is it scientific?

As with Q6 I included this question to aid my interpretation of the sketches of those respondents whom I did not interview.

In my verbal introduction to this question, I characteristically asked students to 'write down what you thought was scientific about the activity you have drawn'.

7.4.3.2: Method for Interpreting Pictures

My analysis of responses to these questions focusses on Q7 for which I created categories of response. Although I make frequent reference to the 'incident' where I feel it relevant, I do not offer a categorical analysis of them. It is possible that this would be worth doing in the future.

I found many of the pupils' sketches extremely pleasing in their own
right. However, I do not believe that the sketches themselves contributed anything additional to the information obtained from my analysis of responses to Q6, i.e. in this application of the methodology the sketches were incidental to analysis.

I present a summary of the categories I created for responses to Q7 in Figure 7.33.

<table>
<thead>
<tr>
<th>Cat. No.</th>
<th>Category of Response</th>
<th>Freq. of Resp.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Mere Use of/Involvement with Natural Phenomena</td>
<td>16</td>
</tr>
<tr>
<td>2</td>
<td>Related to School Science</td>
<td>14</td>
</tr>
<tr>
<td>3</td>
<td>Involved Principle</td>
<td>10</td>
</tr>
<tr>
<td>4</td>
<td>Mere Use of Equipment Invented by Scientists</td>
<td>6</td>
</tr>
<tr>
<td>5</td>
<td>Complex/Inexplicable</td>
<td>6</td>
</tr>
<tr>
<td>6</td>
<td>Tautologous</td>
<td>6</td>
</tr>
<tr>
<td>7</td>
<td>Required Cognitive Skills</td>
<td>5</td>
</tr>
<tr>
<td>8</td>
<td>Required Personal Qualities</td>
<td>4</td>
</tr>
<tr>
<td>9</td>
<td>Involved Writing</td>
<td>2</td>
</tr>
<tr>
<td>10</td>
<td>No Response</td>
<td>3</td>
</tr>
<tr>
<td>11</td>
<td>Uncategorised Responses</td>
<td>7</td>
</tr>
</tbody>
</table>

Figure 7.33 Table Showing Categories of Response for WE. Q7

My discussion of these categories which follows, is highly selective. However, in Appendix 15, I present a more detailed summary table, my rationale for each and every category together with further examples from students' responses.
7.4.3.3. Interpretation of Picture Responses

My interpretation of categories of response to Q7 begins with a consideration of the two largest ones in which appeals were made by students to the mere use of, or involvement with, materials and natural phenomena (i.e. the vehicle of science) and the relatedness of the incident to school-science.

Figure 7.34: Showing a "Response-with-Picture", Category 1, re a "Scientific Activity" (Class 2, r.9).

In these responses, little or no attempt was made by the pupil to elaborate the way in which they were used. In FIs, pupils who had given a C1 (Category 1) type answer to Q7 often appealed to the relatedness of their scientific incident to school-science as one of their first lines of defence (i.e. a verbal equivalent of the Q7, C2 type of response, discussed later) when I confronted their C1 criterion(-ia):
Figure 7.35: Showing a 'Response-with-Picture', Category 1 re a 'Scientific Activity' (Student= Danny; FI6; Class 3, r.16).

131 I [...] to do with electrodes anodes atoms and things.....whys that scientific?

132 D [...] you heard all that in science [...] you learn all about electrodes and things in science...its just sort of scientific...

Figure 7.36: Showing a 'Response-with-Picture', Category 2, re a 'Scientific Activity' (Class 2, r.4).

In these responses, pupils claimed that the activity was scientific either because it had originally been done in a school science lesson or because
it bore a resemblance to a school-science activity. This criterion suggests a weak sense of scientific: not only does the criterion itself make no reference to theorising but also the incidents themselves characteristically implied the mere use of, or involvement with, natural phenomena and/or sophisticated/laboratory equipment. Some pupils appeared to have used the criterion as an 'inexplicable explicative device' (this sort of use appeared to be repeated in a more direct manner in Q7, C5 - see discussion below). Such appeared to be the case with one pupil whose illuminative incident was "playing rounders" on a "SCISP field trip" and whom I later questioned about it:

Figure 7.37: Showing a 'Response-with-Picture', Category 2, re a 'Scientific Activity' (Student- Neil: F1.1; Class 1, r.4).

322 I (...) why was playing rounders scientific?
Neil's later justification - 'energy from a hamburger' (343) - casts himself in a passive role. I suggest that this was an ad hoc justification, a piece of 'instant invention!' However, even the teachers' activity, as described by Neil, can only be said to be scientific in a weak sense.

In the third largest category of response to Q7, pupils cited the use or demonstration of a theory as their criterion for scientific activity:
Figure 7.38: Showing a 'Response-with-Picture' Category 3 (Class 2, r.10).

The majority of these respondents appealed to the demonstration of hand-me-down theories from school-science or common-lore. Others appeared to embody a very loose, general principle such as 'finding out'. I suggest that these responses show at least some recognition, on the part of the pupils, of the importance of theory in the conduct of science. A difficulty in applying McClelland's strong-weak differentiation to responses to Q7 became especially apparent with responses in this category, viz. I concede that the request to draw a scientific activity tends to preclude the act of theorising. I nevertheless find it interesting that none of these pupils abstracted a method - though empirical verification appeared to be implicit in most.

Only one pupil, FI(P')4 (Nigel), whom I interviewed had given a C3 type of answer to Q7:
Figure 7.39, Showing a 'Response-with-Picture', Category 3, re a 'Scientific Activity' (Student= Nigel; FL.4; Class 2, r.12).

205 I [...] why is the contracting of cool air scientific?

212 N because it shows how heat affects air...

213 I but [...] why did you pick on that?...[N shrugs]...[...] why is showing the effect of heat on air scientific?

231 N ...dunno really...I spose...well its to do with air pressure and things like that...[...] I just cant think of anything else that its to do with...well maybe its because its to do with heat...

238 I again you see. I'll have to ask you why is something to do with heat...or something to do with air pressure scientific?

241 N ...I suppose its just the nature of it [...] at the moment if I was to think of anything that I could say was scientific Id think...what lesson would it come under...[...] thats all really...
On the evidence of this excerpt, it would appear that, for Nigel, the essence of the scientificness of his incident was neither the creation nor the demonstration of a theory ("heat affects air"); but rather the mere use of, or involvement with natural phenomena ("heat", "air pressure"), i.e. 'weak' elements of science. When I confronted him on this (238) he further retreated into a 'relatedness to school-science' type of justification (241). At the point where my excerpts ends I asked Nigel if 'the effect of heat on air' might be considered in non-science lessons. Our discussion then moved into a fruitful consideration of what he saw to be the differences in approach to this phenomenon between science lessons and cookery, pottery etc. lessons.

In smaller categories of response to Q7, some pupils cited the use of the physical tools of science: C4. "Mere use of Equipment Invented by Scientists". Others appeared to use the term 'scientific' as an "inexplicable explicative device" - a label for things they did not understand: C5, Complex/Inexplicable. No pupil elaborated a method as such, though the appeals made by some to cognitive processes, e.g. the use of 'logical processes of elimination', were suggestive of one: C7 Required Cognitive Skills. Finally, a handful of pupils cited aspects of personality such as 'patience': C8 Required Personal Qualities. These last were to feature prominently in discussions within FIs as to the pupils' personal meaning(s) of scientific-method.

I regard all the minor categories of response above as embodying 'weak' senses of scientific for reasons that should be apparent from my earlier
comments and discussion.

Clearly there are many interesting lines of discussion whose initiation was facilitated by RWP but which were beyond the scope of WE. I pursued such lines of discussion later.

RWP approach may appear to be rather a simple technique and, indeed, it is 
qua technique. But from this one should not infer that it allows for 
only simplistic dialogue. On the contrary, RWP technique has, on many 
occasions, shown its facilitative utility for highly abstract or complex discussions.

Earlier in my account I emphasised the usefulness of RWP as a technique which lessens or overcomes difficulties on the part of collaboratees in verbalising their ideas. In my experience, however, it has also demonstrated its utility in exploring the personal views of certain highly articulate collaboratees. At first glance this may appear to be somewhat contradictory to my earlier remarks, but, verbally fluent collaboratees sometimes have difficulties in condensing their ideas and in expressing them without a confusing abundance of caveats (confusing to both them and to me). In such cases the RWP approach may help to direct us and focus the collaboratee's attention upon a personal experience instantiation of the issue under consideration and to reduce their sometimes expressed feeling of being "trapped" by their own words. Again, verbal fluency may disguise not only the tentative nature of a person's views, but may also mislead the researcher as to what the collaboratee's views actually are. With respect to the latter case, my use of RWP technique has sometimes,
for example, helped me reveal, by negotiation, a distinction between a collaboratee's confident and initial articulation of, say, characteristically Kuhnian doctrines, and their personal science epistemological views to which they are really committed. Kelly's claim that 'Construing is not to be confounded with verbal formulation' (1955, p.51) may thus fruitfully be construed as a caution in at least two major senses.

Notwithstanding my recently expressed views, I consider the RWP approach to be a principal use with young persons; that is to say, persons of age 16 or less.

RWP represents a flexible investigative approach which allows links with related techniques to be explored and/or strengthened fruitfully. For example, following my RWP investigation described above, Gilbert and Pope (1982) extended their basically IAI approach to investigating young persons' understanding of energy by requesting collaboratees to amend, improve upon or design some of the instance cards to exemplify their ideas of energy i.e. their instances were elicited, e.g.

(Student aged 10 years)

Figure 7.40: An 'Elicited Instance' re Energy.

Gilbert and Pope found that conversations about the personal pictures
provided a fruitful source of constraints/alternative conceptual frameworks which their collaboratees used to construe the concept under consideration (energy).

In addition to suggesting IIA might be used in formal educational research, Pope also speculated upon potential other ways in which the approach might be useful in an educational context:

'[IAI] could act as a catalyst to help a student review his/her personal learning, to help groups of students communicate about experiences in their learning and also allow the tutor some insight into some aspect of learning from the student's point of view.' (Pope, 1981, p. ).

I judge certain of these conjectures to have received indirect corroboration from research conducted at the same time as my RWP study. For example, in an informal interview a science teacher, whom I shall name Miss S. (and with whom I later conducted an FI), claimed that one of the main difficulties for her in teaching science was 'getting pupils to pay attention long enough to become interested'. She cited 'laziness and general lack of motivation', on the part of many pupils, to be 'one of the main hurdles facing the conscientious science teacher today' in this respect. She went on to suggest that 'many pupils would far rather talk to their friends about what they've seen on telly the night before than get on and do some science' and then she named and described some pupils who she felt exemplified this. Amongst these pupils I recognised one in particular from whom I had elicited a RWP:
I tentatively suggested to this teacher that such pupils as she had described might have personal ideas on what it is "to do science", that these ideas might be different from hers and that these pupils might talk in her lessons as a 'sort of protest'. I judged this interpretation to come as something as a revelation to this teacher, though in fairness to her, she was somewhat loath to accept it. One of Kelly's comments seems to be specially pertinent here:

'a teacher might complain that a child was "lazy", but when asked to observe him for several days to see how he went about being "lazy", came up with a description of some very active and purposeful behaviour. "Laziness", then although attributed to the child, has as its principal referrent as far as the psychologist was concerned, the frustration the teacher experienced in trying to get the child to join her in something she thought they ought to be doing.'
I suggest that whilst pedagogical implications of ACM research are difficult to make and presently far from clear despite my attempts, (see Ch.10), the straightforwardness of most of the ACM's investigative methods and the nature and extent of reliance on collaboratee "content" in the drawing of inferences has meant that school teachers may easily be admitted to the research enterprise. Moreover, it may sometimes be possible to provide teachers with feedback of a personally, or locally, useful nature and with a speed that would be quite impossible with research conducted in, say, a "Piagetian-technological" or a psychometric mould. I hope to have rendered plausible this last possibility through my recent example: of course, with appropriate negotiation between researcher and collaboratee, the latter's pictures could be used directly.
7.5. Written Exercise, Part C: Pupils' Personal Meanings of a 'Scientific Observation'

7.5.1. Introduction

In this section I describe my attempts to elicit and explore pupils' personal meanings of a 'scientific observation', on the one hand, and to teach pupils, by means of a demonstration personal to each of them, the 'Constructivist Knowledge Thesis' (CKT) ('All observations are theory-laden') on the other. My interests and intentions in this part of my study were, therefore, jointly investigative and didactive. I pursued them principally by means of certain items within WE but additionally by means of FIs, with a subset of pupils selected from the overall collaboratee sample—these I shall later describe (sections 7.5.3, 7.5.4 below). Prior to that, however, I would like to discuss what I consider to be the importance of this part of my study in an educational context.

I construe the worth of the investigations that I describe in this section to be informed by two, related, areas of personal philosophical commitment:

Firstly, all traditions in philosophy of science that are given serious attention today share a commitment to CKT. Since I have already argued (Ch.2) that a science epistemology is a necessary, though not a sufficient, condition for any theory of science teaching and, secondly, since teaching is to be construed as 'collaborative research' (Ch.10), I conclude that science educationalists must nowadays consider how to teach CKT. How, then, might the teaching of CKT even be begun? CKT runs
counter to common experience of knowledge acquisition and theories of knowledge derived from it. Following Hewson (1980) I suggest that CKT will not seem fruitful to an individual until he has first come to regard it as plausible.

The task might be best first tackled in a negative way viz. through demonstrating the impossibility of a presuppositionless approach in either practice or principle. This method appears to have been used by Popper:

'[ ] The belief that we can start with pure observations alone, without anything in the way of a theory, is absurd .... I try to bring home the ... point to a group of physics students in Vienna by beginning a lecture with the following instructions: 'take pencil and paper; carefully observe, and write down what you have observed' They asked, of course, what I wanted them to observe. Clearly the instruction, 'Observe!' is absurd .... Observation is always selective. It needs a chosen subject, a definite task, an interest, a point of view, a problem. And its description presupposes similarity and classification, which in turn presupposes interests, points of view and problems -'

(Popper, 1972, p.49; original emphasis)

Popper's instructions - 'Carefully observe' etc - appeared to me to represent an easily repeatable classroom exercise.

In considering how a genuinely "constructivist curriculum" should be planned and implemented I am in sympathy with the broad implications that Hirst (1980) derives from Popper's (1945) arguments against large scale ('Utopian') engineering, for, as I argued in Chapter 3, the commitment to 'reflexivity' by Constructive Alternatives entails a rejection of any form of Utopian engineering.
Hirst (1980) made a number of recommendations for enquiry into curriculum planning. For Hirst, such enquiry should involve many "small scale experiments" and it 'must involve a great deal of evidence from the direct participants, both teachers and pupils as much as from observers.' Whilst I considered myself to have at least attempted to fulfill these aims with respect to classroom research, I was concerned lest my research interests and activities "migrate" away from possible and desirable (given my philosophical commitments) pedagogy. Thus my inclusion of Q8, 8a and 8b in WE began my attempt to act upon my own recently expressed conclusions and suggestions:

7.5.2. Procedure for Eliciting Pupils' Personal Meanings of a 'Scientific Observation' and for Teaching the Constructivist Knowledge Thesis

Q8 Carefully observe, then write down what you have observed.

(Q8a what difficulty have you had trying to answer Q8?).

(Q8b Observations are often very important in experiments. Does the difficulty you had trying to answer Q8 tell you anything about people doing experiments that you have never done before?)

Clearly, these instructions in Q.8 are based upon those suggested and used by Popper (1963) and which I cited earlier. I regard my use of Popper's instructions as a 'small scale experiment' in curriculum planning. In the Popper quotation, given earlier, he implies that all his physics students saw the fallacy in his instructions: "they asked, of course, what I wanted them to observe". I was interested to see whether or to what extent this was the case with schoolchildren (I suspect, but do not yet
know for sure, that Popper's students were undergraduates). Thus my inclusion of Q.8 was, in part, a "replication study".

I shall now discuss the alterations and additions that I made to the questions in this part of WE.

Q.8 appeared in all three versions of WE, and in the wording given earlier.

In the first and second versions of WE, I included only one subsidiary question. The wording of this question was the same in both of these versions:

Does the difficulty that some of you have had answering Q.4 tell you anything about people who do experiments that have never been done before?

My purpose for this question was twofold:

Firstly, I wished to focus the didactic element of the question on the conduct of experiments. This was partly due to my main area of interest being science-education. I also felt that to attempt to teach the full implications of the theory-ladenness thesis through a class exercise without discussion would be hopelessly over-ambitious. My most optimistic didactic aim was to bring pupils to accept that observations made in an experiment are both guided and limited by its purpose/design.

Secondly, I hoped to elicit pupils' perceptions of the learning process.

In view of my two-fold aim for the subsidiary question, I split it into
two questions in the final version of WE:

Q.8a. What difficulty have you had trying to answer Q.8?

Q.8b. Observations are often very important in experiments. Does the difficulty you had trying to answer Q.8 tell you anything about people doing experiments that have never been done before?

This made it easier both for pupils to answer and for me to analyse their responses.

As can be seen, I also added a statement "Observations are often very important in experiments" to Q.8b. I did this because the 'didactic leap' otherwise required seemed too great. It was a statement which pupils readily accepted or else expressed during structured interviews. By including it I brought the didactic strategy of the questions within this part closer to that which I used in the corresponding part of the structured interviews, which I shall discuss later.

In my verbal presentation of the questions in this part of WE I began by requesting pupils to "answer the next question in ink". I also asked pupils not to call out when I had written the question (Q.8) on the blackboard. I then wrote Q.8 and allowed what I considered to be a sufficient length of time for those who had interpreted the question "naively" to commit the beginning of their answer to paper. By "naive" I mean an answer in which the pupil had begun noting down observations of any sort. After this period I asked those who had had any difficulty answering the question to answer the next question instead (Q.8a). Before writing this question I stressed that I would accept it as an equally good alternative answer to Q.8. I also invited pupils who had answered Q.8 but
who were unsure about their answer, to answer Q.8a if they wished. In the final version of WE I also included Q.8b. I requested pupils who had answered Q.8a to also answer this question.

In that part of the FI in which I considered students' personal meanings of a 'scientific observation', the didactic strategy that I attempted to follow consisted, roughly speaking, of the following 4 steps:

1 Bring the pupil to accept that had he attempted to write down all he could observe in the lab, he would never have finished. Draw attention to the fact that he had probably imposed several unbidden rules on his observation method even if 'naive', e.g. that he should not move, that he should use only his unaided eyes.

2 From (1) bring the pupil to realise that all observation /attention is selective.

3 Relate (2) to experiments. Bring the pupil to accept that experimental observations are normally restricted by the purpose of the experiment.

4 'Chance favours only the prepared mind'. Bring the pupil to accept that 'all observations are theory-laden'.

7.5.3: Analysis of Responses

My analysis of responses was conducted in 2 parts, (1) Q.8, 8a, (2) Q.8b.
WE Q.8 'Carefully observe, then write down what you have observed

WE Q.8a 'What difficulty have you had trying to answer Q.8?'

<table>
<thead>
<tr>
<th>Cat.No.</th>
<th>Category of Response</th>
<th>Freq.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Sophisticated ('What to observe?')</td>
<td>34</td>
</tr>
<tr>
<td>2</td>
<td>Naive (List of Observations)</td>
<td>31</td>
</tr>
<tr>
<td>3</td>
<td>Sophisticated? ('Do not understand Q.8')</td>
<td>9</td>
</tr>
<tr>
<td>4</td>
<td>No Response</td>
<td>6</td>
</tr>
<tr>
<td>5</td>
<td>Uncategorised data</td>
<td>0</td>
</tr>
</tbody>
</table>

Figure 7.42. Table Showing Categories of Response for WE, Q.8, Q.8a

WE Q8b 'Observations are often very important in experiments. Does the difficulty that you had in trying to answer Q.8 tell you anything about people doing experiments that have never been done before?'

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<tbody>
<tr>
<td>1</td>
<td>&quot;No&quot;</td>
<td>1</td>
<td>Unelaborated</td>
<td>15</td>
</tr>
<tr>
<td></td>
<td></td>
<td>2</td>
<td>Irrelevant to Science</td>
<td>3</td>
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<tr>
<td>2</td>
<td>&quot;Yes&quot;</td>
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<td>Need for Instructions</td>
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<tr>
<td></td>
<td></td>
<td>4</td>
<td>Should Observe More</td>
<td>2</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5</td>
<td>No Response (but answered Q.8a)</td>
<td>49</td>
</tr>
<tr>
<td></td>
<td></td>
<td>6</td>
<td>No Response (Neither Q.8a nor 8b)</td>
<td>6</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7</td>
<td>Uncategorised Data</td>
<td>1</td>
</tr>
</tbody>
</table>

Figure 7.43. Table Showing Categories of Response for WE, Q.8b

As usual, I present more detailed versions of these summary tables, my rationale for each and every category within them and further examples of students' responses in appendices (16(a), 16(b), respectively). In my interpretative discussion of these response categories, below, I shall
consider the questions roughly in the order that I presented them in WE.

7.5.4. Interpretation of Responses

From Figure 7.42 above, it can be seen that approximately equal numbers of students responded in ways which I construed to be 'naive' as did in 'sophisticated' ways.

My working criterion for 'sophisticated' responses was as follows:

'WE, Q.8, 8a, Category 1: 'Sophisticated': Responses in which students demonstrated at least some recognition of the basic flaw I intended to be in Q.8, viz. the unrestricted scope for observation rendered the question unanswerable in either practice or principle.'

  e.g. Class 2, r.18
  'I think the question is silly because anyone can observe hundreds of different things'.

  e.g. Class 4, r.12
  'The difficulty is that you have said Carefully Observe, Observe what?!! This is a badly laid out question no-one could give you a decent answer!! You have not told us what to observe.'

My working criterion for 'naive' responses was, by contrast, as follows:

'WE, Q.8, 8a: Category 2: 'Naive': Responses in which students straightforwardly described idiosyncratically chosen objects or events.'

  e.g. Class 2, r.9
  'Chalk dust on board, people moving around [!], Tony scratching his armpit.'

  e.g. Class 2, r.20
  'I have observed the fish swimming in the tank.'

I tentatively suggested that this relatively high proportion of naive-responses - roughly 40% of the total - is symptomatic of the prevalence of passivist, 'spectator' theories of knowledge:
Naive-responders appear to have unwittingly and unproblematically assumed responsibility for generating categories for observation. I conjecture that many of these individuals would also regard observations made within experiments as self-evidently 'given' responses.

FIIs often pointed to the dangers and inadequacies of deriving inferences about individuals' beliefs from a questionnaire. Such as comments made by two pupils in FIIs did, however, support my decision to derive a third 'Sophisticated?' category of response. My criterion for including responses in this category was as follows:

'WE, Q.8, 8a: Category 3: 'Sophisticated?': Responses which I felt unwilling to classify as 'naive' but whose 'sophistication' I either doubted or otherwise felt was an inappropriate classification.'

Although I went on to discern three sub-categories within this category (see Appendix 16), most (n=4:2:2) were where the student responded, without further elaboration, that they had been unable to answer Q.8 because they had not understood it:

e.g. Class 3, r.1
'\text{I have not been able to answer Q.8 because I don't understand the question.}'

In an FI later however, the same student, Paul (FI.5), did express (or develop) his rationale behind his WE response:

086 I [... you've written here. I have not been able to answer Q.8 because I don't understand it....

P .....mmm.....

I why didn't you understand it?

P couldn't see what we had to observe!
This verbal response "upgraded" my interpretation of his original WE response from 'sophisticated?' to 'sophisticated' and serves as a salutory reminder as to the degree of authenticity that should be attributed to my other categorisations.

Six students recorded no attempt to answer any of the CKT questions (i.e. Q.8, 8a, 8b). 31 had given 'naive' responses to Q.8, 8a. This left 42 to answer Q.8b. Of these 42, 17 went on to give no response and the other 15 gave an unelaborated negative one (sub-category 1):

    e.g. Class 1, r.1

'No'

I suggest that it is risky to draw detailed inferences from these last two frequencies though, clearly, most of the 42 students who were "eligible" to answer Q.8b did not do so. So why did they not record any gained insight? The inferences that I intended the students to draw from Q.8, 8a roughly correspond to steps '(2)' and '(3)' of my didactic strategy, outlined above. Perhaps these students were naive empiricists despite their 'sophisticated' responses to Q.8, 8a? The 'conceptual leap' may have been too great for them in the manner in which I presented it to them, i.e. WE.

Only 6 students (Q.8b, sub-categories 3, 4, and (?) 7) recorded having derived any insight or benefit from their experience of having answered Q.8, 8a. Two of these students concluded that experimentalists should "try to observe more" (Q.8b, sub-category 4):
In the absence of further elaboration this would seem to be almost the reverse of the epistemological lesson which I intended them to draw. For these pupils, more observation - without reference to theory or experimental design - is better. This would seem to betray a sympathy to naive-empiricism in which knowledge and truth is a quantitative matter, a function of accumulated fragments.

Three pupils implied the need for "instructions" when conducting experiments (Q.8b, sub-category 3):

e.g. Class 3, r.14

'Yes it tells me that when doing experiments you should have full instructions'

e.g. Class 4, r.4

'He might not be clear explaining what is to be done'

Again it is difficult to say whether these pupils had drawn the epistemological lesson I had intended them to draw, namely, that the experimental observations are both guided and constrained by the purpose and design of the experiment or some other 'superficial' variant, e.g. instructions are necessary in order to answer a question. I suspect the latter because in each of these responses there was an implied passivity of the experimenter: instructions were 'received' rather than 'created'. This despite the fact that in my wording of Q.8b I had specified "(...) people doing experiments that have never been done before". The students
perceived passivity of the experimenter might reflect their school experience of experimentation - including that of the so-called 'discovery methods' of science teaching.

I tackled the theory-ladenness thesis in four structured interviews. Two of these students had given 'sophisticated?' responses to Q.8a (FI(P')4,6), the other two had given 'naive' responses (FI(P')3,7).

I began this section of the interview with a reconsideration of Q.8 and their response to it. A recurrent hurdle was that the pupils were loath to accept personal responsibility for the observations they had recorded (in the case of FI.7, voiced in the interview). Put another way, these pupils appeared loath to accept that there was not, and had not been, a hidden category of observations required by me, the researcher:

e.g. FI(P')3 (Michael)

310 M (...) I was looking round to see if there was anything to notice deliberately... maybe we were meant to observe people looking puzzled and so I wrote that down as I thought that's what the question had to do. make people look puzzled so the answer was being puzzled.

e.g. FI(P')4 (Nigel)

424 N well it says observe and then write down what you've observed so I took it that you'd got to write down what you'd observed as soon as you'd read that and that was the writing.

Through successive confrontations in discussion of their experiences I believe that I was able to bring three of these pupils to realize that observation is selective, both in everyday life and experiments. (N.B. the constraint of available time dictated that I terminate the interview very soon after I'd begun to consider the theory-ladenness thesis with the
fourth interviewee: FI(P')5).

e.g. FI(P')3 Michael)

327 M (...) its saying there're things which you can sort of sense at one time ... and things that you can deliberately observe and things that are sort of hidden that you can observe.

468 M (...) you're just noticing the things you wanna know (...)

The sophistication of this realization varied considerably. FI(P')3 seemed aware that in conducting a Benedict's test for reducing sugars there may be observables other than the colour change/non-change that might get missed out:

345 M (...) sometimes its sort of just so obvious that its an orange colour ... and sometimes you have to search through your head. is it a dye, is it poisonous. is it so and so.

FI(P')7 by contrast, ended by accepting that observation made within experiments was selective but, for him, this was trivially true. I could not get over the epistemological significance of this to the individual. I suggest that recognition by the student that attention/observation is non-random is still a long way from accepting CKT: I did not, in other words, make much progress beyond step '(2)' of my 4 stage didactic strategy, described earlier. This, I suspect, shall be a recurring difficulty in attempts to teach CKT by other means, notwithstanding certain reservations that I have with respect to the approach that I used, and which I shall discuss below.

I now feel that the fact that I debarred those students who had recorded a response for Q.8 from answering Q.8a and b must have left at least some of them with a sense of having "failed" the exercise.
Although I consider my application of the "Popper question" through a written exercise has provided some useful interpretations, any didactic intentions using this mode of application alone would seem unlikely to succeed. I suggest, however, that a classroom activity involving group discussion, i.e. more along the lines of Popper, would be a useful way of introducing CKT to science students.
CURRICULAR PHILOSOPHY AND STUDENTS' PERSONAL

EPISTEMOLOGIES OF SCIENCE

By David J. Swift

In Two Volumes

Volume Two

A thesis submitted to the University of Surrey in partial fulfilment of the requirements for the degree of Doctor of Philosophy, 1986
For my parents
'Virtually all our disciplines have relied on conceptions which are now incompatible with the Cartesian axiom, and with the static world view we once derived from it. For underlying the new ideas, including those of modern physics, is a unifying order, but it is not causality; it is purpose, and not the purpose of the universe and of man, but the purpose in the universe and in man. In other words, we seem to inhabit a world of dynamic process and structure. Therefore we need a calculus of potentiality rather than one of probability, a dialectic of polarity, one in which unity and diversity are redefined as simultaneous and necessary poles of the same essence'.

ABSTRACT

CURRICULAR PHILOSOPHY AND STUDENTS' PERSONAL EPISTEMOLOGIES OF SCIENCE

In this thesis I employ a constructivist epistemological stance (principally influenced by that due to George Kelly) to critically examine the curricular response to contemporary notions of truth, objectivity and knowledge.

I take science education (at both Secondary and Tertiary levels) as my special reference within the education system.

An important part of my work explores students' and teachers' personal meanings of science and scientific method, i.e. alternative conceptions of science, and I see it as contributing to the growing body of research concerned with alternative conceptions in science: the 'Alternative Conceptions Movement' (ACM) in educational research.

To help articulate my views on these matters I use an augmented version of a framework or model, developed by my immediate colleagues, for conceptualising cognitive aspects of science education and the transformation of scientific knowledge. My version of this framework features components under the following main headings: 'Scientists'-Science', 'Philosophers'-Science', 'Curricular-Science', 'Teachers'-Science', 'Students'-Science', and 'Childrens'-Science'.

I argue that, suitably augmented and interpreted, Kelly's theory is capable of rationally integrating existing ACM research, together with my own.
My classroom research uses a number of complementary investigative methods, some of them novel. These may be grouped under the following three headings:

- interviews
- lesson observations
- written exercises

I present an outline of a theory of teaching which is compatible with ACM research and make recommendations for future science teaching and research.

N.B. To avoid an insidious (male) sexism and 'his/her' formulations which I find tedious, I shall use plural forms throughout this thesis, e.g. their, themself.
ACKNOWLEDGEMENTS

If there is worth in this thesis, then that worth shall, in large part, reflect the intellectual and emotional environment which I have been privileged to share with my colleagues in the Department of Educational Studies.

First and foremost amongst these persons must be my supervisors, Dr. Maureen Pope and Dr. John Gilbert. Amongst the many intellectual invitations which they have extended to me over the years, I must, in particular, thank Maureen for suggesting that I read Kelly, and John that I read Lakatos. The impact of these two suggestions is, I believe, plain to see throughout this thesis.

I must warmly thank Dr. Annette Stannett for extending her hospitality to me during a critical period of my writing, for her astute advice in subjects far beyond her acknowledged field of expertise and for her professional handling of the references to this thesis.

Although I never quite overcame my awe of his intellect, Professor Lewis Elton has also been a tower of strength.

Mr. (later Dr.) Mike Watts and Mr. (later Dr.) Arden Zylberstajn were the first research students whom I met and whom I construed to take epistemological issues (almost) as seriously as I did. As such, they were a tremendous source of inspiration and support to me — they still are.
Other research students from whom I have especially benefitted in critical discussion and in friendship include Mr. (later Dr.) Don Horscroft, Mr. Gary Lafferty, Mr. Mervyn Phillips, Mr. Gordon Watson, and Mr. (later Dr.) Adel Yasseen. Also Mr. Leo Crossfield who was then a Research Officer in the department.

My friends at Manor House - Mr. Peter Boreland, Mr. Tony Chadney, Ms. Beverley Roberts and Mr. Mike Yeadon - contributed a welcome source of constructive criticism and stimulation. Similarly, my brother Mr. Richard Swift and my sister Ms. Margaret Swift (later Mrs. Boreland) together with their respective circles of friends.

I must thank Ms. Shelley Butler, Ms. Fiona Kirton, Ms. Yvonne Fullick, Ms. Tricia Jackson, Ms. Alison Lawrence, my parents Mr. James and Mrs. Vera Swift and Mr. Keith and Mrs. Jackie Williamson for the typing of this thesis.

Responsibility for any errors of judgement and of typography as might remain in this thesis is mine alone.
# CURRICULAR PHILOSOPHY AND STUDENTS' PERSONAL EPISTEMOLOGIES OF SCIENCE

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Chapter 8. Interviews

8.1 Introduction

My purpose in this chapter is to describe the means by which I explored in interviews educands' and educators' personal epistemologies of science. My interviews were of two types: (1) formal and pre-focussed, (2) informal and opportunistic.

8.2 A Focussed Interview (FI) for Exploring Educands' Personal Epistemologies of Science

I developed my focussed interview (FI) through 3 main variants, the first 2 of which were applied principally in pilot studies. These may be designated as follows:

FIv1. '5 Categories', ) Pilot Studies (n=47).
FIv2. '3 Questions', )
FIv3. '15 Questions' ) Main Study (n=22).

These I shall now describe:

8.2.1 Pilot Study FIv1. '5 Categories'

This was where my entire thesis investigation began. To help focus and guide my initial investigations into the nature and development of educands' personal understandings of ideas in biology (cf. Ch.1), I examined all the 'O' and 'A' level biology syllabi and developed a five
part categorization of central themes or concepts in the discipline. These themes were

Life,
Cell,
Heredity
Evolution,
Ecology.

My pilot fieldwork actually began in a Middle School but I rapidly included Secondary Schools (for details see section 8.5.1., below) and conducted the work more or less concurrently. I chose to initiate my classroom research towards the lower end of the education system for three, highly related, reasons: firstly, although even then I anticipated basing my research at the secondary level, I desired to have an overview of the entire school education system in order to provide a context for my main study; secondly, as an intending researcher into concept development this seemed to me to be the most reasonable place to start; thirdly, I wished to make an informed choice of target population for my main study.

My work in the Middle School consisted of three, day long, visits. I spent these days informally observing lessons and generally getting used to talking to children of that age again. In my lesson observations, I noted down what I perceived to be the subject content of the lesson together with any other aspects which I found interesting, but especially anything which I construed to be a pupil's difficulty in understanding an idea. In addition, on the first day of my visit, I conducted an informal "group" interview with five 11 year olds.
Although informal this "group interview" was conducted in private and was, with the prior consent of the pupils, audio-recorded. I simply asked them "What do you think biology is?" and encouraged them to debate it amongst themselves. I judged a familiar broad consensus that biology was "the study of living things" soon to emerge. I was, however, particularly struck by the comment of one individual named Mark, who confidently claimed that biology was "the scientific way of studying nature" and that it was 'not like nature study'. It seemed to me that whilst this view was also fairly readily accepted by the others in the group, some pupils differed in the way in which they understood the expression "the scientific way". Accordingly, when I interviewed these pupils individually the next day, in addition to eliciting their personal meanings of (up to two) terms selected from my 5-part thematic categorization of biology, I asked them to articulate their personal meaning of Mark's expression "the scientific way". Thus it was held variously to be "grown up", "detailed" and "serious". It was "clever" because "you use complicated equipment and chemicals".

Meanwhile, in a series of twelve, day long, visits divided amongst three Secondary Schools, I similarly interviewed three individual pupils about aspects of the lesson(s) in which I had observed them. With these pupils I elicited their personal meaning of 'biology' and each of the five terms comprising my thematic categorization of the discipline - this providing a common, structured, component to my interviews. As with my work in the Middle School, with the prior consent of both the teachers and pupils concerned, I audio-recorded both the lessons I observed and the interviews I conducted.
During my initial interviews with these Secondary school pupils I noticed that all three of them appealed to 'scientific method', 'experimental proof', and the like in the course of justifying (as I judged it) their views in and of biology, whether they be "alternative" or "orthodox". Such appeals were most often left unelaborated unless prompted by me. In one interview, however, a 14 year old, 4th year pupil, named Peter (P), introduced within our exploration of his views on heredity a personal meaning for 'control experiment' which was at striking variance with the orthodoxy. The exchange occurred after he had correctly listed some of the advantages of using drosophila fruit flies in genetic studies:

P. ( ) biologists tend to use small organisms like/for controlled experiments....

I. Why do you think that is?

P. Well..small insects like that are more easily controlled... you don't need big strong cages for them and they're not dangerous.

After further questioning I was convinced that Peter meant this both literally and sincerely. When I asked him if there might be any other meanings for the expression 'controlled experiment' his answers veered into notions of biological control. (i.e. artificial control of pests and parasites by use of other organisms).

Now, I have already commented upon the nature and importance of the control experiment in (especially biological) science and upon the presence of other 'alternative conceptions' which I elicited from pupils in informal interviews within my early lesson observations (see Ch.4). Accordingly, after these initial FIs I resolved to refresh and to clarify my personal understandings of basic principles of experimental design. I soon found, however, that such principles, as
classically presented in text-books, rarely helped me to understand the way in which pupils were using expressions such as 'scientific', 'scientific method' and 'experimental proof' for they seemed to me to be invoking qualitative differences or levels of truth. These, I soon found, could only be elucidated by appeal to philosophy of science.

My growing interest in educands' personal meanings of science and scientific-method was further deepened and extended by two sources of interaction with University students.

Firstly, through my teaching involvement with Dr. John Gilbert on a departmental course on 'Science in Society' I became exposed to first year undergraduate scientists' ideas on these subjects. Certain of these students' responses to our talks led me to believe that most of them subscribed to an 'accumulative fragmentalist' epistemology of science. I first felt this strongly in a lecture which I gave upon the subject of 'The Role of Metaphor in the Conduct of Science'. In this I interpreted the majority of students to express a clear preference for the traditional, empiricist inspired, 'substitution' and 'comparison' theories of metaphor as opposed to the more recent, semantic, 'interactive' theories of metaphor (see Black, 1979 cf. my discussion, section 8.2.2.1., below). The students' scepticism of anything suggesting relativism or even fallibilism in scientific knowledge claims was expressed even more clearly in a later lecture in which I discussed the ideas of Kuhn and which, in these respects, were in close agreement with the epistemology underlying my pedagogical stance.
Secondly, at this time I was resident on the University campus. Challenges to Darwinian theory were then newly popularized in the national press (the so-called 'Creation Controversy') and were a frequent source for debate amongst students. There was much discussion about whether fossil evidence could ever constitute scientific evidence and, consequently, whether Darwin's theory could be held to be a scientific one. Again, this exposed me to students' personal meanings of science and scientific-method. I took advantage of my situation by interviewing a small, opportunistically selected, sample of university students drawn from a broad range of academic years and scientific disciplines. In one such interview, with a 20 year old, 1st year undergraduate Human Biology student named Janet (J), the following exchange occurred during my exploration of her personal meaning of 'evolution':

I. ( ) just now you referred to natural selection as the law of natural selection...can you tell me why you did that?

J. Yeah I did that deliberately because its only because its traditional to call it the theory of natural selection because that's what Darwin called it. Everyone knows it's really a law ( ) apart from the ultra religious types...

I. ( ) why do you think Darwin himself called it the theory of natural selection then?

J. Well because he was just being modest... he had to be modest because he knew he was going against the church and would probably get excommunicated and what have you. that was something a lot more important to people in those days...

I. So....calling something a theory....that's being modest?

J. Yeah definitely.

I. ( ) how do you mean modest?

J. When you say its a theory you're saying how about this to explain it but I might be wrong what do you think but when it's a law that's it. that's how it really is....

- 3.6 -
What interested and impressed me most at the time of this interview was Janet's absolutism with respect to the truth status of scientific laws in general and Darwin's "law" in particular. Our discussion turned to the relative merits of the various factions comprising the 'Creation Controversy'. Later, however, my interpretative interest in this and other parts of our dialogue focussed upon the differences between her personal meanings of the terms 'hypothesis', 'theory' and 'law' which, although most usually embedded and used in the manner of merely descriptive labels in scientific discourse, are properly a part of scientific meta-theory. The choice and use of such terms necessarily imply epistemological commitments. Distinguishing between meanings of, for example, 'law' and 'theory' invokes epistemological issues and controversies such as form vs. corroborating empirical evidence or corroborating empirical evidence vs. acceptance by a consensus with the community of scientists as well as relative levels of truth and objectivity (hypothesis: theory: law). Again, these were concerns which I realised would not adequately be elucidated by reference solely to principles of experimental design and this contributed to my decision to shift the focus of my work towards philosophy of science.

As my first, tentative, step in this direction, I appended the following question to my '5 Categories' FI:

"What do you understand by the expression 'scientific'?'"

I have culled the following brief excerpts from these interviews to illustrate responses to this last question:

"scientific means machines like computers which calculate things accurately"
(Paul, aged 11 years, Middle School pupil).
"Margaret Thatcher thinks she's scientific but really she's unscientific because nothing she does works"
(Susan, aged 11 years, Middle School pupil).

"scientific is being a good observer... scientists don't miss anything. that's why they know the most."
(Greg, aged 13 years, 3rd Year Secondary School pupil).

"scientific means logical ( ) scientists don't rely on imagination. they use proven logic."
(Tony, aged 15 years, 5th Year, Secondary School pupil).

"these days scientific is taken to be a synonym for intelligence... basically if you're scientifically minded you're bright. if not. you're dim."
(Chas, aged 18 years, 1st Year Undergraduate Chemical Engineering student).

"if something's described as scientific it means that its been shown to be true by experiment ( ) not just maths and speculative reasoning."
(Pauline, aged 18 years, 1st Year Undergraduate Physics student).

"scientific suggests a certain degree of order and logic in your approach. both theorizing and experimental work. it's when you depart from this for any time that the blunders get made."
(Mike, aged 24 years, 1st Year Undergraduate Chemical Physics student).

"to be granted the status of a scientific subject it has to be seen to have been developed by the technique which confines it to the facts ( ) subjects which do not have this technique do not deal with facts. the technique I am talking about is of course the scientific method."
('Oti', aged 30+, 2nd Year Postgraduate Chemistry student).

Whilst I have been highly selective in presenting quotations from these particular individuals, I do not judge the contexts from which these excerpts came to have much elaborated or ameliorated their meaning - no doubt partly due to the crudity of my interview technique. Even at this early stage of my research, however, I considered many of the students that I was interviewing to express views on science and scientific-method which I felt to be at variance with those held by myself and modern post-empiricist traditions in philosophy of science and many of the stated objectives of modern science curricula, viz. they emphasise logicism, naive-empiricism and absolutism. It occurred to me
that students' alternative conceptions of science and scientific-method might be as prevalent as those of the biological concepts which had been the main subject of my investigation and, further, that the two might be causally connected, i.e. students' alternative-conceptions of science (and scientific-method) might help to explain the origin and maintenance of their alternative-conceptions in science. This latter inference is strengthened by the prima facie case that scientific knowledge is legitimized in the science classroom, as in science, primarily by appeals to scientific-method (understood in both specific and general 'meta', senses, and whether (re)enacted or merely reported).

Accordingly, I endeavoured to gain some acquaintance with philosophy of science (or, as it turned out, with philosophies of science) and to develop a new version of focussed interview with increased emphasis on eliciting students' personal epistemologies of science.

8.2.2 Pilot/Main Study FIv2' 3 Questions'

I conducted investigations using this second variant of FI in two, distinct, periods which, although sharing a common core of questions, might, for other reasons, be distinguished into two sub-variants of FIv2, viz., '(a)' and '(b)'.

8.2.2.1 Pilot Study FIv2(a).

In these interviews my sample of educands came from Middle, Secondary and Tertiary levels of education, as before. With the Middle and Secondary school pupils, however, I now began to slant my questions relating to the lessons in which I had observed them to issues of method
and methodology (see Ch.4). Another change which I made was to drop my '5 Categories' FI in favour of another intended specifically to explore educands' personal meanings of science and scientific-method.

Prior to these interviews I had striven better to acquaint myself with 'scientific method' - a task which, for reasons already given (section 8.2.1., above), required that I study philosophy of science. From a survey of the relevant literature, both specialist and introductory, I selected 6 traditions from philosophy of science whose ideas I judged to continue to exert significant influence within the "community of philosophers of science". The traditions are associated principally with the following names:

Bacon
Schlick,
Popper,
Kuhn,
Lakatos,
Peyerabend.

My purpose in identifying these major traditions and achieving at least a basic understanding of them was twofold. Firstly, I intended my grounding in philosophy of science to enable me better to identify and to characterize whatever conceptions educands might have of science and scientific method. Since I judge the traditions which I have listed to be the most influential and long lasting ones, I anticipated that many educands' conceptions might at least approximate to one or more of them. I intended, however, that these traditions be used only as a guide to help me to interpret the nature of educands' responses, not as "norms"
to measure them against. My personal epistemology and pedagogy allows for a pluralistic viewpoint within a person. Secondly, I intended my personal education in philosophy of science to inform my construction of FIV2, viz. by examining these 6 traditions I identified a number of highly interrelated topics or issues which were common to them:

- demarcation criterion for 'science', 'scientific';
- 'scientific-method' as a universal procedure, a "method without a subject";
- criterion for theory-choice and the growth of scientific-knowledge;
- the role of history of science in the conduct of science;
- the truth and/or objectivity of scientific knowledge claims;
- the role and value of science in society.

I then devised three questions intended to elicit educands' views upon such issues, viz.

Q1. What do you understand by the expression 'unscientific'?  
Q2. What do you understand by the expression 'scientific-progress'?  
Q3. What do you understand by the expression 'scientific-method'?  

In the first question I decided to use the term 'unscientific', rather than 'scientific' (as per FIV1), in order to avoid the individual merely reciting a popular cliché or algorithm which might not truly represent their views but which they might then feel trapped into justifying. I had inferred from earlier interviews that educands felt strongly that they ought to be able to define 'scientific', especially if they were studying science, and notwithstanding the considerable difficulties they often had in doing so. This was in opposite contrast with the term 'unscientific' which almost all educands seemed happy to elaborate upon. In interviews the emphasis characteristically shifted.
to a consideration of 'scientific' after the individual's meaning of 'unscientific' had been elicited, whether facilitated by myself or not.

In responses to my three questions, educands from all educational levels tended to express surprise at the questions I asked. Many claimed that they had never been asked them before or even that they had never thought about them before. Almost all professed extreme difficulty in answering them – an experience which I judged to be pedagogically important due to the prevalence amongst science curricula of major teaching and learning objectives concerning 'the process of scientific investigation and reasoning' (Fensham, 1983, p.5 - draft of paper circulated in 1980 c.f my discussion in Ch.2).

In their responses to Q1, educands claimed usually to have heard the expression 'unscientific' but were not sure where. However, they commonly recalled it as having been used in a rather pejorative sense to indicate subjectivity, equivocality, emotionality, inaccuracy and even fraud. Thus, for example:

"unscientific means the man [sic] could have cheated."
(Simon, aged 10 years, Middle School pupil).

"unscientific means easy things like P.E. [Physical Education]."
(Heather P., aged 11 years, Middle School pupil).

"unscientific investigations are normally superficial and prone to error but they can be right by accident."
(Mike S., aged 16 years, Lower 6th Year Secondary School science pupil).

"unscientific subjects [ ] are areas of study which lack logic and objectivity."
(Stephen, aged 21 years, 1st Year Undergraduate Physics student).
"unscientific theories are myths [] they show what people want to believe rather than what is true."
(Mike L., aged 18 years, First Year Undergraduate Mechanical Engineering student).

"unscientific means a theory where the inventor has filled in the gaps between the facts but has not admitted this publicly."
(Eric, aged 22 years, Fourth Year Undergraduate Biochemistry student).

When I went on to elicit educands' personal meanings of 'scientific', most of them began by giving accounts which might be characterised as 'true by definition' or, as what amounts to the same thing, 'true by traditional subject classification'. Examples of these respective types of response are:

"unscientific is when you havent got a knowledge of science [] scientists study science specifically."
(Doug., 18 years old, Upper 6th Year - "7th Year" - Science pupil).

"science is the study of scientific things: physics, chemistry, biology."
(Sue K., 14 years old, 4th Year Secondary School Pupil).

On further questioning such educands characteristically expressed positive valuations of 'scientific' in counter-part to their earlier negative ones of 'unscientific'. In these responses the quality of objectivity, or "value-neutrality", of scientific investigations was stressed by educands and (in my interpretation) provided support for Chalmers' assertion that 'The naming of some claim or line of reasoning or piece of research "scientific" is done in a way that is intended to imply some kind of merit or special kind of reliability.' (1982, p.XV).

With Middle School pupils chemistry was held to be the most scientific of the three traditional sciences by virtue of its involvement with chemicals and scientific impedimenta (test-tubes etc.). By contrast,
almost all Secondary School pupils and University students ascribed this status to physics using mainly reductionist arguments such as this:

"The better a subject is the more scientific it is [and] the more scientific something is [the more fundamental its got to be....physics is the best science because thats what all the others can be boiled down to. its at the heart of literally everything [chemistry and biology naturally., but I wouldnt be surprised if even french turned out to be physics at the end of the day."
(Jim B., aged 17 years, Upper 6th Year, Secondary School science pupil).

The marked tendency which I noticed for interviews to list scientific and unscientific (or non-scientific) subject-disciplines and occupations as a first stage in their formulation and/or expression of their personal meanings of the otherwise decontextualised term '(un)scientific' supported my decision to compile a list from interview protocols and to use them in the elicitation process. I selected exemplars from each category ('scientific', 'unscientific') and some whose status I judged to be equivocable. These I later used with accompanying pictorial representations ('Occupation Cards') in a manner which I intended to be an application of Interview-About-Instances (IAI) technique and to offer a more accessible (because contextualised) form of Q1 in my 3 Questions FI (for a critical account of this attempted elicitation method, see section 7.4.2.1.). Although I soon abandoned this adjunct to my 3 Questions FI due to methodological criticisms I had of it, my experience of using Occupation Cards did help me to develop an initial awareness of the potentialities of using occupations as 'elements' for pictorial methods of eliciting personal constructions (later explored in my main study use of Repertory Grid ,PA Techniques and Responding-With-Pictures techniques, respectively - see Ch.7).
In their responses to Q2, educands unanimously construed the expression 'scientific progress' in a manner which supports Putnam's (e.g. 1978) assertion that science has come to mean a term for the successful pursuit of knowledge. Examples of such responses include:

"when you find out that something's for definite that's scientific knowledge but when it's something that nobody knew before it's scientific progress."
(Simon, aged 10 years, Middle School pupil).

"scientific progress [] that's the discovery of more facts and laws of nature."
(Christopher, aged 14 years, 4th Year, Secondary School pupil).

"scientific progress means [] the gaining of knowledge [] we've definitely improved."
(Doug., 18 years old, Upper 6th Year - "7th Year" - science pupil).

"scientific progress [is] man's increasing understanding and mastery over the world. the universe as a whole."
(Mike L., aged 18 years, First Year Undergraduate Mechanical Engineering student).

I further explored educands' personal meanings of scientific progress usually by asking "How do you think science progresses?" In their responses to this question, interviewees usually cited scientific-method as the main mechanism. This aspect of their replies led me into an exploration of issues considered in my discussion of their responses to Q3, below. As a prelude to elaborating upon their views of scientific-method, however, educands often expressed historical appraisals of scientific progress which I interpreted as commitments to variants of 'accumulative fragmentalist' epistemology of science, for example,

"somebody does some work and somebody else carries on... so it's built up stepwise really."
(Mike S., aged 16 years, Lower 6th Year, Secondary School science pupil).

And in this perhaps more sophisticated version where the pupil employed
a theory of scientific progress by successive entailment of theories:

"to advance science [ ] you must know all the accepted theories so you can make up a new one by scrunching them together [ ] the newest theory contains all the old ones. it swallows them up.. they're not gone. they're hidden inside if you look hard enough."

(Sian, aged 17 years, Upper 6th Year, Secondary School science pupil).

These last two quotations also exemplify something else that was very common in educands' answers to my 3 questions, namely, their tendency to use extended metaphors, similes and the like (N.B. henceforth I shall use the term 'metaphor' to refer to all such forms). This tendency was central to the first of 2 compound reasons why I decided to elicit metaphors as part of my interview method:

My first reason originates from my judgement that interviewees who used metaphors found them to be a very helpful vehicle with which to formulate and initially articulate their ideas. I partially explain this by virtue of the novelty which many interviewees claimed my questions had for them, a quality which meant that they lacked a ready "form of words" with which to answer. In further and complementary elaboration of why metaphor may facilitate response to such questions, many of the issues and ideas which I wished to elicit from, and to explore with, interviewees were profoundly metaphysical in nature. Accessing one's metaphysical commitments is notoriously difficult and, indeed, it is normally possible never to need to do so (Taylor, 1974, p.1). Again, metaphysical commitments are often highly personal beliefs constituting 'values', 'core constructs', 'constructive principles' and the like (cf. discussion, section 4.4.). The critical and interrogative qualities of the interview situation, particularly with respect to metaphysical beliefs, may have evoked feeling of 'threat' and/or of
'hostility', in Kellyan senses (see Appendix 3), on the part of the interviewees and may have been behind their often repeated claim to find my questions difficult to answer. Metaphorical expression may be especially suited for eliciting and discussing metaphysical beliefs because it allows extra room for personal and negotiated development (and re-interpretation) of meaning by virtue of the "non-literality" of the metaphor's reference. This last point may best be developed through a brief, joint, consideration of both the form which metaphysical commitments often take and of modern theories of the function of metaphors. Such a consideration, however, comes under the purview of my second reason for deciding to elicit metaphors:

My second reason, then, is intimately connected with my interest in theories of metaphor, an interest which was prompted by Dr. John Gilbert who, early in 1981, introduced for discussion within the Personal Construction of Knowledge Group (PCKG) the collection of papers entitled Metaphor and Thought, edited by Ortony (1979). The bulk of essays in that collection are written from a constructivist perspective and it was naturally these which we explored in our discussions and seminars.

In a paper written and circulated in draft form during that time, Pope and Gilbert provide a lucid summary of a framework of perspectives for appraising metaphor proposed by Max Black who is one of the leading constructivist theorists:

'Firstly, there is the substitution view. According to this the entire sentence that is the focus of the metaphor can be replaced by a set of literal sentences. In the comparison view, the sentence containing the metaphor can be reduced to a paraphrase which contains an
A third model, the interactive view, can be summarized as follows:

In the metaphor 'A is as B',

(i) 'A' is the 'primary subject' and 'B' is the 'secondary subject';
(ii) the secondary subject can be regarded as a 'system of relations', or 'implicative complex';
(iii) the metaphor works by 'projecting upon the primary subject a set of "associated implications" comprised in the implicative complex, that are predictable of the secondary subject';
(iv) 'the maker of the metaphorical statement selects, emphasizes, suppresses and organizes features of the primary subject by applying to it statements isomorphic with the members of the secondary subject's implicative complex';
(v) the two subjects interact as follows: the primary subject causes the learner to select some of the secondary subject's properties and causes construction of a parallel implication complex to fit the primary subject which reciprocally induces parallel changes in the secondary subject.'


Pope and Gilbert's main concern in that paper was to formulate a set of empirical questions concerning the use of metaphor, construed constructivistically as per, e.g., Black's interaction model, in explanation in the context of science education.

For my part, I saw the exploration of interviewees' metaphors primarily in terms of methodological triangulation, viz. as an additional means of construing their ideas - their personal explanations - dimensionally, invoking similarities and contrasts, without using classical Kellyan elicitation techniques.

Constructions of a profoundly metaphysical nature are often expressed initially in an overtly metaphorical form, e.g. light-wave. It thus did not surprise me that interviewees characteristically employed metaphors when answering Q2 (re. their personal meaning of 'scientific progress')
since it may be argued that it is in that question that metaphysical
commitments, concerning ultimate reality and our access to it etc, come
most to the fore.

With this in mind, I initially considered supplying metaphors culled
from the writings of influential philosophers of science and/or their
commentators to explore educands' personal meanings of 'scientific
progress'. Such an approach has been successfully used by Beck, an
anthropologist, who supplied a set of 4 choices of (e.g.) vegetal images
to interviewees in order to elicit their personal constructions of
'ethnicity' and 'ancestral identity' (Beck, 1981). Despite strenuous
attempts, however, I could not find (or create) metaphors which I judged
to be either specific enough between epistemological traditions, on the
one hand, or general enough within such traditions on the other hand. I
also felt that it would be closer to the principles of my research
philosophy if I elicited metaphors. This I did with a handful of
postgraduate students by first asking them if they could give me an
example of a metaphor or an analogy. If they responded with what I
considered to be an authentic example I went on to explain how any
metaphor will, in addition to implying some quality of similarity
between referents, also "break down" in some area(s) of comparison.
This I demonstrated with either their example or one of my own
(unrelated to science). With this done I asked the following question:
'Can you give me a metaphor for 'scientific progress'?'

One student's metaphor which I elicited in this way was as follows:
"science is like a projector which scientists are trying to focus. We know that there is a perfect clear picture somewhere at the end...but they are not sure, cannot be sure whether they will ever be able to focus to it. They all we can be sure of is that the pictures we are getting now are better than those earlier ones and science is getting quicker all the time. At any time the picture is blurred. We know this because scientific predictions often fail to be exact. We know we're going in the right direction because... by comparing later predictions with earlier predictions we can see that we're advancing in the right direction because the predictions are becoming less approximate. You know, more sharp when the end of science is reached everything will be clear everything will work and have its place, nothing will be a mystery."

(Stephen A., aged 25 years, 3rd Year Postgraduate Biochemistry student).

I construed this metaphor to indicate a commitment to convergent epistemological realism and to objectivist epistemology interpretations which I soon corroborated by further questioning. This student expressed enjoyment at having thought about science in this way and said that he was glad that I had not asked him "cold" - a sentiment expressed by some of the others.

Encouraged and informed by these early successes, which seemed to promise a novel means for short circuiting the elicitation process, at least with this particular question, I developed the following guidelines for eliciting and exploring metaphors:

1. Ask the interviewee if they know what a metaphor is (e.g. "Can you tell me what a metaphor is?").

2. If you consider the interviewee to have a personal understanding of metaphor which is suitable for your purpose, then elicit any metaphor (e.g. "Can you give me an example of a metaphor?"). This shall be the 'specimen metaphor'. Usually the interviewee shall have generated one spontaneously in their answer to '(1)'. If the interviewee has difficulty, however, it may be helpful to provide one, making sure that it is as unrelated to your intended universe of discourse as possible.

3. Specimen metaphors obtained by either means (i.e. "spontaneous" or "provided") may need negotiation, viz.
converting into a mutually clearer or otherwise more convenient form.

(4) Explore the specimen metaphor, viz. draw attention to its features of (a), non-literality (non-identity of referents), and, by way of elaboration, (b) similarities and, (c), contrasts between its referents (i.e. areas of comparison in which it "breaks down").

(5) Explain that the features you have identified are common to all metaphors; that the specific manifestations of these features were found by your exploration of their specimen metaphor and it was these which enabled you to understand their personal meaning for that metaphor; that you shall wish to repeat the exploratory process with another metaphor which you shall soon ask them to think up.

(6) Elicit the 'research metaphor'; e.g. "Can you give me a metaphor for scientific progress?"

(7) Negotiate the research metaphor if necessary - as per '(3)'. Repeat '(4)' in light of your specialised knowledge of the subject area.

I used this technique - 'elicited metaphors' - with the 7 pupils comprising the F(I(P')) population of my main study (see table 8.4., below; for a full example and discussion of this technique see Chapter 9).

In their responses to Q3, a minority of pupils attributed neither investigative nor epistemic qualities to 'scientific-method'. Thus, for example, I construed one pupil to understand 'scientific-method' as a very broad characterization of the activity of conducting science differing from that in science classrooms only in degree, not in kind:

"real scientists do the same as us but they use more complicated equipment...and maths...ours is never very dangerous."

(Terry, 17 years old, Lower 6th Year Secondary School science pupil).

Another such pupil seemed to construe scientific-method as a convention for the clear communication of facts for assessment later:
"We use scientific-method to write our experiments down in [ ] it's so Mr. [X] can mark it easier."
(Jackie, aged 14 years, 4th Year Secondary School pupil).

This comment reminded me of Ms. S' comments concerning her attempts to teach her pupils "the basic scientific method" (see Ch.6.), an account which recapitulates the 'scientific fraud' described by Medawar (1964) and which in turn embodies an essentially Baconian, accumulative-fragmentalist, epistemology of science.

I did, indeed, construe the majority of pupils to articulate an accumulative-fragmentalist epistemology of science, some in an almost overtly Baconian manner:

"First you do your observations to get your hypothesis...then you can prove your hypothesis by doing a test of it... if it happens like it says it should, you're right... if not you start again taking more care this time."
(Mike S., aged 16 years, Lower 6th Year, Secondary School science pupil).

Finally, a recurring idea in educands' responses which may be argued to complement the last one was that scientific-method is an algorithm for the achievement, or at least, progress towards the achievement, of objective or factual knowledge. In such constructions the main contribution of scientific-method is the negative one of excluding unscientific elements in thought and action (cf., for example, the last quotation from Mike S. re. 'scientific-method' with that for his view on 'unscientific', quoted earlier). This view was particularly strongly held amongst the older educands whom I interviewed:

"the use of scientific-method stops scientists getting carried away with themselves [ ] it stops the discovery of new theories being contaminated with things the scientist might want to believe [ ] in this way science gradually sorts facts from fiction."
(Anita, aged 20 years, 2nd Year Undergraduate Biochemistry student).
I hope that the quotations which I have excerpted from the interviews of FIV1 and FIV2(a) illustrate the sorts of categories of response which I was beginning to discern. Whilst I believe that these interviews provide a compelling case for the view that amongst the educands whom I interviewed there was a prevailing commitment to some kind of empiricist epistemology of science - and it can be put no more precisely than that - I came to doubt the subtlety and sophistication of my 3 Question approach. In some of my later FIV2(a) interviews which tended to be longer I discerned slight indications that whilst some educands might initially elaborate and endorse (say) an empiricist-inductivist epistemology they might later in the interview seem to favour (say) a Weltanschauung one, or vice versa. In some cases like these I suspected that the context of our discussion (e.g. specific episodes from the history of science) might influence the educand's epistemological commitments - a feature that would be consistent with my own epistemological stance which both embraces context dependency of meaning and tolerates epistemological pluralism. The approach I adopted in phase 2 of my 3 Questions approach - FIV2(b) - was influenced by such possibilities and considerations.

8.2.2.2. Main Study FIV2(b)

I conducted these interviews with only 7 educands drawn entirely from the 3rd and 4th Years of a Secondary School.

Partly in an attempt to develop FIV2, I augmented my 3 Questions with a prior Written Exercise (WE), as discussed in Ch.7. Pupils' responses to each of the 3 sections of WE provided me with information as to
their views roughly corresponding to my 3 Questions in the following way:

WE Section 'A' re. classification of a scientific job: FIV2, Q1 re. unscientific;

WE Section 'B' re. illuminative incident of scientific activity: FIV2, Q3 re. scientific-method;

WE Section 'C' re. theory-ladenness of observations: FIV2, Q2 re. scientific progress.

In each interview I had the pupil's WE before us. This provided not only a useful entree into my 3 Questions of FIV2 within the interview itself, but also on-going feedback for the development of my WE which went through a number of revisions partly as a result of this. The interviews also provided some degree of corroboration that the occupation classifications of WE Q1 would, indeed, constitute 'representative elements' in my later application of a supplied element Repertory Grid.

In addition to augmenting FIV2 with references to WE I had observed two lessons immediately prior to conducting this set of interviews. I had chosen to observe each of these lessons partly due to their respective teachers' common intention to use a model as part of the experimental design and partly due to my desire to pilot my use of a lesson observation method developed by Schatzman and Strauss (1973, as described in Ch.4. Five of the seven interviewees were drawn from one or other of these lessons. With these pupils I asked questions relating to the specific experimental design used in each lesson (which was of intrinsic interest to me) and so was also able to appraise the relative efficacy of Schatzman & Strauss' observation method in the cause of 'methodological triangulation' (discussed in Ch.5.) relative to my own methods which I had used before.
Although I originally intended these interviews to be only a part of my pilot-studies, in the event I judged the material they generated to be sufficiently rich to be included within my main study: the FIv2(b) interviews correspond to the FI(P') educands of my main sample, described in section 8.5.2., below. This notwithstanding, prior to interviewing the remaining educands comprising my main sample, I strove to improve my interview method further: FIv3.

8.2.2.3. Main Study FIv3. '15 Questions'

My sample of educands for these interviews was as co-extensive with my main-study class/tutorial-group observations as possible, viz. 7 x Lower 6th Year science pupils - 'FI(P")'; 8 x 1st Year Undergraduate science students - 'FI(St)' (see Ch.6 for my discussion of class/tutorial-group observations). Within FIv3, however, I tended only to refer to observations I had made in a class or tutorial group if they concerned that particular interviewee and if I felt that by my doing so I might develop my personal understanding of that interviewee's ideas. Alternatively, an interviewee, knowing that I had been present at the time, might occasionally include within their exposition a reference to a discussion or some other incident which took place within a lesson or tutorial. In FIv3, then, my class and tutorial observations served primarily as a critical background knowledge informing my interpretation of interviewees' responses.

As my account so far implies, by the time of these interviews I had given up as over-ambitious my earlier plans to link specific alternative-conceptions of science with specific alternative-conceptions
in science by way of an explanation - I suggest that such would make an interesting project for future research for the origin and maintenance of the latter (see Ch.10). This notwithstanding the fact that I was beginning also by now to find independent "theoretical" corroboration (i.e. a complementary philosophical rationale) for this conjecture in the writings of both Kelly and Feyerabend and whose basic stances I had embraced almost from the beginning of my research, e.g.

'Men not only construe their alternatives, but they construe also criteria for choosing between them' (Kelly, 1969, p.85)

'We concede that our epistemic activities may have a decisive influence even upon the most solid piece of cosmological furniture - they may make gods disappear and replace them by heaps of atoms in empty space.' (Feyerabend, 1978, p.70).

My experience from FIv2(a), (b) corroborated my judgement that only one form of FI would be necessary for application across Secondary and Tertiary populations of educands. I made the majority of my improvements to FIv2 by examining the sub-questions and elicitative tactics which I had been using and then including more of those which I judged to have worked both more explicitly within interviews and more consistently within interviews. I was also guided by the tentative categories of response which I had created in earlier pilot-studies. (This very largely explains the compatibility, for purposes of interpretation and analysis, of FI(P') with FI(P") and FI(St) populations of educands). Finally, I also found Klemke, Hollinger and Kline's list of 'some main topics in philosophy of science' helpful in this task:

'(1) The formal sciences: logic and mathematics. Logic and math are often referred to as sciences. In what sense, if any, are they sciences? How do we know logical and mathematical truths? What, if anything, are they true of? What is the relation of mathematics to empirical science?
(2) Scientific description. What constitutes an adequate scientific description? What is the "logic" of concept formation which enters into such description?
(3) Scientific explanation. What is meant by saying that science explains? What is a scientific explanation? Are there other kinds of explanations? If so, how are they related to those of science?
(4) Prediction. We say that science predicts. What makes this possible? What is the relation of prediction to explanation? What is the relation of testing to both?
(5) Causality and law. We sometimes hear it said that science explains by means of laws. What are scientific laws? How do we serve to explain? Further, we sometimes speak of explaining laws. How can that be? Many laws are known as causal laws. What does that mean? Are there noncausal laws? If so, what are they?
(6) Theories, models, and scientific systems. We also hear it said that science explains by means of theories. What are theories? How are they related to laws? How do they function in explanation? What is meant by a "model" in science? What role do models play in science?
(7) Determinism. Discussions of lawfulness lead to the question of determinism. What is meant by determinism in science? Is the deterministic thesis (if it is a thesis) true? Or what reason, if any, do we have for thinking it to be true?
(8) Philosophical problems of physical science. The physical sciences have, in recent years, provided a number of philosophical problems. For example, some have held that relativity theory introduces a subjective component into science. Is this true? Others have said that quantum physics denies or refutes determinism. Is this true or false?
(9) Philosophical problems of biology and psychology. First, are these sciences genuinely distinct? If so, why? If not, why not? Further, are these sciences ultimately reducible to physics, or perhaps to physics and chemistry? This gets us into the old "vitalism/mechanism" controversy.
(10) The social sciences. There are some who deny that the social sciences are genuine sciences. Why? Are they right or wrong? Is there any fundamental difference between the natural sciences and the social sciences?
(11) History. Is history a science? We often speak of historical laws. Are there really any such laws? Or are there only general trends? Or neither?
(12) Reduction and the unity of science. We have already briefly referred to this issue. The question here is whether it is possible to reduce one science to another and whether all of the sciences are ultimately reducible to a single science or a combination of fundamental sciences (such as physics and chemistry).
(13) Extensions of science. Sometimes scientists turn into metaphysicians. They make "radical" statements about the universe - e.g., about the ultimate heat-death, or that it is imbued with moral progress. Is there any validity in these claims?
(14) Science and values. Does science have anything to say with regard to values? Or is it value-neutral?
(15) Science and religion. Do the findings and conclusions of science have any implications for traditional religious or theological commitments? If so, what are they?
Science and culture. Both religion and the domain of values may be considered to be parts or aspects of culture. But surely the term culture also refers to other activities and practices. What is the relationship of science to these?

The limits of science. Are there limits of science? If so, what are they? By what criteria, if any, can we establish that such limits are genuine?

(Klemke, Hollinger and Kline, 1980, p.4-5).

My deliberations resulted in an FI comprised of 15 questions amongst which my original 3 questions of FIv2 were retained, albeit, the expression 'scientific-progress' now amended to 'an advancement in scientific knowledge' in an attempt to focus attention upon epistemic dimensions of scientific-progress. The key words associated with these questions (which were asked in a similar manner to those in FIv1,2) may be grouped into the following "domains" which, in some applications or within some persons, may overlap or even co-extend:

<table>
<thead>
<tr>
<th>'commonsense'</th>
<th>'hypothesis'</th>
<th>'scientific method'</th>
<th>'unscientific'</th>
</tr>
</thead>
<tbody>
<tr>
<td>'to know'</td>
<td>'theory'</td>
<td>'expériment'</td>
<td>'an advancement</td>
</tr>
<tr>
<td>'fact'</td>
<td>'law'</td>
<td>'control experiment'</td>
<td>in scientific</td>
</tr>
<tr>
<td>'truth'</td>
<td></td>
<td>'experimental test'</td>
<td>knowledge'</td>
</tr>
<tr>
<td>'proof'</td>
<td></td>
<td></td>
<td>'a knowledge of</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>history of science'</td>
</tr>
</tbody>
</table>

A. Personal Epistemologies  
B. Scientific Meta-Theory  
C. Scientific Meta-Method  
D. Scientific Epistemology

Table 8.1. FIv3. '15 Questions' (Key Words)

In FIv3 I would additionally resort to classification of occupations, responding-with-pictures technique and/or elicited metaphors where I thought that this might help.
8.3. A Focussed Interview for Exploring Educators' Personal
Epistemologies of Science

My sample of educators was comprised of 6 teachers - 'Fl(T)', and 2
lecturers - 'Fl(L)'. Included amongst them were those educators whose
classes or tutorial groups I had observed as part of my main study
(Fl(T)2, Fl(L)1, 2 - see section 8.5.2., below; also see Ch.6 for an
account of the class/tutorial observations).

I "pilot ed", or developed, this FI only in the sense that I had had
many informal interviews (see section 8.4., below) with educators by the
time of conducting these interviews and some of the questions which I
tended to ask were derivative of my FI for educands.

Amongst things which I wished to explore with educators was whether they
perceived a difference between their 'personal epistemology of
science' and what I shall call their 'professional epistemology of
science'. In this study I have tended to assume that the latter
coincides with, or includes, what I shall term the educator's
'pedagogic epistemology of science' although I concede that they
need not do so in all cases. Where such a distinction could be made
(for example, with some lecturers), however, my interest for purposes of
this thesis would only be with their pedagogic epistemology of science.

With respect to educators' pedagogic epistemology of science, then, I
was keen to explore their perception of curricular constraints and
possibilities for the teaching of science in general, and philosophy of
Finally, I was interested to look for any general similarities and contrasts between teachers' and lecturers' personal and professional epistemologies of science and which might be argued to be attributable to the lecturers' current occupational status of 'scientist'.

I would start interviews with a request for the educator to provide me with a synopsis of their personal academic background. In the case of teachers, my principal research interest here was to find out if they had ever been employed as a scientist. Following this I would proceed, where appropriate, with a series of questions the main one of which was

- "Can you tell me what, in broad terms, you are trying to achieve through your science teaching?"

If the ensuing conversation did not cover the issues in which I was interested I would pursue my enquiry with questions such as

- "Do you try to engender understanding about scientific method through your teaching?"

- "What is your personal meaning of the expression 'an advancement in scientific knowledge'?"

- "Do you think that a knowledge of history of science is important for the scientist? (vs. the student-scientist?)"

- "Are there any teaching techniques that you feel are especially enlightened or useful in the science classroom?"

With those interviewees whose classes or tutorial groups I had observed as part of my main study, I would usually ask some pre-prepared questions which were specific to them.
I deliberately timed my FIs with each population of educators to take
place only after I had implemented all other investigative methods with
the educators' associated populations of educands and had also completed
at least a preliminary interpretation of the data created therefrom.
This was in order for me to achieve the richest possible contextual
knowledge informing my conduct and interpretation of these interviews
since it was principally in light of these that I anticipated making any
recommendations for improvements in the actual practice of teaching, as
opposed to more-or-less academic suggestions for "future research".

8.4 Informal Interviews

In between lesson or tutorial observations and FIs there were many
opportunities for me to talk informally with educators and educands.
Similarly, there were many occasions when I overheard conversations held
by others or individual comments made. Again, at the end of a lesson or
tutorial observation or an interview educators and educands would often
add or volunteer comments after I had switched off the audio-recorder
(sometimes because I had switched it off: see discussion, section 8.3.,
below). When such comments or dialogue seemed to me to be relevant to
my research I noted down the actual words spoken as accurately as I was
able to do. I did this at the earliest opportunity after the event.
To assist me in the task I often spoke my recollections into a miniature
audio-recorder which I carried around with me at all times for this
purpose.
8.5 Interviewee Main Sample

8.5.1 Source of Interviewees

I have presented a description of the educational institutions, classes and tutorial groups from which I selected interviewees, together with details of my entrees for them, in Ch.6.

8.5.2 Details of Interviewees

A total of 30 interviews comprised my main sample. These interviews were conducted between two main interviewee populations, viz. educands (pupils and students) and educators (teachers and lecturers):

<table>
<thead>
<tr>
<th>Educands</th>
<th>Educators</th>
</tr>
</thead>
<tbody>
<tr>
<td>P, n = 14</td>
<td>T, n = 6</td>
</tr>
<tr>
<td>St, n = 8</td>
<td>L, n = 2</td>
</tr>
</tbody>
</table>

Secondary Educ. Level.

Tertiary Educ. Level.

Where P = Pupils, St = Students,
T = Teachers, L = Lecturers.

Table 8.2. Showing Composition of Interviewee Main Sample

A more detailed presentation is given in Table 8.4 overleaf.

I selected P' educands on the basis of responses to WE which I found interesting. Thus, for example, I selected (subject to the willingness of the individuals concerned) 3 pupils from my 3rd category of response to WE Q8, i.e. 'Sophisticated?', in order to explore the possibility
Table 8.3 Detailed composition of interviewee main sample
that some pupils who had categorized in this way might more authentically have been included in either my 'Sophisticated' or 'Naive' category. In the case of one of these pupils - Paul (F1(P')5) - I judged this, indeed, to be the case and so "upgraded" my categorization of his response from 'Sophisticated?' to 'Sophisticated' - thereby indicating not only a limitation upon the authenticity of my categorization of responses to this question but also the value of 'methodological triangulation' as a methodological check.

I intended P" and St populations of educands to be co-extensive with my main study class and tutorial observations, respectively. With the exception of one pupil (from T.O. Group 2), who was ill throughout the period during which I was conveniently able to conduct interviews, this was, in fact, the case.

Finally, I similarly chose to interview all educators whose lessons or tutorials I had observed. In addition, however, I interviewed an opportunistically selected sample of teachers.

8.6. Method of Conducting Focussed Interviews

When eliciting consent from educands and educators to interview them I characterised the purpose of my research as "an investigation of pupils' (students') and teachers' (lecturers') personal meanings of some words commonly used in and about science". I also gave educands an assurance that there were "no right or wrong answers".
With the prior consent of interviewees I audio-recorded all interviews which I conducted. Interviews were private and anonymous in so far as no one else from the relevant educational institution was present at the time or ever heard the recording. I also promised each interviewee that I would strive to preserve their anonymity in every other way both within and without their institution. To render this promise palpable to interviewees, I told them that at the conclusion of the recorded part of the interview they would have the option of retaining the recording if for any reason they so wished - an offer which I repeated at the conclusion of the actual interview.

Within schools, I usually interviewed both pupils and teachers in an empty laboratory and preparation room previously designated for this purpose. Within Universities, I conducted my pilot-interviews within my student accommodation (a study-bedroom). For my Main-Study, the University library kindly made available, at my request, a room (with a key!) specifically for my use throughout the interview period of one week. I interviewed lecturers in their personal laboratory-office.

I began all FIs with a request for a synopsis of the educand's or educator's personal occupational and academic background. I also committed these details to an 'interview proforma', appropriate to each interviewee population, i.e. P, St., T, or L.

My methodological concern during this part of the interview was to help the interviewee relax and to speak whilst being audio-recorded.
With all educands, I began the FI proper by asking them for their personal meaning of 'unscientific'. Thereafter, however, I did not rigidly adhere to either a set sequence in which to ask the remaining, pre-planned, questions or selection from such questions, over and above my '3 Questions' of FIv2 which were common to all my Main Study FIs. In my '15 Questions' interview (i.e. FIv3), for example, I did not always elicit meanings for every expression - my list was only intended as a guide. During FIv3 dialogues, I used a series of 12.5 x 7.5cm cards upon each of which were the key words (as per Table 8.1., above) corresponding to one of the 15 questions. I "embedded" each key-word(s) within a question in a manner similar to FIv2. I hoped that these cards would assist in the elicitation process by helping the educands to focus their attention upon the expression under consideration and, likewise, by their manual sorting and clustering of cards where the generation of distinctions between personal meanings of key words seemed useful. An example of these cards is reproduced in Figure 8.4, below:

![scientific-method](image)

Figure 8.4. Showing FIv3 Elicitation Card (Actual Size).
In all Main Stud FIs with educands, I also had elicitation proformae for "scientific occupation" (Cf. Ch.7, section 7.3.1.2.1: discussion re. 'Elicitation of Elements') and for "scientific activity" (Cf. Ch.7, section 7.42.2: discussion re. 'Responding-With-Pictures Technique') in case I might wish to incorporate these approaches. With P' educands only I had their school exercise book, any documentary teaching materials used in lessons in which I had observed them, and their WE script at hand to assist with my questions. This was principally to minimize the purely memorial demands of my questions.

At the end of each FI, by which I mean when I had ceased to audio-record it, I invited each interviewee to comment upon their experience of the interview. This was to find out whether they had views which they had not felt able or willing to commit to the audio-recording. I also repeated as sensitively as possible my invitation to keep the recording if they so wished - an offer never taken up. Finally, I asked interviewees to refrain from discussing the content of the interview with any potential interviewee until after the collective interview period.

Interviews lasted from 20 minutes to nearly 2 hours: with P' educands the mean duration was around 20-25 minutes and with P" and St. educands around 40-50 minutes; with teachers around 30-40 minutes and with the two lecturers, one lasted 45 minutes, the other 2 hours.
Clearly no method of "recording an interview" - here understood only in the very general sense of making some kind of representation or account of it - can recapture the total experience of the interview. From the moment a "record" is made, "data" is inevitably "degraded" to some degree. Equally from that moment, however, an interpretation has begun even if this is "only" in the sense of endorsing, tacitly or explicitly, the constraints which are necessarily imposed by the method of recording upon the possible class(es) of "data" to be sought. Audio-recording alone, for example, imposes the interpretative judgement that spoken words (language) are more important than, e.g., gestures (movement). If the reverse judgement is made, or a judgement which attributes more equal importance to the two, then some form of visual, or audio-visual, method of recording might respectively be more suitable. Moreover, the choice of method of recording may impose specific constraints upon the responses of the collaboratee and which may only be discerned when a different method is adopted. The principal purpose, which I have already alluded to, of appending IIs to FIs was to explore the well recognised possibility that some persons do not feel that they can speak freely when being audio-recorded however sensitively the interview may have been conducted. Thus the activities of recording and interpreting ("analysis") cannot ultimately or entirely be separated, though they may continue usefully to be distinguished in many contexts.

With these recently made points in mind, I can state that, at different stages of the whole process, I carried out elements of my methods of transcription and of interpretation either concurrently or cyclicly
("re-iteratively"). This was partly due to my being personally responsible for executing all aspects of these tasks, as I hope shall become clear in my account which follows.

As I mentioned earlier, at the end of the FI proper I engaged in an II with the interviewee. Following this, i.e. immediately after the interviewee had departed, I noted pertinent details of the II in an 'Interview Field Notebook'. Also then, or on the evening of the interview at the latest, I began a first, or "preliminary", stage of interpretation. This consisted of my implementing, in a very abbreviated way and with the aid of memory and/or a "scan" listening to the audio-recording, the 'strategy of recording' due to Schatzman and Strauss (1973) and which I had also used in my Class Observations (see Ch.6 for my detailed description of this method). Thus I made 'Observation Notes' (ONs) relating principally to the interviewee's emotional or gestural state at certain points of the interview (FI and/or II) which I felt might be important in my detailed interpretation later and which I might otherwise forget, e.g. 'John appeared very nervous when I asked him to distinguish 'fact' from 'truth'.' I also noted down the gist of any responses which I found interesting in light of my then reasonably undifferentiated and unelaborated complex of psychological, epistemological and pedagogical knowledge and commitments. I often accompanied these tentative interpretations or 'Theoretical Notes' (TNs), and which I would sometimes also propose as a possible 'category of response' ('(c?)'), inferred from my prior interpretative experience of pilot-FIs and WE, with a 'Methodological Note' (MN) to repeat the question or other tactic which elicited the view if I judged it to have been efficacious. In my field note-
situation - naive empiricist? Ask next C. for origin of scientific problems. The purpose of these notes was simply to initiate, right from the start, and in the context of my interviewing, what might be called a "formative" or "cyclical", rather than a deductive or analytical, method of interpretation. Figure 8.5*, below, shows a photo-reduction of my field interview notes which I made for an undergraduate student, named Andrew (FI (St) 3), (cf. transcript Appendix 17)

This first, preliminary, stage of interpretation partially pre-empted and short-circuited my second stage. The latter consisted of a full transcription of the FI, together with a more thorough and explicit (re)application of Schatzman and Strauss' (1973) 'strategy for recording'.
To elaborate upon my second stage of interpretation, I used a transcription method or format which was loosely based upon one proposed by Watts, Harrison and Gilbert (1982). This was partially because I shared with these authors the fundamental judgement that whilst the content of speech should be recorded as accurately as possible, the representation of intonation, pitch range etc was thought to be relatively unimportant for our research interests (alternative conceptions in and of science, respectively).

I shall follow Watts et al. by discussing the transcription method in terms of style and notation, although I have altered many details of their original proposal.

(a) Style. Capital letters, quotation marks, full stops, commas, semi-colons and the like are features of written English which correspond only loosely to those of spoken English. Accordingly, these are kept to a minimum to avoid pre-emptive interpretation.

(b) Format and Notation. To facilitate tracing any part of the interview when editing, the transcription is laid out with a first column on the left-hand side for recording the number of tape revolutions from the "rev-counter", e.g. 001. Side two of a tape is indicated thus, e.g. 2/001. The first side of a second tape is shown by, e.g. 3/001, the second side by, e.g. 4/001, and so on. In the second column on the left hand side are entered the initial of the speaker - 'I' for the interviewer and the initial of either the first name for educands or the second name plus title (e.g. Mr, Mrs, Ms, Dr) for educators. In typed transcripts, the speech fills the rest of the page but in the original manuscript (from which they are copy-typed)
there is a 3 1/2cm margin on the right-hand side in which ONs, TNs and MNs may be recorded. The following are details of notation.

(1) -Use of capital letters is generally restricted to an unambiguously begun new utterance, for a similarly begun new sentence within an utterance and for proper names. Question and exclamation marks are used in the normal way.

(2) -Pauses are shown by a full stop or series of full stops, the number providing a rough indication of the duration of the pause. Longer pauses are shown thus, e.g. (pause), (long pause). Commas are used only as apostrophies; colons and semi-colons are dispensed with.

(3) -Interruptions are shown by ending the previous speaker's utterance with two dashes, enclosing the interrupter's utterance with two dashes at either end, and prefacing the original speaker's resumed utterance with two dashes.

e.g. A. 001 the quick brown......
B. 001 ....the quick uhuh...
A. 002 ....fox jumps over the lazy dog.

(4) -Transcriber's doubt is shown by an asterisk within a bracket with as much of the sound included as possible, e.g. (*), (*the quick).

(5) -Minor interpretations and observations are included within brackets where appropriate, e.g. (agreeing), (laughs), (looks distressed), (coughs) (alarm bell sounds).
(6) Repetitions and minor errors of speech which are judged to be non-
substantive are omitted without indication. Ums and ers are
included where appropriate.

(7) Within excerpts from transcripts, used for purposes of commentary
and interpretation, omitted, locally non-substantive, speech is
indicated by empty brackets inserted within an utterance(s), e.g.
the quick brown fox ( ) jumps over the lazy dog. A short
"bridging passage" or word may be included within the brackets
where this is judged to assist the flow of words and not to alter
the sense originally intended by the speaker, e.g. the quick brown
fox jumps (over) the lazy dog.

I made free reference to my Field Interview Notebook throughout my
interpretative annotation of the transcript which I interpolated with my
transcription.

A page excerpted from my "second stage" interpretative transcription,
again of the FI with Andrew (FI(St)3) shows some of these recently
discussed features of the method:
I may now return emphasis to a consideration of my method of interpretation.

I began with a single audio-recording of an FI and at appropriate points during (or after) transcription would attempt to decide what that particular interviewee was trying to say about the 'meta-concept' in...
question. Such decisions would represent "interpretative hypotheses". These I would note down as TNs and would use tentatively in my approach to interpreting the corresponding parts of the next FI. As a result of this subsequent application of interpretative hypotheses, I would judge some to have utility in understanding and explaining a response(s) from the later transcript, suggesting a commonality between responses. Where I judged this to be the case I would propose a tentative explanatory title for a 'category of response'. I was assisted in this process by the patterns of responses which I had already discerned and recorded in my Field Interview Notebook, which, in turn, was informed by my prior interpretative experience of WG and pilot-interviews. Equally some interpretative hypotheses from a later transcript helped me to (re)interpret responses from an earlier transcript. So my strategy of interpretation duly became cyclical. I additionally used MNs to alert my attention to, and to direct, my inter-transcript comparisons and re-interpretations. Throughout this process I repeatedly "collapsed" a number of tentative categories of response under one of what became my "final" set of categories (see section 8.8., below). Of course, some interpretative hypotheses remained unique and specific to a respondent.
8.8 Categories of Response.

8.8.1 Introduction

In this section of the chapter I shall present a summary of the categories of response for each of the two populations (ie, educands and educators). With each category I shall present the substantive meta-theory where it is novel or seems necessary to do so, otherwise I shall refer the reader to where it was introduced earlier in the thesis.

For an in-depth interview case-study of a pupil, see Chapter 9. Full transcripts of an interview with a student, a teacher and a lecturer are included in Appendices 17, 18 and 19, respectively.

8.8.2 Educands

Category 1

Questions re Epistemology of Science Novel and Difficult to Answer

(a) Tautologous
(b) Related to School Science
(c) No Personal Scientific Activities Outside School
(d) Inexplicable Explicative Device
(e) Rhetoric of Conclusions
(f) 3rd Personification

Category 2

"Weak" Senses of 'Science', 'Scientific'

- in McClelland's (1984) sense, see Chapter 5.
Complementing McClelland's distinction between 'strong' and 'weak' senses of 'science' and 'scientific' (Category 1, above), I propose a distinction between 'strong' and 'weak' senses of 'method' and methodology.

(a) 'strong' senses of 'method', methodology:

These are where an individual articulates, or where the description of an experiment makes more-or-less explicit mention of, the design of an experiment, i.e., the mechanism for theory-choice used. For example, use of a control. It should be noted that whilst the grounds for theory-choice are usually empirical, they are neither always nor necessarily so.

(b) 'weak' senses of 'method', 'methodology':

These are where reference is made in only description terms to the equipment chosen and its method of operation, or to materials chosen and their preparation. These 'weak' aspects of method make no direct reference to a mechanism for making a knowledge claim (theory-choice). They cannot, however, be entirely separated from it: my distinction between 'strong' and 'weak' senses is one of degree, not of kind. For example, the selection of porcelain as the material bound up with ideas from within a broader theoretical context concerning chemical structure and bonding. References solely to the impedimentation and sub-procedures associated with the conduct of scientific investigation reflect uses of the term 'scientific-method' in only my 'weak' sense. Such references coincide with 'weak' senses of 'science', 'scientific'; however, 'strong' senses of 'science', 'scientific', need not necessarily coincide with 'strong' senses of 'method', 'methodology'.
Category 4

**Empiricist Epistemology of Science**

- in Hesse's (1980) sense, see Chapter 4.

(a) Empirical Verification.

(b) Inductive Reasoning.

(c) Brute Data.

(d) "Accidental" Scientific Observation.

(e) Mysterious Origin of Problem Situation.

(f) Importance of Priority in Scientific Discovery.

(g) Proof Exclusively Empirical.

(h) 'Operationalism'.

(i) Exclusion of 'Context of Discovery'.

(j) Growth of Scientific Knowledge by 'Accumulative Fragmentation'.

(k) Growth of Scientific Knowledge by 'Theory Reduction'.

(l) Growth of Scientific Knowledge by 'Convergent Epistemological Realism'.

(m) Value Neutrality of Scientific Knowledge and Conduct of Science.

(n) Empiricist Meanings for Some Key Meta-Theoretical Scientific Terms (ie, Hypotheses, Theory, Law, Experiment).

Category 5

**Post-Empiricist Epistemology of Science**

- in Hesse's (1980) sense, see Chapter 4.

(a) Cult of Personality

(b) Post-Empiricist Meanings of Some Key Meta-Theoretical Terms

(c) Ethical Criterion for 'Scientific Progress' ('An advancement in scientific knowledge')

(d) Epistemological Pluralism

- 8.48 -
'Self-Organised Learning' in Science Education Expressed as Desired Alternative to Current Practice

1. Real personal learning depends upon an ability to use oneself as a test-bed for personal validity and viability. The construction of internal referents is primary. External criteria, normative standards, and assessment by others are secondary. Thus the quality of learning becomes defined within the person's own evaluative systems rather than judged against the criteria arrived at by 'experts'.

2. The dynamics of self-organised learning depends upon an ability to monitor the construction and reconstruction of personal meaning over time. The development, expansion, modification and refinement of our personal models of the world can thus be systematically regulated and appreciated. Inadequate monitoring leads to inappropriate models and this can be viewed as disruptions to personal growth.

3. Shared meaning as against public knowledge must be truly negotiated. Individuals, pairs, groups, and institutions can each become conversational entities capable of adaptive, organised learning. Such conversational networks construct their own viability and validity and thus exhibit a capacity for creative and flexible growth.'

(Thomas and Harri-Augstein, 1985, pp xxix - xxx).

8.8.3 Educators

Category 1. Personal vs Professional Epistemology of Science

Category 2. Empiricist Epistemology of Science

Category 3. Post-Empiricist Epistemology of Science

Category 4. Epistemology Not Necessary for Learning Science

Category 5. History of Science Not Necessary for Learning Science
Chapter 9: Case Study

9.1 Introduction: Purpose and Method of Presentation of Case Study

In this chapter I present a case study of a focussed interview which I conducted with a secondary school pupil named Michael (FI(P')3).

My purpose for this case study is to provide an enriched representation of the complex interactive processes involved in eliciting and exploring in interview students' personal epistemologies of science to complement the summarised account of my previous chapter. I intend this case study also to serve as an "argument-in-application" for the pluralistic methodology which I have advocated and used throughout this thesis.

The method I have used in presenting this case study consists of a series of excerpts from the interview transcripts I have selected interpolated with my own interpretative commentary. In accordance with my research philosophy I have also included material from Written Exercises, Classroom Observations, and Informal Interviews etc. where I have judged this to be useful.

The excerpts which I present are never less than an 'exchange', viz. a question asked by one speaker followed by a response from the other speaker. This I consider to be the minimum authentic unit commensurate with my purpose for this case-study, indeed, I have more usually excerpted 'episodes', viz. a series of exchanges bound by a common theme.
I present exchanges and episodes in the order in which they actually occurred in the interview. Where I present excerpted 'utterances' viz. speech from a single speaker within my commentary, however, I sometimes alter this where I judge this to be helpful. For similar reasons I sometimes alter within excerpted utterances the original emphasis placed upon the word(s). I append tape revolution counter numbers to utterances to facilitate reference to (and only to) the fuller context of exchanges and episodes already excerpted within the case-study.

I have retained the transcription notation described in the previous chapter. The excerpted dialogue amounts to approximately 80% of the full transcript.

9.2 Case Study: a Focussed Interview with Michael (FI(P')3)

At the time of the interview Michael was a 13 year old pupil who was "double-entered" for both CSE IS and GCE 'O' level SCISP examinations. I agreed with his teacher, Ms. S., who described him as "one of the brightest" in her class. I also judged Michael to possess quite a sophisticated sense of humour and an outgoing personality. Although so young, I thought that the combination of these qualities rendered Michael's career ambition to become either a 'politician' or a 'medical doctor' not unrealistic.

I began this interview as with all P' educands, by asking Michael some questions to do with the experimental design used in the lesson which I had pilot-observed, and in which he had taken part as a member of the
class, viz. 'The Action of Saliva on Starch' — see Appendix 4 for lesson synopsis and teaching materials. That lesson was particularly interesting to me because it employed a model within a control-experimental design. To help reduce the demand on memory in my initial questions, which I intended to elicit Michael's personal understandings of both these things, I referred to a labelled diagram of the apparatus used in the lesson:

![Diagram](image)

**Figure 9.1 Diagram showing Apparatus Embodying Model and Control Aspects to the Experimental Design used in Lesson Observed**

Part of our first exchange was as follows:

**I 004** why do you think that you did the various things that you did within that experiment to somehow answer this question. how does food get into the body?

**M 017** well we've done it because the food goes into the oesophagus and goes down to the intestines and we wondered how it was absorbed into the blood stream as to using starch. starch on its own would be too large. the molecules would get through the Visking tubing which is semi-permeable so that wouldn't do it on its own so it had to be broken down somehow so it was through the saliva that it got actually broken down and got through into the bloodstream.
I considered this answer to demonstrate a perfectly adequate, albeit, very basic, exposition in terms of Scientists' Science. I noticed in particular that Michael had grasped the central problem of the experiment, namely that starch molecules would be too large to get through the Visking tubing.

I next tried to focus upon Michael's understanding of the modelled components within the apparatus, e.g.

I 024 you're saying this is how it got through into the bloodstream. just one thing Mike. I didn't notice any blood in the experiment!

M 025 no we used water instead. we didn't want to knife people and get their blood. we used water!

and

I 034 I see. um ... go back to the title again. how does food get into the body? you mentioned Visking tubing

M 036 yes. semi-permeable sort of thing ... its in a way like the intestine ... how it works ... things can be absorbed all the way through it ... things can be absorbed through it in say water's like blood isn't it.

I 039 so Visking you mean is like the gut wall?

M 039 yeh

I 040 do you know how its like it. you say its semi-permeable ... um what does semi-permeable mean?

M 043 its a bit like a leaking skin ... things can get out of it ... control things and control things can be moved out of it like it brings oxygen in from the wall then it expels carbon dioxide out ... its like that thing from ...

I 046 I see. so you're using the Visking tubing to what?

M 048 to show that using it sort of as the gut or the intestines show that things can be passed out.
It is not unknown for pupils not to realise that, in the experiment, water is being used to model blood (as opposed to, say, urine) and that Visking tubing is, in fact, a model (as opposed to a section from real gut). Michael's humorous retort (025) and later comments, however, convinced me that he was aware of both modelled components and at least something of the value, if not the limitations, of using them. I did not pursue his apparent comparison (043) of the function of the gut wall with gaseous exchange in the lungs since I was keen to move on and explore his personal perceptions of the control aspect to the experiment, the more so since he had introduced and used the term 'control' in what I judged to be a conventional sense within that utterance.

Michael, however, soon convinced me that he had indeed grasped the basic significance of the experimental design, e.g.

I 060 why did you have those three test tubes do you think?

M 062 to show that it was no fluke .. that starch and saliva .. it was a mixture that made it get through the Visking tubing . not just that saliva goes through it .. but starch can't go through it .

I 065 you needed all three to show that it wasn't a fluke?

M 065 yes .

I 065 O.K. . um to show that what couldn't get through it?

M 067 that er starch on its own . or saliva on its own . couldn't get through . it had to be a mixture of both .

I 067 I see . I see . what would have happened if you hadn't had for example er if you hadn't had this middle test tube the one with just starch in it . what would have been the problem then?

M 070 well . we wouldn't have been able to do the experiment properly .

I 071 why?
M 071 well. we wouldn't have known whether it was just the starch on its own that got through, or the saliva breaking it down into particles. It's just that the saliva breaks down the starch, but we didn't know that it could have been the starch just going through on its own so we needed the middle test tube.

This was also the inference I drew from my interview with his classmate, Nigel (FI(P)4) and from my impression of the general confidence and competence with which members of the class had actually undertaken the experiment. I found their apparently ready understanding of this experimental design particularly interesting since, in an II after the lesson in question, the teacher had declared that she and that class had "never done anything like this experiment before" and, further, that neither she nor anyone else had taught them the principle of control.

I speculate that the "embedded" manner in which I discussed this aspect of the experiment may have rendered my questions more meaningful rather in the manner that problems in logical or deductive inference have been shown to be more tractable when embedded in familiar contexts (see, e.g. Wason and Johnson-Laird, 1972). I also suspect that the use of controls in many "experiments" intrinsic to advertisements, especially those on television, may facilitate learning of this experimental design — an interesting possibility for future research.

I devoted the rest of this interview to eliciting and exploring Michael's personal epistemology of science. From this I concluded that overall he endorsed a positivistic brand of empiricism as I shall now try to show.

I had interpreted Michael's responses to WE, section B, viz. RWP re "a scientific activity outside school", to reflect a frustrated desire, by
now familiar to me from other WE scripts and FIs, to include an "off-the-peg" school science activity and as such, represented an uneasy compromise between his criterion for scientificness and the demands of my question:

![Diagram showing Michael's response]

**Figure 9.2.** Showing Photo-reduction of Michael's Responses to WE, Section B

His written justification of the activity's scientific status similarly followed a familiar route of tautology followed by an appeal to 'weak' senses of science. These things not withstanding, his responses exhibited some novel twists which led me to request the interview with him: while the 'scientific activity' was carried out in school, as with so many examples chosen by pupils, Michael's took place in an English, as opposed
to a science, lesson; the activity was a CND project which promised to raise moral and/or political issues to do with the conduct of science; he had read about the "scientific" phenomena he referred to (albeit in this instance, necessarily so).

I began the epistemological part of the interview with a request for Michael to elaborate upon his responses to this section of WE:

I 076 ( ) may I ask you a question about four, which if you remember is where I asked you to think if something happened in your life outside school and in which you were involved in scientific activity? That was the question and I asked you to draw a sketch and write a little description underneath. now... you've put here seeing how an A-bomb works in my English CND project.

M 088 yeah. well before I had to explain what CND was all about. I had to see how the atom bomb worked. the uranium protons and electrons went together and how neutrons bombarded the other neutrons to make it split and that's the only thing I could really remember that was scientific. well it wasn't that scientific really.

I 092 why wasn't it all that scientific?

M 093 because it was mostly reading about it. it wasn't actually doing something scientific. like actually building one or something like that.

I 094 [laughs] I see. so a scientific activity er would it be right to say that a scientific activity is where you actually do something scientific. rather than just read about it?

M 096 yeh.. because sitting down in a chair picking up a book isn't really scientific? so its more scientific if you're doing whatever you're doing - what is scientific

Michael's qualification to the scientificness of his 'scientific activity', immediately following his approximation to a scientist's science explanation of the working of an A-bomb (088), supported my initial conjecture that his selection of an activity which could meet my otherwise unelaborated dual criteria of 'scientific' and 'outside school'
had been something of a struggle and a compromise to him. Most interesting to me, throughout this last series of exchanges, was Michael's emphasis on the practical in the conduct of science. This provided the first indication of Michael's commitment to some form of empiricist philosophy of science. I immediately sought a way to test this interpretation. Luckily, discussion of the development of the A-bomb (and nuclear weapons in general) is ideally suited for this purpose since it provides one of the clearest demonstrations of both massive "pure", theoretical, science on the one hand, and unequivocable and indispensable empirical test on the other hand. My entree to this subject was facilitated by the then current television series on the life of J.R. Oppenheimer, theoretical physicist and so-called "father of the atomic bomb" - upon enquiring I found that Michael had viewed part of the second episode. I tried to elicit his views upon the role of theory in a manner which related both to the activities of the weapon physicists and to Michael's views on the scientificness of reading:

I 100 what about the original scientists? these were the people who were designing the atom bomb in the '40s and they were called scientists in the programme anyway, they were all physicists. and yet certainly at the stage that that episode that you saw was at . the second one . they weren't actually doing anything in terms of making anything or whatever they were tending to sort of read books do a few calculations . draw a few things on blackboards and so on. how do you think it was that they called themselves scientists in the programme . when you say that scientific activity means that you actually do something?

M 117 well . they were intending to do something but they had to make sure what they were doing at first of all which was really scientific . so they were making calculations .

I found Michael's statement that the scientists 'were intending to do
something, an ingenious way of preserving the practical quality he had earlier identified with scientific activity. I also found it interesting that Michael had, as I interpreted, selected 'making calculations' from amongst other activities I have suggested and proposed this as a means by which the scientists could 'make sure', from the outset, that what they were doing was really scientific. Although (perhaps because) "performing calculations" is a stereotypic scientific activity, I note that calculations are ontologically neutral – performing them is not, of itself, theoretical activity – and as such mathematics is embraced by positivistic brands of empiricist epistemology of science. Our dialogue continued:

I 121 so ... there's two things here, isn't there? if you're intending to do something . then you can still be scientific?

M 124 well I suppose what I was reading in books . it was fairly scientific . but I wasn't actually doing it was I?

I 124 I see . and the fact that you didn't do anything at the end of it . you didn't actually make an atom bomb thank goodness . does that mean that your CND project was unscientific?

M 128 not as such . but part of the project had to be had to be on how it works because you can't criticise something or be in favour of it if you don't know what its about. so you had to write down what it was about and weigh up the two sides of it .

I 131 I see . so it wasn't scientific as such?

M 132 no . it was more social you know . knowing what it was actually about .

I 133 does that mean it was unscientific or does it mean it was less scientific? I'm not quite clear

M 135 it isn't unscientific its less scientific .

In addition to reaffirming 'actually doing it' (124) as his pre-eminent criterion for a scientific activity, Michael introduced the first reference he had made to criticism and judgement which was not directly
tied to practical experience (128). He seemed to me, however, to identify the fruits of 'knowing what it was actually about' with 'more social' activities (132), and to contrast this with scientific activities (cf. 128:132). This was a contrasting aspect of science which Michael was to repeat and to elaborate upon later.

I proceeded by attempting to get Michael to clarify his personal meaning for his phrase 'less scientific' (135), by asking him to give me an example of an activity which would be 'definitely un-scientific' (136). Michael gave several examples only to dilute their un-scientific status by appealing to some aspect of each of them which he could relate to School-Science. This series of exchanges ended with Michael concluding that 'there's nothing really that could be definitely un-scientific' (142).

We continued:

I 146 so everything is scientific more or less. sometimes more or sometimes less. is that what you're saying?

M 147 yes. there are some jobs that are definitely really aren't scientific. you know just a little bit in it. then there are some jobs that are really just scientific. and some that are both like doctors because there's the social side of it. dealing with the patient and the medical side of it.

I 151 I see doctors and nurses are both scientific and unscientific?

M 152 yeah

I 152 because they have this mixture of the social side of it like you had in your CND project as well as the scientific side. so is it fair to say from what you've said so far that the more scientific something is the less of the social aspect?

M 157 yeh. be present in that activity

In this series of exchanges, Michael reintroduced his contrasting aspect
of scientific, viz. 'the social side of it' (147). As his response at (157) makes clear, however, the 'social side' of an activity represents only a difference in degree, not in kind, from what, to mimic Michael's idiom, I shall call the "scientific side". I judge this distinction to again betoken a sympathy to empiricism, viz. the separate between fact and value.

I attempted to corroborate and to explore this possibility by considering how the "social side" of Oppenheimer's later work may have altered his status as a scientist in Michael's opinion:

I 158 tell me something about Oppenheimer. after he had done a lot of work on the atomic bomb. a lot of this reading and all the rest of it. he actually was in charge of making it. and as you know they dropped it in Japan and then later in his life he was still a famous scientist. a famous physicist but then. particularly Oppenheimer became very worried about the social aspects of the atomic bomb. did he become less scientific when he was becoming concerned after the war with the social aspects?

M 168 when he was actually making it. he didn't know how the super powers were going to use it. he was thinking. they knew roughly that it would kill a lot of people - but he wasn't really interested. he just did the sort of historical thing of actually making the bomb that might have been er..

I 172 he was interested in the historical thing. what was the historical thing?

M 174 well like being the first person to build a bomb. or doing it before the Germans did because the race was on at that time. they had to do it. because the Germans were doing it.

In light of my earlier arguments in Chapter 4 concerning personal responsibility and epistemology, together with my support for post-empiricist epistemology, I suggest that this exchange provides a clear demonstration of one of the social dangers of empiricist epistemology, viz. it provides a tacit or explicit rationale for
distancing oneself (and for others) from one's social context, thereby abnegating one's personal responsibility for one's thoughts and actions expressed in a social context. Thus, Michael began by claiming that whilst Oppenheimer was 'actually making' the A-bomb, he didn't know how the super powers were going to use it (168). Almost immediately, however, he seems to me to judge the patent unlikelihood of this and so attempts, rather unconvincingly, to dilute it - 'they knew roughly that it would kill a lot of people' (168). Michael goes on to separate Oppenheimer from his social context - 'but he wasn't really interested' (168) - except insofar that what Oppenheimer was interested in was 'the historical thing (of) being the first person to build a bomb' (168, 174).

Now Michael's interpretation of Oppenheimer's position on these issues in contrast with his "repentance" in the '50s, is closely corroborated by some of this scientist's own words, e.g.:

'A scientist cannot hold back progress because of fears of what the world will do with his discoveries' (Oppenheimer, quoted by Goodchild 1980, p.170).

and later, with reference principally to the development of the hydrogen bomb:

'It is in my judgement in these things that when you see something that is technically sweet you go ahead and do it and argue about what to do about it only after you have had your technical success' (Oppenheimer speaking in 1954, quoted by Easlea, 1983, p.129).

But the image of science that both Michael and Oppenheimer project, in which scientists are portrayed as seeking priority in empirical verification ('technical success') and this is understood to be undertaken and appraised independently of social circumstance, implies that it is
"objective" and in other respects also it is amorphous with the empiricist account. The case of Oppenheimer and the weapons physicists, however, illustrates one of the chief inadequacies of the empiricists' account, viz. it is blind with respect to human justice and morality. This is not however to say that it is neutral - after all, 'they had to do it because the Germans were doing it' (174). The sad fact is that the work on the A-bomb actually intensified after the surrender of Nazi Germany (Easlea 1983, page 83 et seq), and, indeed, it is possible to argue that empiricist thinking facilitates wars, including 'cold' wars, by providing scientists with a rationale for insulating themselves from personal responsibility for their thoughts and actions expressed in a social context, viz. 'objectivity', with the result that their efforts may be appropriated to whatever ends those who are willing to fund them decide.

Now, in practice, empiricist thinking provides only "time out" for investigators to indulge in their otherwise blind pursuit of 'technical success' - few empiricist scientists consistently and enduringly maintain their aloofness from social and humane concerns whatever the effects of their scientific enquiries may have been. I have already alluded to Oppenheimer's famous change of heart and mind concerning the morality of the A-bomb research in which he claimed that 'the physicists have known sin' (Oppenheimer speaking in 1948, quoted by Goodchild, 1980 p.174) - much to the distress and annoyance of many of his colleagues. Be this as it may, from a post-empiricist point of view, this is not enough: the empiricist account culpably puts the cart before the horse both in practice and in principle. Moreover, there have been, and no doubt always shall be, enough outstanding and influential individuals who adhered
rigidly and enduringly to orthodox empiricist doctrine. These points have been argued in detail by Easlea (1983) who identifies empiricist epistemology with a 'compulsive masculinity syndrome' (cf. my discussion in chapter 4). Easlea takes the development of nuclear weapons in general, and that of fusion weapons in particular, as a paradigmatic example of the syndrome in action:

'Even in his published works, Teller [the so-called 'father of the H-bomb'] does not claim that his impassioned advocacy of super weapons was entirely a consequence of the physicist's belief in meeting the 'security' needs of the Western Alliance. Teller's credo, in fact, takes us back to the credo of the founding father of the 'masculine philosophy', Francis Bacon, and to Mary Shelley's profound commentary on that masculine philosophy. We need only remember Francis Bacon's cry 'to the effecting of all things possible', and the compulsive, dangerous search into the unknown conducted by Walton and Frankenstein, best characterised by Walton's confidence to his sister, 'there is something at work in my soul which I do not understand'. For immediately Teller refers in his 1955 apologia to the story of the fusion bomb - the super bomb - as the story of 'the adventure of trying to do what at one time seemed impossible'


My dialogue with Michael continued:

I 177 I see. Can I just ask you about one part of what you've just said which was the bit about being the first to do something?

M 179 People are like that sometimes. They always want to be the first. Second is not good enough. That's the thing that people have sort of grown up with the. Like put into their system like wanting to be the first. Not just him. The people wanted to beat the Germans and be first.

I 185 Is wanting to be the first part of being scientific, do you think?

M 186 No. No. Not all the time no. There are social science like people kidney patients and heart patients and people like that. There's a thing of not just feeding them pills every day. Getting to know them. Doing good more psychologically than medically.
In light of some of Michael's earlier views (e.g. 168), I interpreted his first remark here (179) to indicate a distinction between competitive victory and priority in empirical verification - with the latter being the more important as a criterion for scientific, though the two may coincide in some instances. These features again are commensurate with empiricist epistemology and are noticeably absent in his contrasting illustration of 'social science' (186), which features qualities of cooperation and benevolence.

We continued:

I 191 would you say Mike that on the whole science nowadays is taking more notice of the social side of things or less?

M 192 yes I think it is. you've got scientist organisations take America and Britain against the bomb. they help make it well actually they may help make it then they realised it was a mistake and they are paying to get rid of it

I 196 you say that they realise that it was a mistake, in what way is that?

M 197 well they've got so many weapons and they're going to eventually use them and lots of people are going to get killed and those have got families. cos they got families. other people have got families. they just don't want their families to be killed sort of thing. like that they're more aware of it.

I 202 so are you saying that sometimes scientists um ... might have to as it were give up the pleasure of being the first to do something because they realise that they might by being the first to do something actually ...?

M 207 yeah. actually to do something wrong so I think they have to give up that bit apart from .... there's always wrong things like building bombs and things like that but its better if its things like the heart if they're successful the product comes on the market but so many people can survive and its important of being first not actually another one just of being first .......

I construe certain of Michael's remarks to show a tendency for fatalistic
thinking – a characteristic which is paradoxically combined in equal measure with that of manipulative intervention in empiricism. Thus 'they’ve got so many weapons and they’re going to eventually use them' (197). Similarly, his concluding analysis seems to be a rather bleak mixture of market economics and evolution theory in which choice and morality within scientific enquiry are absent: 'there's always wrong things like building bombs and things like that but its better if its things like the heart .. if they're successful the product comes on the market but so many people can survive' (207). Part of Easlea's commentary on Teller's autobiographical reminiscences of his involvement with the development of nuclear weapons may help further illustrate how these seemingly irreconcilable qualities of fatalism and manipulative intervention can actually be made to complement, indeed to amplify, each other within an empiricist rationalisation of literally any "scientific" enquiry and at any stage:

'Whatever [ ] the many specific political and military reasons why such 'experiments' should continue, there also exists, Teller writes 'this very general reason – the tradition of exploring the unknown' in one of his very latest publications – his 1979 book Energy from Heaven and Earth – Teller states he is sometimes asked whether he is sorry he worked on nuclear weapons. He answers that he is not. He was privileged indeed to have had the opportunity of participating in 'one of the most fantastic adventures that a scientist can have'. Teller's final verdict is that 'the atomic adventure was not the first, nor the last, nor the greatest adventure in history. But it was great and it was inevitable'. Whatever the verdict on the building of the super weapon it was, however, clearly not inevitable. That it was built was very much in part a consequence of the 'magnificent obsession' of Dr. Edward Teller' (Easlea, 1983, page 137: original emphasis).

I next confronted Michael with what I considered to be a discrepancy
between my request for the scientific activity to be outside school and
his selection of one conducted within an English lesson:

I 212  yeah .  yeah may I ask you another question about your example
here Mike .  you've chosen something .  in my original question I
asked if you could draw something that had happened to you in
which you were involved in scientific activity not in school.
now ... you've chosen something that looks rather like the sort
of thing that you would do in school .  um ... why was that?

M 220  (pause) um I'm not really sure if it would be something in school
.  something in school is more like if you're doing the experiment
we've just talked about you know .  things like that .. we don't
read a lot of books in science

I 223  you don't in school?

M 224  I do normally .  but not in science .  we don't read a lot of books
.  you work from books but mostly the experimentations are not
from books ...

I 226  so ..... the fact that you were reading books was one reason why
you thought this was an activity um ... which although it had a
scientific aspect it was something which you could legitimately
put down as something as being outside school.

M 231  yes

Here Michael again affirmed the primacy of practical ability -
'experimentations' (224) - amongst his criteria. It seemed to me that
Michael might, like so many pupils, have chosen the activity partly due to
the fact that it could have been done outside school (unlike, say, a
school experiment involving specialised equipment) and partly because of
the subject matter involving stereotypic phenomena of science, i.e.
implicating a 'weak' sense of science.

I went on to explore this latter possibility:

I 231  uhuh .  I've noticed that you put as your reason as you thought it
was scientific as an atom bomb works scientifically .  I read
about neutrons and protons, critical mass, uranium.

M 234 (laughs) well I don't mean the actual bomb works scientifically. Scientists make the atom bomb work and to normal people its complicated um it does work so in a way its scientific because scientists make it scientific.

I 237 scientists make it scientific?

M 238 ... because scientists . people think ... you know . ordinary people that if a scientist does something like that ... the actual thing is going to work scientifically . as it does it doesn't work physically

I suggest that Michael's responses in this episode fall into a combination of categories: 'tautology', e.g. 'because scientists made it scientific' (234); 'inexplicable explicative device', e.g. 'its complicated' (234).

As with many interviewees, Michael also started to express his views in the third person, e.g. 'to normal people' (234), 'people think ... you know ordinary people' (238). This I took to be a possible ploy to avoid taking personal responsibility for the views he was advancing due to his personal doubt and anticipated criticism from me. In chapter 4 however I have also argued that the objectivism of empiricist epistemology is one of the main attractions for precisely this reason: avoidance of personal responsibility. I thus came to interpret the adoption of third person form by an interviewee as a (very) tentative indication of sympathy to empiricist epistemology. Michael's closing remark - 'the actual thing is going to work scientifically, as it does it doesn't work physically' (238) - would contradict his opening statement that 'I don't mean the actual bomb works scientifically' (234) were it not in "third personified". Here also 'tautology' and 'inexplicable explicative device' categories may be expressed in equal measure: the bomb works 'scientifically' but apparently not 'physically'!

- 9.19 -
I next tried to get Michael to generalise away from his specific example of a scientific activity in case of its possible exotic uniqueness and the already established scientific status of the leading individuals involved:

I 241 um ... yes ... one thing I'm a bit confused about there though Mike is that or its one thing when its a famous scientist working to make something like the atom bomb .. Oppenheimer . what about when someone else, Joe Bloggs is working on some project we won't say what it is, now, how did you decide when he's a scientist or not . because what he makes sound rather like if you were the famous scientist to start with if Joe Bloggs was known to be a famous scientist to start with . whatever he made might be scientific just because he was called a famous scientist in the first place on the other hand if he was completely unknown? .....  

M 252 yeah . thats another thing . a scientist has got an aspect of being a person with not much hair . glasses and a white coat and a board, writing down notes but you can be just as scientific if you've got a name like Fred Smith. You can just go along and propose to do something . its the image that's created by the media .  

I 257 so ... scientists come in all shapes and sizes?  

M 257 yeah  

I 258 men and women, and they don't necessarily have to wear white coats and to be the nutty professor type image that's what you're saying?  

M 260 yeah  

I 260 but . being scientific . what's that you see? ... its a bit more tricky isn't it?  

M 263 .... scientific is more like being clever in a technical sort of way  

I 264 clever in a technical sort of way?  

M 264 people don't understand such things like atoms for example . but scientists have sort of . not say got a gift you know in that way they know a lot about it and they can put it to use . for example .....  

Perhaps in consequence of my poor phrasing of the first question (241) Michael's exposition and criticism of the stereotypic image of the
scientist (252-260) was confined to what I considered to be trivialities (i.e. forms of dress). Michael's answer to my second question (260) once again evoked an empiricist (operationalist?) account of science: 'being clever in a technical sort of way' (263). When I asked him to elaborate (264), however, he again adopted a 'third person' form in combination with an 'inexplicable explicative device': 'people don't understand such things like atoms for example, but scientists have sort of, not say got a gift you know in that way they know a lot about it and they can put it to use' (264).

In another attempt to elicit any role that Michael might perceive theorising to have within the conduct of science without, as it were, imposing or presaging my own requirement for it by the form of my question, I went on to describe the greater relative emphasis placed upon thinking, as compared with experimenting, about the atom in early Greek history:

I 269 the atom's an interesting example . because the idea of the atom goes back a long long way many centuries before the image of the scientist with the bald head and a white lab. coat came about ... now they didn't have books to read about it because they hadn't been written then, but they had to do an awful lot of thinking about atoms and . of course when the original people . the Greeks did it they hadn't got all the technological things to go with their thoughts they hadn't got laboratories or all the rest of it . now they were thinking about the ideas of atoms . small particles and that .... were they scientific do you think?

M 284 yeah . I think if the Greeks wouldn't have been where they were then people wouldn't be as scientific as they are today . but then it might not have been the Greeks it might have been the Egyptians .. but the Greeks were a scientific race of people.

I 289 so they were less scientific because they hadn't got the technical side of it?
I'm not saying that, but the image that's created is that if it's scientific you've got to have computers and everything... and it's not so.

Well this is the image as you've said comes from the media. Perhaps, but what's your view Mike? What do you think? Do you think the Greeks were less scientific or do you think they were just as scientific in their way or? ....

I think they were scientific but not as scientific as the people today because the people today have got computers and everything but they wouldn't have that today if the Greeks didn't exist. Because they were the first ones to bring science into our...

So you go along to some extent with the media image, you think the media has got it right to some extent?

To some extent, yeah

In this episode I found it interesting that although Michael initially criticised my interpretation, which he 'third personified' by identifying it with 'the image that is created' (290) - again, presumably by the media (cf. 252), when I put him on the spot - 'what's your view Mike' (291) he immediately appealed to 'weak' senses of sciences: 'I think [the Greeks] were scientific but not as scientific as the people today because the people today have got computers and everything' (296).

I next turned my attention to Michael's WE1 response to my 'theory-ladenness' question:

If you remember I said "take a piece of paper and pencil carefully observe and then write down what you have observed" and you've written as your answer 'I've observed people looking puzzled at this question'.

Well because everybody was looking puzzled. So I thought I'd better write down. That's an observation. It just come to me like that. Everybody was like that. And I feel that everybody was looking puzzled so I write it down like that.

What do you think they were being puzzled about? What were you puzzled about?
M 318 what the question really meant. you could observe anything. you
could have a baby picking his nose or Barry sucking the end of
his pencil or something like that. people tapping their fingers
on the desk. it could be anything at all. ( ).

I 329 this fact that people are puzzled, were you puzzled?

M 332 first of all. yeah. then I was looking around to see if there
was anything we was meant to notice deliberately. maybe we
were meant to observe people looking puzzled. so I wrote that
down because I thought that's what the question is supposed to do
to make people look puzzled so the answer was being puzzled

Michael's initial response seemed to me to fall into the same category as
that which he gave to WE, i.e. 'naive': 'it just come to me' (314). He
soon showed, however, that he had seen the central flaw to the question -
'you could observe anything ( ) anything at all' (318). Furthermore, the
rationale he gave for his specific choice of 'people looking puzzled'
(332) showed clearly that his own mind had been anything but 'naive',
'tabula rasa', during his participation in WE: 'I was looking around to
see if there was anything we was meant to observe deliberately. maybe
we were meant to observe people looking puzzled, so I wrote that down'
(332). Michael's script answers to other questions in WE1 also showed
that he had ideas and expectations about what he thought I was "really"
(i.e. covertly) trying to find out; e.g. WE Q2 (cf. Appendix 11):

'you have asked us this question to ask us to categorise
everyday jobs. all jobs involve a little science though.
You wanted us to find out what jobs contain science and
non-scientific things. You expect us to find more
non-scientific than any others'

When I told Michael that I hadn't in fact intended them to observe
anything specific and that 'I was just curious to see what you put', he
reiterated his view that the observation(s) 'could be anything'. I
proceeded to try to elicit or achieve an awareness on Michael's part on
the selectivity of observation and attention (cf. stage 2 of my 'didactic strategy' described in Section 7.5):

I 348 do you think you've learned anything from that problem you had with the questions?

M 350 yes its the same because .... things you can observe at one time . things that you can deliberately observe and things that are sort of hidden that you can observe like people looking puzzled . its not something you'd really notice all of the time . when you look around the classroom, those hidden things that you didn't give a thought . luck or coincidence or whatever you make of it or answer

I 357 tell me Mike . you do a lot of experiments don't you in school?

M 357 Yeah

I 358 do you think this question and the problem you had answering it has any relevance to doing experiments?

M 361 yeh . it has because when you observe something it can go a bright orange colour so you could observe that or it might have black specks in it or whatever . you don't actually have to observe you can make experiments to see if its poisonous . its sort of got hidden things . its not just a clear orange liquid you could observe that it might be say a dye or something like that or anything . iodine or ..

I369 I see . so whether you notice thats got an orange colour or whether you test it for something else like whether its poisonous or not depends on what you're trying to look for is that right?

M 373 yeah . yeah but sometimes you don't have to look for anything . its just so obvious that its an orange colour but sometimes you have to search for is it a dye or is it poisonous or is it so and so .

I felt that Michael's remarks in this episode demonstrated that he had, indeed, grasped the notion that all observation and attention is selective. I also, however, judged there to be some indications that the route from here to an acceptance of the theory-ladenness of all observations would not be easy, viz. the examples of 'hidden things' (361) which Michael gave were things which could not, in any straightforward
sense, be considered to be unobservable, e.g. 'poisonous' (361); similarly his statement that 'sometimes you don't have to look for anything, its just so obvious' (373) might be interpreted to be a sympathy for the empiricist notion of 'brute data' due to the implied possibility of genuinely "accidental" observations. I construed Michael's references to 'a bright orange colour' (361) etc to be an allusion to the class experiment which we had discussed earlier. I attempted to confront Michael with a phenomenon which could not be construed within the horizon of phenomenal expectations of the theory underlying Benedict's test for reducing sugars, viz. I chose his suggestion of 'black specks' (361). This he quickly and not unreasonably dealt with in terms of experimental error and he recommended repeating the experiment. Part of his answer, however, cast some light both on the frustrations experienced in the science classroom by Michael qua young pupil scientists and a hint of the sort of science learning activities that he would prefer to engage in:

I 405 so you'd do it under different conditions. um ... in the hope of what?

M 407 in the hope that it would come out how it should come out because scientists and people have been doing it for. I don't know how long. been doing it for a long time. but Benedict's solution has always turned orange. and I don't know why I should be wrong and I don't know what the point is of doing the experiment in the first place but it would be worth doing it again to see if I had found out something more or perhaps the Benedict's solution wasn't Benedict's solution.

This I took to be a tentative advocacy of Self Organised Learning.

I tried to render less ambiguous the epistemological issue that I wished to explore by inviting Michael to consider the blue colour of his jumper as a variable in his repeat of the experiment.
I 448 I was just thinking Mike. You're wearing a blue jumper today. Would you think of maybe changing the colour of the jumper when you repeated the experiment. As one factor to alter?

M 454 No I wouldn't think about that actually.

I 455 Why not?

M 455 Might have black dandruff or something like that well if I was a scientist I'd probably wear some sort of cover like a white coat. Something like that but I wouldn't think of changing it no.

I 463 Well what you've said about having black dandruff is a perfectly good idea. I was thinking from another angle ( ) might just the colour. The Benedict's solution brought within in a foot of blue might have an --

M 469 I don't think it would have any effect. No.

I 470 What makes you think it wouldn't have any effect?

M 472 Well people. Scientists haven't come up and said things like that before. Blue jumpers maybe. But acrylic wool having an effect on Benedict's solution or it could be say my (* Albion neck) could affect your tape recorder something like that ... so strange you don't really think about it.

Whatever the demerits of my question with respect to its didactic purpose,

I was interested that Michael chose to justify his answer by invoking the authority of other persons - 'People, scientists haven't come up and said things like that before' (472) - rather than speculating (as I had hoped he would) upon the likely irrelevancy of the colour of his jumper for this particular chemical reaction, i.e. Benedict's test for reducing sugars, given the accompanying theoretical context in which it is presently understood. This response also diluted slightly his earlier advocacy of Self Organised Learning (407).

We went on:

I 479 So you'd draw a bit on what scientists had found out or not found
out before. Can you now Mike relate back to um ... this question about carefully observing and write down what people observe and the difficulty you had observing it. Now ..... in your repeat of this experiment with the Benedict's solution you'd talked about altering a certain number of factors. All sorts of things. The Benedict's. The test tube. The temperature etc etc but there are certain things that you wouldn't think of altering like. The colour of your clothes just for an example you wouldn't for example take into account what you'd had for breakfast that morning. Um does this tell you anything about the range of things that you're observing?

M 499 its endless. Really. There's just endless of things that you don't think to observe like my (indicates pen kit) being in a plastic bag which you know things you don't really put down. They're not important ( * ) you're sort of noticing things you wanna know ...

I 505 ...... so just observing. Like in the question. It could be endless could it?

M 505 yeah

I 505 you could go on observing more and more things and more and more aspects but when you're doing experiments. you don't write down endless observations do you?

M 512 no. Not really. You need to. Like the test tube was 5 cm. above the flame or in the flame I suppose that would have some bearing but if you said you moved it around that would be an observation that you could write down. Not like Paul's wearing a red jumper and er I was wearing blue socks you know things like that.

I 521 so what does that tell you about when you're doing an experiment. Bearing in mind that just observing. Is endless. Yet in an experiment you don't write down that Paul's picking his nose or whatever?

M 526 I suppose the teacher gives you some idea or your knowledge of what to expect first of all. If the teacher says I'll do this experiment you might expect something to blow up deliberately. Like we did these experiments with sodium.

I 531 ah yes.

M 531 things like that, you know you're a bit wary. ... the teacher might warn you that it's got an explosive tendency something like that so or if it's safe in doing the experiment you would expect the teacher to tell you that there's a chance of it spitting up in your face and you see its like that its part trust part experience so you really know roughly what to expect.
I 540 part trust. part experience?

M 541 say if you were mixing an iodine Benedict's solution, I don't know what would happen and it turned pink and you turned to your teacher is it allright turning pink and she said yes I would have expected it to turn pink so if it ends up turning green then you ask her is it meant to turn green and she says no its meant to turn pink and everybody else's turns pink you've done the experiment wrong and you have to do it again.

I 550 yes. this is where the trust bit comes in is it?

M 550 mm

Despite two promising remarks made by Michael which suggested to me that he had clearly grasped the selectivity of observational attention and recording, i.e. 'you're sort of noticing the things you wanna know' (499) and his view that only observations which 'would have some bearing' (512) need be written down, he again went on to cast himself in a theoretically passive role albeit by a slightly different route, viz. by appeal to the authority of the science teacher: 'I suppose the teacher gives you some idea or your knowledge of what to expect first of all'(526). I consider Michael's views in each of these last two episodes to embody 'weak' senses of scientific and to invoke a 'rhetoric of conclusions'.

Partly in case we became rooted in an over literal account of Michael's experiences of school science, however interesting they might have been in other ways, I attempted to elicit comparisons and contrasts between the activities of scientists and those of pupil scientists:

I 550 tell me Mike. do you regard yourself being really the same as a scientist. when you're in your school laboratory?

M 555 no

I 555 no?
M 557 no. its part science. part. social is not really the word. no
. you know. you are fairly scientific but not all of it like
you've got to consider. do an experiment with things like
equipment. you might have an argument about the result or
something like that you know ...

I 564 who might have an argument about the result?

M 565 me and say the next group. so that would be more as a scientific
argument.

I 568 that would be more like real scientists would it?

M 570 that's a bit more scientific. having an argument. but putting
stuff away that isn't scientific.

Notwithstanding Michael's ambiguous contrasting characterisation of school
science - 'its part science, part, social is not really the word' (557) -
I took his suggestion that having 'an argument about the result' (557)
would be 'a bit more scientific' (570) as compared with 'putting stuff
away [which] isn't scientific' (570), to be an interesting development in
his criterion for science for it may be construed as the first suggestion
of fallibility of scientific knowledge.

By this point in the interview, however, I had already decided to proceed
by asking Michael to elaborate upon his meanings and criterial sympathies
for the terms 'trust' and 'experience' which he had introduced earlier
(531):

I 571 could you tell me if there's any difference between you and as it
were official scientists. in terms of trust and experience you
mentioned a bit earlier?

M 577 well, scientists are a lot older than more. they're more
experienced and probably more trustful because they've worked
with people a long time.

I 581 they're more trustful?
M 581 yeh . to people that work with them . so I can trust my friends
I 583 not to cheat on the experiment?
M 584 yeh . not to throw Bunsen Burners around and ...
I 584 I see .
M 585 or splash sodium around .
I 585 or deliberately put black specks in your Benedicts!
M 585 yeah!

Recognising that my question (571) might have begged this more
conventional meaning of 'trust', I gradually, and with some difficulty,
tried to home in on possible epistemological meanings that Michael
might have for the terms 'trust' and 'experience':

I 585 um . that's one way of using the trust isn't it . but I . maybe I
misunderstood you a bit earlier . I thought that when you were
talking about what you observed in an experiment . I got the
impression that if you were looking for something . like the
possibility of it blowing up you mentioned that it was in a way
trusting . your teacher and also based on your own experience of
other things . they'd blown up accidentally or whatever . a
combination of those two things .. contributes to how you behave
in an experiment . what you look for, what you do and how careful
you are etc etc. now official in inverted commas scientists
where does ... how does the trust part of it come into their
work do you think, or does it not come into their work?

M 609 if they've done an experiment they've done loads of times before'
they sort of trust that it will come up with the same results as
it has loads of times before and they . the people they work with
try and help . they've got to help them . trust them not put
different things in their ..... 

I 615 yes . but you see this is trust in the sense of not being a faker
of results and things . isn't it . that's one side of trust . and
then on the other side you're saying if the real scientists .
official scientists had done the experiment loads of times you
can trust his results . but what about the first time he does
an experiment ...?

M 626 yeh . it was what I was just going on to say . scientists . when
sort of more alchemists .... they didn't know what was going to
happen .. they didn't have no trust .. its just through them
their early scientific experiments that we can roughly know what'll happen when we do our experiments ... its sort of through them we can do science more safely

I 636 I'm sorry Mike I'm not making myself very clear here am I . you see I was just thinking . you know what we were saying about observation being endless and so on and so forth then if we relate that to trust and saying that in a school classroom um ... you've got your own experiences of previous experiments and you've got some trust as well about the official inverted commas scientists um . do they not have trust if they're doing the experiment for the first time ever in the world ..... you know what does trust mean for an official scientist doing an experiment for the first time?

M 658 he hasn't really got no trust has he?

I 659 he hasn't?

M 659 well say if he was mixing sodium with water he might know roughly what sodium was like but of he'd probably say oh the water is turning green but on the other hand you know it pops . but he wouldn't know that so he wouldn't really have no trust . just that he's unlucky to be doing the experiment first .. bit of bad luck .

I 668 so he would have no idea?

M 668 no . he might have a little experience which gives him that trust

I 671 little bit of experience .. what from?

M 671 sodium . just imagine sodium has been around right .. and they know roughly what it is like but they've never tested it with water or in liquid (*) there might have been an explosion in a sodium factory and they could put it down as a faulty thing . he could just have that thing in the back of his mind . it might have been the faulty equipment . it might have been the actual sodium that could be unbalanced but apart from that he hasn't really got no guarantee that is safe

I construe these last two episodes to show the tension that I felt between my joint desire to elicit Michael's views about theory-ladenness on the one hand and, on the other, to teach it . The issue of 'safety' may also have become conflicted with that of empirical prediction in this part of the elicitation process. These things notwithstanding, I interpret Michael's remarks to provide further evidence of his commitment to empirical
inductivism.

Thus, Michael began (609) by distinguishing between two meanings of 'trust'. The first is understood in terms of induction (which I shall call 'inductive trust'): 'if they've done the experiment loads of times before they sort of trust that it will come up with the same result as it has loads of times before'. (609) The second meaning of 'trust' is the more familiar one of personal qualities such as honesty, integrity, etc. (which I shall call 'personal trust') 'the people they work with try and help. they've got to help them. trust them not to put things in their ...' (609).

Next, when I confronted Michael with the instance of a scientist doing an experiment for the 'first time' in an attempt to explore his awareness of the possibility of an empirical prediction (knowledge claim) being justifiable by imagination alone, i.e. 'trust' as an uncorroborated conjecture, as opposed to knowledge by empirical induction, he expressed the classical empiricist assumption of the mind as (initially) a tabula rasa by claiming that 'scientists ( ) didn't know what was going to happen .. they didn't have no trust' (626). This remark should be compared with that he made earlier (168) concerning the alleged ignorance of the Los Alamos scientists of the likelihood of the atomic bomb actually being used ..... Michael goes on merely to assume the pioneering success of scientists - 'its just through them. their early scientific experiments. that we can roughly know what'll happen when we do our experiments .. its sort of through them that we can do science more safely!' in what amounts to a defence of induction-by-induction (Hume!),
viz. the established empirical success of scientists' science, albeit mysteriously achieved in pioneering instances, justifies the school science 'trust' in (and repetition of) it.

I repeated my question concerning the possible role of epistemological 'trust' in conducting an experiment for the first time, this time trying to focus Michael's attention on scientists' science alone (636). Michael's reply that the scientist 'hasn't really got no trust has he?' (658) may again be interpreted to mean a "blank mind" on the part of the scientist, i.e. the scientist had no expectations with respect to the outcome of the experiment.

The example Michael used in his elaboration (659-671) of his answer similarly shows a sympathy for empirical inductivism. My detailed demonstration of this may be assisted by prior reference to Medawar's description of a 'Baconian experiment':

'The word experiment has changed its meaning. [ ] A Baconian 'experiment' had the connotation that still persists in the French experience today: a Baconian experiment is a contrived experience or contrived happening as opposed to a natural experience or a happening, for Bacon rightly supposed that common knowledge was not enough and that there was no relying upon luck of observation - upon 'casual felicity of particular events'. [ ] Rubbing two sticks together to see what happens is an experiment in Bacon's sense; rubbing two sticks together to see if enough heat can be generated by friction to ignite them is an experiment in the modern sense. An experiment of the first kind leaves one with no answer to the question:

"Why on earth are you rubbing those two sticks together?"
(Medawar, 1969, p. 1 : original emphasis).
Thus, Michael's example begins 'if [the scientist] was mixing sodium with water' (659): there is no explanation as to why the scientist might wish to do so and I suspect that had I had the presence of mind to ask Michael about this he would have replied along the lines of "to see what happens".

Because the pioneer scientist cannot already know from experience what happens under these conditions 'he wouldn't really have no trust' (659) and so he is just 'unlucky' to be doing the experiment first .... bit of bad luck' (659) this, again, may be understood to imply a conjectureless mind as may be elucidated by some further comments by Medawar who writes from a constructivist perspective:

'What are we to make of luck in our methodology of science? In the inductive view, luck strikes me as completely inexplicable; it can arise only from the gratuitous obtrusion of something utterly unexpected upon the senses; it is like winning a prize in a lottery in which we did not buy a ticket. To buy a ticket is to define a category of expectations, and then the reason why we win is obvious: we were in luck; for once in a way our hopes were gratified. We have Fontanelle's and Pasteur's word for it but luck makes sense only against a background of prior expectations. Ever since his experiences in the First World War, Alexander Fleming had been deeply concerned by the problem of infected wounds. It was his lifelong ambition to discover a non-toxic anti-bacterial agent, and in penicillin he found a winner — by luck, if you like; but he held a ticket which entitled him to win a prize.'

(Medawar, 1982, p. 93: original emphasis).

It is important to understand that Medawar is debarring only a certain type of luck from the conduct of science: I take the allusion to Pasteur to his famous dictum that 'Chance [or "good luck"] favours only the prepared mind'; conversely, "bad luck" might be said to afflict only the inappropriately prepared mind. Given this caveat, there is still space
for "luck" in constructivist mythology of science and, indeed, the case of Fleming and penicillin provide an excellent example for as Medawar later explains:

'As it happens there was an element of blind luck in the discovery of penicillin, though it was unknown to Fleming. Most antibiotics – hundreds are now known – are murderously toxic, because they arrest the growth of bacteria by interfering with metabolic processes of a kind that bacteria have in common with higher organisms. Penicillin is comparatively innocuous because it happens to interfere with a synthetic process peculiar to bacteria, namely the synthesis of a distinctive structural element of the bacterial cell wall.'

(Medawar, 1982, p. 274: original emphasis).

Finally, Michael's reintroduction of 'inductive trust' – 'he might have a little experience which gives him that trust' (668), and his example that 'there might have been an explosion in a sodium factory' (671), all but begs the question at issue: for Michael, the scientist's insight remained empirically led.

A little later I made my final attempt to elicit, or bring about, Michael's awareness of theory-ladenness:

I 2/008 would you think Mike that given the question about carefully observe . and observation being endless do you think that from all that you've said ... that it would be fair to say that er scientists always have some idea as to what might happen when they do an experiment?

M 2/012 (long pause) well ...

I 2/012 I'm not saying that they are always right .. but do you think that a scientist always has an idea as to what might happen when he does an experiment?

M 2/014 I wouldn't say always . I'd say for a fair amount of the time though . ( )
I interpret Michael's response (2/014) again to allow for the possibility of presuppositionless, Baconian, experiments.

Now it was only after I had completed my P' interviews that I came to judge that whilst a consideration (involving actual experience) of selective attention is useful, even necessary, as both the means to investigate persons' awareness of the theory-ladenness of observations and as a vehicle for beginning to teach the notion, it is actually insufficient with respect, at least, for the latter purpose. I now feel sure that to stand a realistic chance of teaching this thesis requires a direct consideration (again, involving actual experience) of being able to perceive successfully a single object or image in different, mutually exclusive, ways, i.e. a direct consideration and experience of theory-ladenness, e.g. by means of the classic illusion figures of perception psychology. Hence the authenticity of certain of the inferences I have felt able to make in connection with "theory-ladenness of observations in the classroom" may have been weakened slightly by this shortcoming of my initial questioning strategy.

In the final part of the interview, I elicited from Michael a metaphor for 'scientific progress' using my 6 stage procedure (cf. Section 8.2.2.1):

I 2/034 can you tell me what a metaphor is?

M 2/037 is it when a state changes? like ... metamorphosis when say a metal is heated to a high temperature it turns to liquid . or is it more like body changes like the incredible hulk

I 2/039 (laughs) well you're quite true . about metamorphosis ... but I'm just thinking now .. just in ordinary language . do you
know what a metaphor is. A metaphor?

M 2/042 no

I 2/044 can you give me an example?

M 2/044 metaphor is when. er. could it be melting like solidifying? is it Greek for changing or?

I 2/046 you're right in many ways but I think you're trying to relate it to chemistry. you know sublimation and all that sort of thing. changes of state and things like that. now you're quite right about metamorphosis being about changing um its just that if you could try and get outside the science lesson part of this interview and just think. if you were in the playground. of an example of what someone was saying just speaking about anything at all. of a metaphor do you know what it means in language?

M 2/055 change in certain conditions?

I 2/056 well I'll tell you what I will do I'll give you an example of a metaphor. Peter is a lion. O.K. that's an example of a metaphor

M 2/058 so's they'll expect Peter to be a boy do you mean it more in that way?

I 2/060 Peter is a boy?

M 2/060 you know instead of Peter is a lion

I 2/061 yes. yes. you see Peter is a boy. is um quite true isn't it as a statement of fact. Peter is a lion is not true is it? its not literally true. Peter is not equal to a lion because he's a boy um. now would that saying make sense to you if someone said ooh Peter is a lion. would that make sense to you even though its not true?

M 2/067 yes. if he's going around. (growls) like that you know pretending to be a lion say.

I 2/069 if he's pretending to be a lion then would it make some kind of sense would it?

M 2/069 it would make some kind of sense. yeh

I 2/070 if Peter was um beating up Paul in the playground and someone else said. Peter is a lion now there he wouldn't be pretending that he was a lion but would this expression Peter is a lion would that still make sense to you?

M 2/074 well it could do in the meaning that he was ferocious.
menacing. like he was beating up this boy or whatever.

I 2/076 I see so .. what made you think of that particular aspect of menacing. ferocious?

M 2/078 that he's fighting and that they'd call him a lion. if er Peter is going to sleep you can't really say that Peter is a lion.

I 2/080 why not?

M 2/081 well its the image that we're given that a lion is a ferocious menacing animal.

I 2/082 so when you use the metaphor like Peter is a lion .. you're thinking now what's a lion like. its known .. the image of a lion to use your expression. is ferocious and menacing? (M agreeing) and .. they're using those parts of the image of a lion to as it were characterise what Peter's like or what Peter's doing. O.K.? They're not using --

M 2/087 adjectives in another way?

I 2/087 yeah that's right he's beating up Paul in a lion like way .. what they're not doing is saying that he's literally a lion in all respects. he hasn't got 4 legs and a tail for example .. O.K. so this is a metaphor where you're using certain aspects .. in this case its the lion. to characterise what say what Peter is doing but it is not complete. there are parts where this is metaphor. you can't carry over similarities. now this is true of all metaphors. There are similarities between the two things you're considering. but you can't say one equals the other because it will always at some point not be exactly equal. ( )

Since Michael didn't know what a metaphor was (2/034-2/055): cf. stage'(1)'), I provided a specimen metaphor, namely 'Peter is a lion' (2/056: cf. stage '(2)'), which we then explored. I drew attention to (or elicited) similarities between referrants, e.g. 'ferocious and menacing' (M 2/094), contrasts between referrants, i.e. '(Peter hasn't got 4 legs and a tail)' (I 2/087), and the fact that these qualities are characteristic of all metaphors: 'O.K. (et seq)' (I 2/087, excerpt above cf. stages '(4)' and '(5)').
In the latter part of my last utterance, begun above (I 2/087), I went on to elicit and explore the research metaphor (cf. stages '(6)' and '(7)'):

I 2/087 ( ) now what I would like you to do in the last ten minutes or so . is can you think up a metaphor for scientific progress? Scientific progress is?

M 2/103 (long pause) well its good and bad .. well .. like a tiger menacing and .... something like that?

I 2/105 something like that. yeah . you think in your head what you think scientific progress is

M 2/108 (long pause) its like a schizophrenic

I 2/108 its like a schizophrenic . scientific progress is schizophrenic?

M 2/110 schizophrenic yes . it could be bad in ways and good in other ways .. like schizophrenics sometimes you're a good person and another time you could be murdering someone . that's what science is like in a way you've got people making building bombs and other people getting antidotes for cancer and heart disease . so there's two sides to it in a sort of Jekyll and Hyde.

I 2/115 I see . so scientific progress is like a Jekyll and Hyde? ..

M 2/115 yeh .

Now, before I consider my exploration and interpretation of Michael's metaphor for 'scientific progress', I would like briefly to discuss the elicitation technique itself, i.e. 'elicited metaphors'.

Whilst my use of the technique in this particular interview is far from my best example, it does illustrate a number of recurring problems which I encountered with eliciting metaphors.

Firstly, and perhaps predictably with persons of his age, Michael was unfamiliar with the notion of metaphor and I would probably have done
better to start (as I did in later interviews) by asking him to consider a provided specimen metaphor.

Secondly, I discern problems with my purposes for the technique which are of a more fundamental nature concerning the underlying theory. To elaborate, one of the attractions to me of metaphor, constructivistically construed, is that it shares a certain feature with the notion of a personal construct, viz. it constitutes simultaneously both a mental process and a mental product. As Schoen puts it in discussing his 'generative' theory of metaphor:

'[metaphor is] central to the task of accounting for our perspective on the world: how we think about these things, make sense of reality, and set the problems we later try to solve. In this [generative] sense, 'metaphor' refers to a certain kind of product - a perspective or frame, a way of looking at things - and to a certain kind of process - a process by which new perspectives on the world come into existence' (Schoen, 1979, p. 254).

Eliciting metaphors as an investigative technique thus seemed to me to complement the inferences which I had made for PCP/ACM (cf. chapter 5.), notably that there is no purely descriptive educational research: every investigation of a person's views necessarily involves some degree of learning on the part of researcher and researched.

I now feel, however, that unless metaphors are elicited with the expressed purpose of learning something new, then some kind of merely figurative employment of metaphor by the interviewee shall almost inevitably result, thereby negating the chief hoped for potentiality of
the technique, viz. facilitating a person's access to, and communication of, certain of their core constructs. This, indeed, seemed to be the case with Michael who first stated his view "literally" - 'well its good and bad' (2/103) - and then went on to advocate his 'tiger' (2/103) and 'schizophrenic' (2/108) metaphor.

I believe that, appropriately handled, 'elicited metaphors' may constitute a powerful teaching technique which is compatible with, and contributive to, a 'Personal Construct Pedagogy' and, furthermore, that constructivist theories of the dynamics of metaphor function may help elucidate the Kellyan process of 'superordination' in personal construction systems, i.e. learning (see my discussion of these claims in chapter 10).

Where the emphasis in purpose is upon eliciting a person's existing ideas, as in my investigation, exploration of "tacit" metaphors i.e. metaphors introduced spontaneously by interviewees, using some of my guidelines for 'elicited metaphors' technique, may bear fruit. A further comment from Schoen made in applying his 'generative' theory of metaphor to the critical exploration and explanation of social policy, may be argued to support this conclusion:

'[ ] the notion of generative metaphor then becomes an interpretative tool [my emphasis] for the critical analysis of social policy. My point here is not that we ought to think metaphorically about social policy problems, but that we do already think about them in terms of certain pervasive, tacit generative metaphors; and that we ought to become critically aware of these generative metaphors, to increase the rigour and precision of our analysis of social policy problems'

(Schoen, 1979, p. 256: original emphasis).
Notwithstanding my recent criticisms of 'elicited metaphors' technique in its investigative role, Michael's research metaphor of 'scientific progress' being 'like a schizophrenic' (2/108) perhaps provides more than a merely figurative summary of certain of his earlier expressed views.

To elaborate, Michael's example of 'a schizophrenic' i.e. 'Jekyll and Hyde' (2/110), suggests to me that his meaning of the term, like that of many lay persons, corresponds more closely to clinical notions of multiple personality ("split" personality) rather than those of schizophrenia. This I found fascinating because Michael's metaphorical application of Jekyll and Hyde might suggest a fracturing of consciousness on the part of scientists (considered as a community or as 'individuals) in pursuit of their work and which might correspond to (and be explained by) the empiricist separation of an 'is' from an 'ought'.

With this possibility in mind I first tried to find out whether Michael construed 'scientific progress' as a perspective dependent on an objective phenomenon:

I 2/115 would it be fair to say that every time that one person says science has progressed .. another person would say no it hasn't progressed . its got worse . would you say?

M 2/118 yeh . you can say that . it has got worse because people die because of it but it has progressed as well because people are living because of it .

I 2/120 do you think that this Jekyll and this Hyde .. the good and the bad sides of science .. do they exactly balance each other out. .. or do you think that the good is slightly more than the bad element . or bad element is slightly more than the good element .?
I think the good is slightly more than the bad. You do hear a lot of more bad things than there are good things about it in the press. I would imagine so. You hear about the experiments on animals which is bad. Bombs and that which is bad, but (there's) good things like new treatment and new technology things like.....

Michael's answer is ambiguous: on the one hand, he recognises the existence of different perspectives, i.e. 'you can say that' (2/118), on the other hand, he reinforced his earlier, "absolute", examples of 'good' scientific progress, i.e. 'people getting antidotes for cancer and heart disease (2/110): 'treatment and new technology' (2/124), and 'bad' scientific progress, i.e. 'people building bombs' (2/110): 'experiments on animals'('bombs' (2/124) conducted by different individual, or groups of individual, scientists.

We continued:

I now you're saying that you think that the good side of science outweighs the bad side overall? (M agreeing). But what you tend to read about more often is the bad side? (M agreeing)

M when you watch programmes like Tomorrow's World you realise there's a gooder side..... like what you read in papers and experiments on animals which is bad and should be stopped things. Like that. Like or the scientists have just built a new super bomb so in a way the bad outdoes the good because if it was used a lot more people would be killed and that is very bad. You can look at it in that way but really I think in normal non-war times say the good is better than the bad but when it comes to a war or something science is worse than good.

I so far you've related good and bad to what you earlier called the social aspect of science can you talk about scientific progress without referring to the social aspect?

M yeh if you're that way inclined you could say progress is making the atomic bomb but I don't think that. For example you could say oh its peaceful. It keeps the peace. Another may say that scientific progress is computers and things like that storing information and things like that. If you are. Sort
of say science is good 'cause its built an hydrogen bomb because its keeps the peace that's what some people think. I don't agree with that idea but some people do think like that. there's a new computer which is a good science progress things like that.

In this final episode in the interview, Michael again showed his willingness and ability both to acknowledge the existence of different points of view, e.g. 'you can look at it in that way' (2/132), 'if you're that way inclined you could say' (2/145), 'another may say' (2/145), and to assert and delineate his own position, e.g. 'experiments on animals which is bad and should be stopped' (2/132), 'but I don't think that' (2/145), 'I don't agree with that idea but some people do think like that' (2/145).

I interpreted Michael's personal meaning of scientific progress to be cast primarily in terms of technology (and 'technical success') in the cause or application of preserving and enriching all forms of life. Such is determined by a post hoc evaluation of the empirically (technically) successful conduct of science for being "merely" scientific, recall, is 'more like being clever in a technical sort of way' (263). Michael seemed to me both to accept and reject aspects of empiricist epistemology, viz. he seemed to accept the empiricist's view that the conduct of science itself, and scientificness, is independent of social and moral issues, but yet seemed to reject the empiricist notion that empirical success is not only a necessary condition for scientific progress but also a sufficient one.

I have for the sake of clarity, perhaps, presented Michael's position rather too simply in support of this two part case.
With respect to the former, i.e. Michael's acceptance of the objectivity of science, much of my interpretation rests upon Michael's earlier contrast between 'technical' and 'social' aspects of occupations. While I have treated these as opposite poles of a personal construct in Kelly's sense it might be more authentic to say only that they tend very nearly to a relation of oppositeness.

With respect to the latter, i.e. Michael's rejection of empiricist criteria for scientific progress, I corroborate this interpretation by his use of the construct 'good vs. bad' and by his choice of instances of 'bad' scientific progress (or non progress, i.e. mere 'science'?) which he rejects and delineates from his meaning of 'good' scientific progress (i.e. scientific progress). Most noticeably amongst these, bearing in mind his earlier defence of Oppenheimer, is his judgement that 'if you're that way inclined you could say progress is making the atomic bomb but I don't think that' (2/145). Here it may be more authentic to construe Michael as a person with a developed social conscience, the effective exercise of which is undermined by his commitment to empiricist epistemology.

To explain how Michael's apparent double standard (defence of Oppenheimer qua scientist versus nuclear weapons as 'bad' scientific progress) may have come about, I suggest that one of the central doctrines of empiricist (objectivist) epistemology, namely, the distinction between an 'is' and an 'ought', reinforced by the doctrine of 'brute data' in all its many forms (including that of 'objective' falsification), encourages (now to
interpolate Michael's idiom) a 'Jekyll anf Hyde' fracturing of consciousness between the 'technical' and the 'social'.

From the post empiricist perspective that I have been defending throughout this thesis, empiricist objectivism is a dangerous shimmerer. It is dangerous because such epistemologies may, as I suggested earlier, be appealed to in support of any enquiry at any stage (Teller), notwithstanding changes of heart on the part of some (Oppenheimer). The same applies to empiricist mentors or commentators of science, including student scientists such as Michael.

Traditions of empiricist epistemology strive to exclude all considerations of the personal and the social in their criteria for science, propounding instead an objective or scientific calculus of prediction. Science and scientists are thus removed from their social context and thereby also from social responsibility. From a post empiricist viewpoint, however, our understanding of science and scientists is correspondingly impaired by so doing. This is particularly apparent in the cruder, "after the fact", forms of empiricist epistemology such as was expressed by Michael: cf. his opaque speculations about the ability of scientists to be 'clever in a technical sort of way' (263) being a 'gift' (264) etc.

Although no doubt moving beyond the meanings Michael intended for his metaphor, it may be interesting to note that Robert Louis Stevenson himself (as he has been interpreted by Calder, 1979) used his composite character of Jekyll and Hyde to symbolise not only the good and evil aspects which are a proper part of every person but also to present a
critique of a certain kind of goodness, namely, Calvinism.

Calder argues that Calvinism was construed by Stevenson to be a repressive and counter productively rigid form of morality:

"the point about Jekyll is not that he is a moral and decent man but that he has always been leading a double life. And he is leading a double life because he has aimed so high. He wanted respect, honour and distinction, to be highly regarded in society, and thus felt that he had to conceal any irregularities in his life."

(Calder, 1979, p.11).

She cites a passage from Jekyll and Hyde in support of this view:

"Hence it came about that I concealed my pleasures; and that when I reached years of reflection, and began to look round me and take stock of my progress and position in the world, I stood already committed to a profound duplicity of life. Many a man would have blazoned such irregularities as I was guilty of; but from the high views that I had set before me, I regarded and hid them with an almost morbid sense of shame. It was thus rather the exacting nature of my aspirations than any particular degradation in my faults, that made me what I was, and, with even a deeper trench than in the majority of men, severed in me those provinces of good and evil which divide and compound man's dual nature'.

(Stevenson, quoted by Calder, 1979, p. 11)

What I am suggesting is that traditions of empiricist epistemology share some of the excessively high minded idealism - and consequent defects - of Calvinism. Specifically, the unobtainability (from a post empiricist point of view) of the empiricist goal of objective knowledge endorses a 'profound duplicity' on the part of empiricist minded scientists and their commentators (philosophers, historians, teachers, students etc). Thus
Calder goes on to relate the hidden 'irregularities' of the character Dr. Jekyll, referred to in the passage quoted from the novel, above, to those of Stevenson's own life, viz. his 'bohemian reaction to Edinburgh middle class respectability, which involved frequenting the taverns and brothels of Edinburgh's underworld' (Calder, 1979, pages 11-12). I, by analogy, evoke the empiricist rewriting of history of science (and "whiggish" history of science generally cf. Section 4.2.) and demonstrated by some teachers (cf. Section 8.8.3).

Calder later concludes:

'Dr. Jekyll, in becoming Mr. Hyde, is liberating himself. He experiences a 'solution of the bonds of obligation, an unknown but not an innocent freedom of the soul' and because Jekyll has tried so hard to be good - he has led a life 'of effort, virtue and control' - the undeveloped, unexercised evil side of his nature is what is set free. The greater the aspirations towards good of Jekyll, the greater the monstrosity of Hyde.

Was Stevenson suggesting that it was dangerous to suppress certain elements of human nature? I think he was, and I think there is a great deal of psychological truth in what he is saying. He had experienced directly the iron grip of Calvinism and of bourgeois morality on human behaviour, and he had recognised that it could be destructive, destructive because it affirmed that good for the majority was something external, artificial, not intrinsic to human nature: men could not be good unless they were told how to be good. Stevenson himself recognised a rather different kind of morality, a morality that came from within, that depended on a sensitive understanding of human relationships and responsibilities, that was flexible, individual, spontaneous.'

(Calder, 1979, p. 13: my emphasis).

Here again, I suggest that Calder's construal of Stevenson's morality closely complements features of post-empiricist approaches to history and
Calder argues that Stevenson 'maintained very strong feelings about human nature, about man's inhumanity to man in general, and to women even more' (1979, p. 9) and remarks that

'It is interesting and significant that all the characters in the story are in a sense isolated. They have no wives, no family, no close friendships. They have servants and they have acquaintances, but that is all. And there is little sense of busy city life. Mr. Utterson and his friend Enfield clearly have a cool, distant relationship; there is no real intimacy. One of the most striking effects of the story's tone is the juxtaposition of these cool, rather arid characters, isolated and emotionally uncommitted, with the extreme horror and disgust which Hyde and all that is associated with him engenders. The result is more sinister than if the story were built up out of warmer, communal relationships.'

(Calder, 1979, p. 12).

I suggest that Calder's views, above, have pertinence to empiricist epistemology and that further elaboration of them mediated by appeal to Jekyll and Hyde as a sub-text, would complement and, in a modest way, extend the approach adopted by Easlea (1983) who similarly refers to Mary Shelley's Frankenstein in his critique of empiricist epistemology qua 'masculine' service (cf. my discussion of Easlea's thesis, chapter 4, section 4.4).

Whatever the worth of my scanty articulated views, it may be argued that by relating them to Michael's quality and degree of epistemological awareness I have overstated my case, given his younger age. Against this, however, I refer the reader to my previously and subsequently described
research from which I conclude that whilst it is always possible for an educand critically to create, modify or maintain a personal epistemology, there is little justification for relying upon the existing education system to facilitate these processes (cf. especially chapter 5). As I shall argue in more detail in chapter 10, the case of Michael (and other interviewees) corroborates my view that the empiricist image of science projected overall by our present education system undermines the responsibility of science to society: it effectively precludes those who are not themselves (or do not become) professional scientists from science policy and it leaves empiricist scientists poorly equipped to behave in a socially responsible way (which is not to say that post empiricist epistemology would preclude all scientific errors with social import). Lest it be thought otherwise, then, my purpose in this case-study has not been to criticise Michael himself. On the contrary, I greatly admire the way in which he stated and defended his own views whilst entertaining the views of others. I suggest that this interview demonstrates that Michael was both able and willing to assume personal responsibility in a formal educational setting and would thrive in a personal construct pedagogy.

Finally, on a methodological note, I suggest that this interview demonstrates the considerable benefits that may be had from (m)ethodological pluralism (triangulation of research methods).
Chapter 10. Discussion: Beyond Scientism in Schools?

'Let him [the child] know his fairy tale accurately, and hence perfect joy or awe in the conception of it as if it were real; thus he will always be exercising his power of grasping realities ....'

(John Ruskin, introduction to German Popular Stories, 1868; quoted by Pagels, 1982, p.155).

10.1. Introduction

This thesis began with three, overarching, complementary and overlapping aims, viz:

(1) To explore students' and teachers' personal epistemologies of science;

(2) To elucidate the epistemological image of science projected overall by the existing education system;

(3) To reconstrue Kelly's 1955 articulation of 'Man-the-Scientist'

The reconstruing of 'Man-the-Scientist' is carried out in light of both influential traditions in epistemology, on the one hand, and of the interests and commitments I discern to inform the Alternative Conceptions Movement in educational research, on the other hand, with a view to outlining a 'Personal Construct Pedagogy' applicable with initial special
reference to the teaching of science.

My purpose in this chapter, then, is (a) to summarise the inferences I have made from my investigations of '(1)' and '(2)', and which I have already discussed in detail, in their respective contexts, and, (b) to extend these inferences in further elaboration and corroboration of arguments already begun in Chapters 3, 4 and 5 and in fulfilment of '(3)'.

10.2. Epistemology in the Classroom – A Contemporary Appraisal

My field research corroborates the view that departures from the 'empiricist-inductivist' ("Baconian") model of science, long discredited by philosophers of science and science educators alike, are more apparent than real. I concur, then, with Cawthron and Rowell who, from their own studies, which included interpretation of curricular materials (for which see also Zylberstajn, 1983), conclude that 'school science generally projects an image of science which can be called empiricist inductivist' (1978, p.33) and moreover, extend it – with some qualifications – to undergraduate 'university science' ('tertiary science').

Thus, from my investigation of teaching materials and teaching practice (Chapter 6), I suggest that philosophy of science is generally dealt with sparsely, implicitly, and in an objectivist-empiricist image.

In the school, one teacher claimed to introduce consideration of
scientific method (along with laboratory safety and administrative procedures concerning homework etc) in the first lesson of term by means of a "paradigm lesson" (her descriptor). This consisted of a demonstration experiment together with a demonstration write-up. The format of the write-up was not identified to the class as scientific method, but it matched the teacher's articulation of it in interview: it had all the features of a "Medawar fraud" (cf. Medawar, 1964) i.e. empiricist-inductivist. Other teachers similarly introduced scientific method in an embedded and piecemeal manner through considerations of experimental design. None voluntarily considered the nature of scientific knowledge sub specie aeternitatis. Consideration of history of science by means of classroom discussion, dictation or teaching materials was extremely rare and when such consideration was given, the history of science had the quality of a "Whiggish" account, usually of an empiricist-inductivist and certainly of an objectivist-empiricist nature.

At the tertiary level, the overall picture was the same. There was more discussion of the nature of scientific evidence but again in embedded form, viz. the merits of competing experiments and experimental designs used to investigate particular subject areas in science. Again, a "Whiggish" history of science was projected in the empiricist-inductivist - or at least - objectivist-empiricist image.

In interviews (Chapter 8), educators demonstrated little detailed knowledge of philosophy of science but all displayed a predominant sympathy for an objectivist-empiricist model, with two teachers articulating overtly Baconian accounts. Whilst many educators expressed a
desire to include more (1) philosophy of science in their teaching, they characteristically appealed to constraints imposed by the existing curriculum content. On further enquiry, however, all educators seemed to support only a "weak" thesis of philosophy of science in science education — thereby effectively underwriting an objectivist-empiricist image of science projected by the education system. I judged overall that educators' personal epistemologies of science matched their professional ones (both extant and desired) and that these were predominantly objectivist-empiricist.

Most educands found my questions both novel and difficult to answer — which I took to be a reflection, in part, upon the low valuation placed upon philosophy of science by both teachers and the curriculum, notwithstanding the stated curriculum objectives. As with their educators, I interpreted a prevailing commitment to objectivist-empiricist models of science, with the Baconian and Positivist accounts strongly represented.

Now, my many caveats concerning interviewees' commitments to objectivist-empiricist epistemology are because none could rigorously be categorised even that broadly. Commensurate with Kelly's Fragmentation Corollary, all my interviewees displayed at least some degree of epistemological pluralism, including sympathies for post-objectivist-empiricist accounts in some contexts, as I hope to have made clear in section 8.8.

In partial elaboration of the last paragraph, however, some educands
indicated that they had availed themselves of a number of informal or non-compulsory sources of education in philosophy of science. These consisted mainly of television documentaries on topics such as the so-called 'Creation Controversy' (at the tertiary level, this was also mediated by the Christian Union) and large particle accelerator experiments, on the one hand, and television dramatisations of the lives of great scientists such as Pasteur and Oppenheimer on the other hand. For some students at the university, one source of education in philosophy of science came from an optional but excellent, balanced, course with a large historical component devoted to exploring "principles and perspectives in science".

Amongst some of these educands, their experiences of these predominantly informal sources had clearly shaken their commitment in the objectivist image of science - yet they still ultimately retained it, perhaps seeing the only alternative as analogous to magic or alchemy. Many educands, indeed, many educators, with whom I talked displayed developed social consciences and sensibilities in connection with the conduct of science and this tended to come out when discussing their personal meanings for scientific progress (advancement in scientific knowledge). With such individuals, I found that there was often something rather poignant about their sense of sadness and hopelessness in the face of "bad" science (e.g. nuclear weapons) and in its continued inevitability, for this seemed to me to be consequential of their objectivist commitments. This, as I have argued in Chapter 4, is not to say that objectivist epistemology and science conducted according to its tenets, is inherently bad, but it is blind with respect to social or inter-personal considerations without being neutral.
With interviews especially (as opposed to written exercises: Chapter 7) where trust and mutual respect could be engendered, educands demonstrated that they were both willing and able to accept personal responsibility in a formal educational setting in that they expressed, developed and critically defended their own ideas - qualities which bode well for a Personal Construct Pedagogy. Educands were often vociferous in their criticism of the existing science education system. They expressed desires for activities complementary to Thomas and Harri-Augstein's (1985) principles and themes of 'Self Organised Learning' (see section 8.8., above; section 10.3, below). Thus educands complained, for example, that science lessons were unscientific because they only did writing, whereas a scientific science lesson would involve argument and discussion; that existing science lessons were pointless by virtue of educand experiments, merely replication experiments, whereas novel, self-devised, experiments would be genuinely interesting; and so on.

Notwithstanding the potentialities implied by these recent comments, I conclude that, generally speaking, and at the present time, the curricular response to the pluralism of substantial rival traditions in philosophy of science established over the last three decades has been confused and half hearted. I contend that a comment by Schwab is sadly as pertinent today as it was when he uttered it, nearly 25 years ago:

'Our teaching laboratories invite students to discover the satisfaction of techniques mastered. They emphasise the desirability of patience, accuracy, and precision. They testify to the soundness of existing knowledge. But rarely indeed do they invite students
to discover the limitations of present knowledge or identify unsolved problems and areas of present ignorance. Much less do they invite students to invent, to devise and explore possibilities alternative to current formulations'.

(Schwab, 1962, p.39).

My research suggests that curriculum planners have yet to come to terms with the lack of orthodoxy in contemporary philosophy of science: the image of science projected overall is not only an empiricist-inductivist one, it is also a less sophisticated version than would be achieved by a conscious and conscientious articulation of, say, Bacon's epistemology.

Now, my earlier remarks concerning educators' low or ambivalent valuation of philosophy of science in the curriculum and their lack of expertise in the subject should not be taken to imply indifference to it on their part. On the contrary, whilst the educators whose classes or tutorial groups I had observed as part of my main study immediately granted me an interview (indeed, this had been negotiated even before I had conducted my observations), I found it significant that teachers often seemed to me to be very nervous about being interviewed by me - some to the extent of avoiding me - once they knew, presumably from discussions with earlier interviewees, some details about the questions that I would be asking. (I had characterised my interview as an "inquiry into teachers' personal meanings of some words used in and about science". Upon completing each interview I had also requested each interviewee not to discuss the content of the interview with others whom I had not interviewed). Such nervousness was most noticeable amongst the very young, inexperienced, teachers (as might be expected) and older teachers some of whom were candid enough to admit (in informal interviews) that this was because they
were frightened that they would somehow be "out of date". Yet all were happy to talk and to share time with me in the staffroom.

Perhaps the idea of "nervousness" might be extended to curriculum planners by way of an explanation for their present practice of placing teachers in a "double bind", viz. on the one hand including curriculum objectives concerning "the processes and reasoning involved in scientific method", by yet, on the other hand, not reflecting this in a coherent or whole hearted manner in the course examinations and meanwhile increasing course content as well.

In elaboration of the above, I suggest that few pedagogues would now disagree with Driver and Erickson who argue that 'what is called for is a clarification and redefinition of what is taken to be school science' (Driver and Erickson, 1983, p.55). However, responding to this remark is complicated in its epistemological aspect by the prima facie case that there are at least three possible curricular responses to contemporary philosophy of science, namely, (1) monistic, (2) pluralistic, (3) eclectic. Each option would seem to have its own strengths and weaknesses.

Thus the monistic option, whereby a single tradition is chosen from philosophy of science offers the possibility of coherency but raises the question of who in principle could have the competence to make such a choice? Moreover, assuming that such a choice was somehow made and reflected the prevailing preference for constructivist epistemology, certain topics in science (i.e. quantum mechanics) have successfully
defied widespread acceptance by philosophers and scientists of ontologically realist interpretation.

The pluralistic option, in which a variety of influential epistemologies are taught lessens the problem of competency of choice and overcomes the last problem of the first option. This, however, would seem to undermine any coherency in the underlying pedagogy. Moreover, it would be a travesty of epistemological sophistication if pupils had to answer questions in examinations along the lines of "compare and contrast Kuhn's incommensurability thesis with that of Feyerabend. Illustrate your answer with an example from nineteenth century science" for reasons explained by Nietzsche:

'Imagine a young head, without much experience of life, being stuffed with fifty systems ... and fifty criticisms of them, all jumbled up together - what an overgrown wilderness it will come to be! What a mocking of a philosophical education! It is, in fact, avowedly an education, not for philosophy but for an examination in philosophy'.

(Nietzsche, quoted by Passmore, 1980, p.5).

Finally, the eclectic option, in which an attempt is made to combine the best elements from a number of influential epistemologies would seem to be impossible in principle due to the fundamental and irreconcilable differences (incommensurabilities?) between them.

Thus each option has been presented and rejected. I contend that curricular science currently suffers from an "identity crisis". It is the purpose of my next section to show that a Personal Construct Pedagogy may help to break this impasse.
10.3. Towards a Personal Construct Pedagogy.

I have already mentioned the links perceived by Kelly himself between psychotherapy and learning (Chapter 5). Karst, in commenting upon Kelly's theory in its clinical role, argues that

'It is my opinion that PCT is one of a small set of theories which can afford to be technically eclectic, is technically fertile, and still remains rationally integrated'.

He goes on to demonstrate some of the (psychotherapeutic) 'technical variety' that PCP can generate and the 'technical eclecticism' it can incorporate. In extension of Karst's paper, Winter argues that

'The technical eclecticism of personal construct theory reflects its central philosophical assumption of constructive alternatism; and that a treatment service organised in accordance with this assumption could accommodate therapists of different theoretical persuasions, matching clients and therapeutic conditions in terms of dimensions by the 'personal styles' research'.
(Winter, 1985, p.129: my emphasis).

Now, I contend that PCP offers the same advantages of tolerance of 'technical eclecticism' within a 'rationally integrated' framework with respect to rival epistemologies of, and theories in, science, and, thence, to inform a science pedagogy which is appropriate both to the present lack of orthodoxy in philosophy of science and to the tenets of the ACM.
These points may be justified and given a preliminary plausibility by reference to certain of my earlier arguments in Chapters 3 and 4, namely, that the construct articulates a general meta-theory which is capable of tolerating 'methodological pluralism' (embracing a 'pluralistic methodology' in its 'elaborative' aspect); that the relationship of 'opposite contrast' constitutes a relation of 'incommensurability', and that by invoking the 'arrow of time thesis' and the 'assumption of epistemological realism', this relationship may be demonstrated to afford a 'transformational theory of reference' (a "subjectivist theory of objective reference") in which empirical evidence ('crucial experiments') functions in a manner analogous to icons in traditions of religion.

By means of these and other arguments I claimed that the formal content of PCP may be construed epistemologically as a species of 'relativist-methodological-constructivism' in which the constructive structure of Kelly's notion of a construction system (whether personal or "communal") is congruent with Feyerabend's conception of knowledge as 'an ever increasing ocean of mutually incompatible (and perhaps even incommensurable) alternatives'

(Feyerabend, 1975, p.30: original emphasis).

Whilst such a conception of knowledge is perfectly able to accommodate rival theories and meta-theories, it underwrites a thesis of radical relativity: more than mere fallibility is implied here for objectivity of knowledge claims are ruled out completely. Clearly, this must have radical consequences for theories of teaching, learning and the growth of
knowledge; as I put it (adapting Kelly's idiom):

"There could be no fairer destiny for any personal construct than that it should make an impact upon a larger construction system, in which it lives on as a relevant contrast".

So much for the epistemological aspect of PCP. How, then, might we proceed to articulate, in outline, a "Personal Construct Pedagogy"?

I believe that the most fruitful way to start is to consider the following question: 'What is education for?' Indeed, I believe that this question is of pre-eminence fundamentality and that every person who considers their activities to be connected in some way with "education" is obliged to provide at least a provisional or general answer to it. Furthermore, such persons should strive to ensure that their activities do not undermine their notions as to what education is for; at the very least, they should, at all times, strive to apprehend a relationship between the two.

Mindful though I have been of Kelly's cautions concerning the "over extension" of a theory, such as his own, beyond its original 'range of convenience', I suggest that teachers and educationalists may yet agree with Kelly's view that

'We are concerned with finding better ways to help a person reconstrue his life so that he need not be a victim of his past'.
(Kelly, 1955, p.23).

This statement constitutes the cornerstone of my "Personal Construct
Philosophy of Education", in light of which I attempt to outline a "Personal Construct Pedagogy". I argue that this basic statement of intent "cuts both ways", viz. a compatible theory of teaching must somehow give due recognition to the importance of both the consensual knowledge of society and the personal, possibly idiosyncratic, knowledge of the learner. At the same time, the reflexivity of Kelly's theory demands that similar recognition be afforded to the personal knowledge of the teacher. Thus, a truly compatible, responsible, pedagogy would celebrate all 3 parts of the triadic relationship long accepted to characterise formal teaching, viz.

"A teaches B to C"

(Where 'A' = Teacher, 'B' = a subject or discipline, and 'C' = a pupil).

I believe that these basic commitments may best initially be elucidated by reference to the following three principles for 'Self Organised Learning' articulated by the personal construct psychologists Thomas and Harri-Augstein:

1. Real personal learning depends upon an ability to use oneself as a test-bed for personal validity and viability. The construction of internal referents is primary. External criteria, normative standards, and assessment by others are secondary. Thus the quality of learning becomes defined within the person's own evaluative systems rather than judged against the criteria arrived at by 'experts'.

2. The dynamics of self-organised learning depends upon an ability to monitor the construction and reconstruction
of personal meaning over time. The development, expansion, modification and refinement of our personal models of the world can thus be systematically regulated and appreciated. Inadequate monitoring leads to inappropriate models and this can be viewed as disruptions to personal growth.

Shared meaning as against public knowledge must be truly negotiated. Individuals, pairs, groups and institutions can each become conversational entities capable of adaptive, organised learning. Such conversational networks construct their own viability and validity and thus exhibit a capacity for creative and flexible growth.

(Thomas and Harri-Augstein, 1985, pp. xxix-xxx).

With these principles in mind, I believe that a Personal Construct Pedagogy would begin with a requirement that educators assume a 'credulous attitude' (Kelly, 1955, p.174) with respect to the personal conceptions of their educands together with a desire to promote the same in the latter in vice versa. Teaching and learning in formal settings would thus be construed as a form of collaborative research, albeit, skewed because (most often) initiated by educators.

The rejection in PCP of the notion of relative levels of truth in favour of relative degrees of personal utility would rule out the use of normative assessment procedures, indeed, all genuinely alternative conceptions (unlike trivially different conceptions) would be seen as being worthy of being rewarded in some way. In a Personal Construct Pedagogy, as in Personal Construct Psychology, the construction characteristic of all individuals is held to be the same; differences being only in degree, not in kind. Whilst we continue to allow persons to be penalised by the State for maintaining and expressing such alternative conceptual frameworks as they might hold, we endorse (tacitly
or explicitly) an education system that coerces, rather than invites, persons to "reject" (alternative conceptions as "stumbling blocks"), "transcend" (alternative conceptions as "building blocks") or otherwise alter their idiosyncratic views. Accordingly, educands' genuinely alternative conceptions would be "integrated" rather than "assimilated" within the education system and society in a manner analogous to that suggested by Jenkins for 'racial equality':

'I define integration [ ] not as a flattening process of assimilation but as equal opportunity, accompanied by cultural diversity, in an atmosphere of mutual tolerance'. (Jenkins, 1967, p.267).

The scope for "successful educational outcomes" would thus be extended beyond that of only the 'Unified Scientific Outcome' (Gilbert, Osborne and Fensham, 1982).

This raises important issues and problems to do with assessment procedures and which I shall not attempt to discuss here. Moreover, their resolution is not helped by the fact that Kelly adopted an ambiguous stance with respect to normativism within his psychotherapeutic application of PCP (Warren, 1985). Against such difficulties, however, I suggest that one of the most radical and desirable possibilities that a Personal Construct Pedagogy might extend is that student scientists might become a valuable research resource for science itself. By this I mean that students, once enjoined in the enterprise, might provide a huge supply of fruitful constructive principles - thereby rehabilitating the notion of "little physicist". For this to be achieved, however, closer links between working scientists and science education would have to be developed (see
Feyerabend embraces complementary views to those which I have recently quoted from Kelly and he relates them explicitly to education:

'General education should prepare a citizen to choose between the standards ['which define special subjects and special professions'], or to find his way in a society that contains groups committed to various standards but it must under no condition bend his mind so that it conforms to the standards of one particular group'.
(Feyerabend, 1975a, p.218: original emphasis).

And elsewhere he argues that

'\[ \] it is of paramount importance to strengthen the minds of the young and 'strengthening the minds of the young' means strengthening them against any easy acceptance of comprehensive views. What we need here is an education that makes people contrary, counter-suggestive, without making them incapable of devoting themselves to the elaboration of any single view. How can this aim be achieved?

It can be achieved by protecting the tremendous imagination which children possess and by developing to the full the spirit of contradiction that exists in them'.
(Feyerabend, 1975b, p.7: original emphasis).

Such protection and facilitation of the 'tremendous imagination which children possess would, however, require special teaching and learning techniques. Luckily, these may be inferred in a more direct and elaborated way from Kelly's writings than from Feyerabend's - I am referring to Kelly(an)'s battery of psychotherapeutic techniques: see Swift, 1985c, and Pope 1985, for discussions of Fixed Role Therapy in a formal educational context.
My incommensurability criterion for alternative-hood of a conception may likewise be used to guide, non-preemptively, classroom teaching tactics intended to engender 'cognitive conflict': for purposes of group discussion, the most fruitful learner conception is the incommensurable one. This, I believe, would be an improvement upon the tendency in the literature to give vague (though not worthless) advice to be guided by 'experience' (see, e.g. Nussbaum and Novick, 1981a,b).

In light of my earlier discussion (section 8.2.2.1), I suggest that the principal utility of 'elicited metaphors' lies not in their ability to elicit pre-existing personal meanings but as a device for promoting learning. As I understand it, the reason for this is that a metaphor - understood in Schoen's (e.g. 1979) 'generative' sense - embraces a relation of incommensurability. The elicitation of metaphors would thus become one means of how to help educands create and articulate incommensurable alternatives - thereby learning something of the 'process of scientific investigation and reasoning'.

Since teaching and learning is construed as collaborative research, many of the investigative techniques used and developed in this thesis (such as RWP) might also be used for teaching purposes.

In Kellyan terms it is inappropriate to apply the definite article to any particular construct. Contrast poles for a construct of, say, "oxygen" (to take a conception which is introduced early in most science curricula) shall vary according to the purposes to which they are put. I believe,
for example, that I could make quite a convincing historiographic case for the view that in 1774 Lavoisier construed colourless gases obtained under certain specified conditions in terms of the construct "This is oxygen, it is not phlogiston", (and Priestley in terms of "This is 'dephlogisticated air', it is not phlogiston). In the 20th. century, by contrast, we are perhaps more likely to construe such gases in terms of constructs such as "This is oxygen, it is not nitrogen", or more subtly, "This is oxygen, it is not carbon dioxide". The (Kellyan) fact that contrast poles may be left implicit (and given the chimera of objectivism they usually have been), then, does not mean that they are not there. Nor do contrast poles have to remain the same even though we might use the same label for an emergent pole in different contexts (purposes), i.e. contrast poles may be iterative. This is as true in science, philosophy of science, and the science classroom as it is in life in general. An individual may help themselves to find better ways of construing a particular aspect of the world by identifying which contrast pole they have hitherto been employing. But an individual attempting to criticise or understand another person's construct must personally identify their contrast pole - or risk arguing or understanding at crossed purposes. Of course, this is only a risk, not a certainty: much, for example, has been achieved due to commonality of construction. The point I am making, however, is both that much may not have been achieved and much may have been achieved wrongly (i.e. undesiredly) through leaving contrast poles implicit, especially in social contexts. The (Kellyan) fact that the label of an emergent pole (e.g. oxygen) may endure through time and may be applicable in a variety of contexts does not allow us to assume that it has been or is the same construct (or ontological commitment/constructive principle)
throughout. Nor may we simply "translate" the terms of earlier or rival constructs into those of our later or preferred constructs until there is mutual satisfaction that the purposes for which they were intended and the criteria by which they are judged are the same. This betokens (amongst other things) a need to negotiate the context of the conception(s) under consideration; and as Kelly urges: '[ ] We insist on demonstrating relevance before we lose any sleep over a proposition' (Kelly, 1969, p. 72), a Personal Construct Pedagogy would endorse Bruner's view that

'The fact of individual differences argues for pluralism and for an enlightened opportunism in the materials and methods of instruction. Earlier we asserted [...] that no ideal sequence exists for any group of children'.
(Bruner, 1968, p.71).

In summary of these recent views, the following two classroom principles or slogans of a Personal Construct Pedagogy may be proposed:

Be aware that there is dimensionality in all knowledge claims - and negotiate for it!

Be aware that we can only ever aspire to inter-subjective similarity of knowledge claims - never objectivity, still less absolute truth!

The overall character of a Personal Construct Pedagogy, and its relationship to previously influential perspectives or traditions, may be elucidated by reference to a four part distinction proposed by Pope and Keen (1981) and which has elsewhere been summarised by Pope and Gilbert:

'The first of these is the 'cultural transmission' view. In this perspective, the teacher is mainly a transmitter of information, rules or values which have been collected in the past. The learner can acquire
absolute truth by a process of iterative accumulation. The epistemological underpinning for this approach is 'realism'. An appropriate metaphor for the view of the learner put forward by cultural transmission ideology is that of 'the machine', with the teacher being 'the engineer'.

The second of these is the 'romanticist' view, which stresses that what comes from within the individual is the most important part of development - the pedagogical environment should be permissive enough to allow 'inner good' to unfold and 'inner bad' to come under control. The emphasis is placed on health and growth and working through aspects of emotional development. This stress on emotion is consistent with an epistemology which involves the discovery of the natural and inner self. An appropriate metaphor for such an approach would be that of 'organic growth', the learner being 'the plant' and the teacher 'the horticulturalist'.

The third of these is the 'progressivist' view according to which the student should be provided with an environment which encourages active thinking. The acquisition of knowledge is seen as an act of change in the pattern of thinking brought about by experiential problem-solving situations. Reality is the interaction of human beings with their environment - the emphasis is on an active person reaching out to make sense of a universe by engaging in the reconstruction and interpretation of experiences. The didactic nature of 'conversation' could be taken as the appropriate metaphor here.

The fourth view is the 'deschooling' view, which argues that knowledge should not be seen as a purely intellectual concern. What is relevant to the learner is an essential concern of the teacher, and thus the emotional as well as the intellectual life of the student are paramount issues'. (Pope and Gilbert, 1983, p.250: my emphasis).

I believe that a Personal Construct Pedagogy would combine characteristics from both 'progressivist' (as articulated by Dewey) and 'de-schooling' (as articulated by Illich) perspectives.

An education "system" conducted along the lines I propose would be "open
education" in the sense that it would be community based. This feature might even be reflected in the architecture of school buildings, viz. they would be designed to facilitate cross-disciplinary study and resource access, rather along the lines of Countesthorpe College (described and discussed by, e.g., Watts, J, 1977).

The emphasis would be upon 'process' rather than 'content': after decades of a mainly Piagetian inspired "cult of action" in classroom teaching tactics and activities, a Personal Construct Pedagogy would re-habilitate the 'thought experiment' in formal educational settings (see Helm and Gilbert, 1985; Helm, Gilbert and Watts, 1985).

Relevance of the content chosen, and 'ontological responsibility' for it on the parts of both educands and educators, would be achieved through 'conflictual collaboration' (to use the Bishop of Durham's excellent phrase: see Chapter 4, n.30) and guaranteed by the social negotiation of personal constructs. The emphasis upon 'process' would not necessarily result in a reduction of course content but its composition could no longer be pre-determined. This would be because an unspecifiable proportion of that content would be introduced by educands (including their parents) according to their locally perceived interests and needs.

'Philosophy of education' would cease to be a 'second-order pursuit' engaged in by professional philosophers (as influentially construed in recent decades by, e.g., R.S. Peters and his school: for a critique and a discussion, see Haack, 1976). Rather, educands, their parents, educators and educationalists (including philosophers of education) would all
contribute to an ongoing (re)appraisal of the educational values informing teaching policy (primarily of their local school).

It would be anticipated that there would be difficulties in realising such a pedagogy. I suggest that chief amongst these, would be the loss of status which many educators would perceive due to the radical relativity of knowledge which is intrinsic to its epistemology (cf. Feyerabend's comments on the 'spectre of relativism' - quoted in section 4.4).

In addition, Ph.D research by Scott (personal communication) suggests that there is fragmentation between construals of teaching and construals of learning on the part of some educators and educationalists. As Thomas comments

'Education, training and therapy are all concerned with learning but theories of learning as they appear in the psychological literature would realistically more often be named theories of teaching. They are concerned with how the teachers' (or experimenters') strategies and actions influence the learner. A personal construct theory approach to learning would treat it from the learner's point of view; as the construction of new meanings or the reconstruction of existing meanings in directions which are valued by the learner' (Thomas, 1978, p.47: my emphasis).

This view complements Kelly's exhortation that teachers should construe their role as helping

'[ ] to design and implement each child's own undertakings, as well as to assist in interpreting the outcomes and in devising more cogent behavioural inquiries. [ ]. To be a fully accredited participant in the experimental enterprise she must gain some sense of what is being seen through the child's eyes'. (Kelly, 1970b, p.262: my emphasis).
Again, many educators and educationalists baulk at the quality of dimensionality in constructs and, hence, in all knowledge claims. Whilst I have tried to elucidate what may actually be involved in construing, say, a scientific theory (Chapter 4), I believe that some of the perceived difficulties may be reduced or removed by relinquishing Kelly's original strict adherence to universal bi-polarity of constructs and embracing instead a less constrained notion of 'conception' in which a commitment to the notion of dimensionality would be preserved, but it would allow for it to be one of relevant contrasts.

Finally, there is the issue of the social construction of knowledge and, here, criticism has come from both within and without the ACM, with stances tending to polarise between advocates of personal construction and those of social construction.

There is a prima facie case to be made against extending Kelly's ideas to pedagogy because of the resoluteness of their emphasis upon the "perspective of the personal": as Kelly remarked about his theory

'We start with a person. Organisms, lower animals and societies can wait'. (Kelly, 1970a, p.9).

Manifestly, 'societies' and social construction of knowledge cannot 'wait' in pedagogy.

Now, although Kelly does not ignore within the formal content of PCP the issue of social construction of knowledge altogether, viz. his 'social
corollary' which states that 'to the extent that one person construes the construction process of another they may play a role in a social process involving the other person' (Kelly, 1955, p.95), it has been roundly criticised for (amongst other things) its tautological character (Holland, 1970).

Now, I have already argued (in Chapter 4), that I consider Kelly's Commonality Corollary to enable researchers and teachers usefully and adequately to construe processes which might otherwise be termed 'social'. In elaboration of this, however, I contend that if we implement Kelly's injunction that we think in opposites, then we may argue that his meaning of 'person' is predicated upon some counterpart for 'society'. In other words, they are dichotomous poles in a construct for construing. Further, if my earlier suggestion that dimensionality is a fundamental orientating manoeuvre by which object relations, including other minds, are established is recalled, then I think that we may plausibly imagine situations - "social" situations - in which a construct may only be said fully to exist (in an abstract sense) between two minds. Processes of social constructions serve to articulate a contrast pole: such a pole may initially be perceived by a person in loose terms but to "reside" in another person's mind and dialogue serves to check that a relation of opposite contrast does, indeed, obtain between them. Alternatively, two or more persons sharing a commitment to one pole may work together in eliciting and articulating opposite contrasts from others. I find Ph.D research by Watson (personal communication) into social processes of construction corroborative of these tentative speculations, viz. Watson uses the biological idea of an 'ecosystem' as a metaphor informing his own
meta-theoretical notion of a 'conceptual ecosystem': in his account of the constructive dynamics of such systems he talks of 'coactive conceptions'. (Cf. also Thomas and Harri-Augstein's notion of a 'community of selves', quotation above).

Whatever the formal and philosophical difficulties involved in trying to relate personal and social construction of knowledge within a Personal Construct Pedagogy, personal constructivists whose interests lie within a formal educational context have placed increasing emphasis upon social issues. For example, in an attempt tentatively to articulate a constructivist theory of teaching practice, Novak explores similarities and contrasts between Dewey's and Kelly's ideas and argues that

'Regardless of the similarities, however, Kelly left out two important elements of Dewey's notion of science: (1) its embeddedness in the qualitative immediacy of experience and (2) its connection to democratic living. Without these connections, science is abstracted from its roots and consequences'. (Novak, 1985, p.5).

Novak endorses Gowin's (1981) view that education should be studied as an event which involves the blending of 'four irreducible commonplaces' (Novak, 1985, p.3), namely, teaching, learning, curriculum, and governance. Novak summarises his discussion of the interrelationships between these 'educative events' with the help of the following diagram:
Figure 10.1. Diagram Showing Inter-relationships Between Educative Events (after Novak, 1985, p.7)

Now, at the risk of ending this section upon a negative note, I contend that some educational researchers display an unfortunate tendency to promulgate their commitment to the social construction of knowledge in a manner which suggests that it is forever incompatible with approaches which emphasise the contribution and characteristics of the personal.
For reasons which I hope are now apparent, I regard this as a false antithesis (by contrast with a fruitful dichotomy). Moreover, certain influential social theorists demonstrate little understanding in their written works of the constructivist epistemology underlying personal approaches within the ACM. Solomon, for example, argues that

'Perhaps it was because educational researchers in this field had often themselves received a scientific training that many began by attributing a naive scientific method to the children. Each child had sense perceptions and experiences from which, it was assumed, a series of hypotheses about the world were built up. The child then compared further experiences with the outcomes they had expected. Daily life provided plenty of opportunities to check predictions against the results of mini-experiments or unplanned happenings. In this way, such theorists argued, the child's notions were adapted and refined so that they could be reliably used to explain the common events of the physical world.

Do the empirical ground rules really support such an individual constructivist position? If informal knowledge has been personally assembled in such a rational way why is it then inconsistently applied? When the history of science itself has thrown up so many different theories in mechanics, light, heat and respiration, to explain the simple happenings of our world, how is it that our young children hold so many ideas in common? If our pupils have such a valuable intuitive grasp of the hypothetico-deductive method, why do they then have such difficulty understanding the method of school science?'.
(Solomon, 1984, p.1).

I suggest that the epistemology informing the position which she labels and describes as 'individual constructivist' would only pass muster as a caricature of empirical-inductivism. Her criticism thus completely misses its intellectual target. This is a pity because it cannot help but undermine, at least to some extent, the possibility of fruitful dialogue with personal constructivists.
Similarly, in a later paper, Solomon reviews the emergence of the ACM and identifies within it what she terms a 'strong theoretical position' in which 'children were [construed] like scientists and [ ] their personal ideas were [understood to be] constructed in the same way as scientific theories'. (Solomon, 1986, pp. 5-6).

Solomon includes Kellyan educational research within such a position and singles it out for criticism:

'Two important points should be noted about this strong position. In the first place, Kelly's philosophy is unashamedly personal and, indeed, isolated. He assumes that each person makes up hypotheses, and then rejects or refines them as a result of his own private experience, much as a very naive philosophy of science might propose. But he also holds that no one person can construe the stream of events in the same way as another. The second point concerns the misfit between this theory and the growing body of empirical data. Many researchers, [ ], had shown clearly that the students did not apply their ideas consistently, even when the scientific problems involved were very similar. Neither could careful refutation of children's misconceptions by experiment or argument be relied upon to produce lasting change. The rationality that [ ] Kelly so insisted upon, was contrary to observation'. (Solomon, 1986, p.6).

In light of all my earlier arguments and discussion, I would dispute every one of these criticisms.

10.3.1. Recommendations for Teaching and Curricular Reform

As I shall briefly summarise them below, many of my recommendations for teaching and curricular reform shall appear to be in agreement with those.
made by many other educationalists – and so they are. Since, however, one
of the main purposes of this thesis was to develop a meta-theory for a
pedagogy which is appropriate in light of the present lack of orthodoxy in
philosophy of science and compatible with the ethos and epistemological
commitments of the ACM, I shall rely upon the reader to elaborate more
detailed inferences and distinctions concerning ways and means.

10.3.1.1. History and Philosophy of Science Education for Science Teachers

I concur with the growing number of authors (e.g. Rowell and Cawthron,
1982, Rogers, 1982) who argue that teachers (and teacher trainers) should
receive education in the history and philosophy of science. I suggest
that this could begin on a large scale very soon since it would be
anticipated and desired that both student teachers and working teachers
would contribute to the orientation and composition of such education.
With respect to the latter, I envisage in-service history and philosophy
of science workshops.

10.3.1.2. Reduction in Subject Autonomy

Teachers would be encouraged to blur the divisions between subjects (cf.
Holton's, 1973, notion of 'connective' science teaching). They would do
this by consciously and conscientiously plundering other subjects for
materials which they might find useful. This would be facilitated by
(e.g.) the following:
10.3.1.2.1 Historiographic History of Science

Resources would be developed which would help teachers to introduce curricular science historiographically, viz. episodes from the development of scientists' science would not be divorced from their personal and social context. Consideration, for example, would be given to the metaphysical commitments (constructive principles) of scientists in specific historical debates over theory-choice. On occasion, classroom examination and discussion of primary texts might provide a useful heuristic for science-teaching — see, e.g. Spray's (1981) suggestions, for the classroom use of Phillip Miller's *The Gardener's Dictionary* published in 1724.

10.3.1.2.2 Perception Psychology

Educands would be invited not only to confront the theory-ladenness of observations historiographically viz. by examination of historical episodes in the history of science, see e.g. Ross' (1972) case history of embryology, but also to experience it directly through some of the experiments and materials of perception psychology, see e.g. Wood-Robinson's (1982) suggestions for use of the Ponzo, and Ames' Room, illusions in science teaching.

10.3.1.3 Explicit Scientific Metaphysics in Classroom Activities

Historiographic consideration of scientific metaphysics would be
complemented by their explicit consideration within classroom activities. Thus I concur with Elkana who urges educators

'[ ] to let the student realise his own expectations as to the result of every experiment; to show him what his metaphysical presuppositions are, to show him how to interpret his experimental results in the framework of his own metaphysics, and thus to show him that the experiment served possibly as a 'crucial experiment' for deciding between two metaphysics (Agassi's formulation). If he succeeds in interpreting the result in the framework of his metaphysics (which can be far from our accepted presuppositions) let him continue to test critically the implications of his views'.
(Elkana, 1970, p.34).

10.3.1.4. Skill Sessions

'Skill sessions' (Ogborn, 1977), are a form of tutorial which are here described for use with physics undergraduates. In them emphasis is not placed on content, but on the development and practice of important skills such as estimating orders of magnitude and translating data from words into graphs and algebraic equations; in other words skills in qualitative reasoning. Suitably modified skill sessions have been used by Ogborn and others at levels from the sixth form to the final year at university, and in engineering, zoology, genetics and other subjects.

10.3.1.5. Realistic Images of Science as an Occupation

An attempt will be made to provide student scientists with a broad range of experiences of the conduct of science. Specific classroom activities
would be designed to emphasise specific aspects of science as an occupation, e.g. quantitative accuracy, imagination. As a part of this, closer links between school science and working scientists would be developed, e.g. talks to pupils by a variety of working scientists and/or school visits to professional laboratories.

10.3.2 Recommendations for Future Research

There have been many research interests within my outline of a Personal Construct Pedagogy and which I have left largely implicit. These include consideration of the political climate which would be compatible and I believe, necessary for a Personal Construct Pedagogy ever fully to be realised (I believe it would be along the lines of a 'free society' as described by Feyerabend cf. my discussion in Chapter 4), the development of appropriate evaluating procedures for teaching outcomes (which carries the danger of an infinite regress) and the development of a formalisation of the relationship between personal and social construction of knowledge.

In addition, however, I make the following five recommendations for future research:

10.3.2.1. Do Personal Epistemologies Maintain Personal Conceptions?

In this thesis I have extended the ACM assumption that understanding educands' ideas in science is important in formal educational settings (Driver and Erickson, 1982: quoted in Chapter 5) to embrace educands' ideas of science since I have argued that the formal difference between
them is one of degree, not of kind, and I have not tested this further, meta-theoretical, assumption in the field. For this to be done, I envisage case study investigations and demonstrations, dimensionality in the classroom.

10.3.2.2 A 'Z' Axis for ACM Research

In view of the importance I have argued should be placed upon 'constructive principles' in construing the origination and maintenance of (alternative) conceptions, I recommend further research to elucidate their (possible) existence in educands.

Such a branch of research complements Holton's (1973) suggestion that a 'Z axis' should be incorporated within history and philosophy of science - a suggestion which has been described concisely by Piatelli-Palmarini:

'From its inception, every scientific programme develops out of "themata" extremely general strategies of ordering reality expressed through "quasi-aesthetic judgements [...] with deep psychological roots". [Holton, 1973, p.26]. Gerald Holton suggests a discipline, called "thematic analysis of science", which adds a third dimension or a "Z axis" to the empirical and the heuristic-analytical axes heretofore established by historians and philosophers of science. "This third dimension is the dimension of fundamental presupposition, notions, terms, methodological judgements and decisions - in short, of themata or themes - which are themselves neither directly evolved from, nor resolvable into, objective observation on the one hand, or logical, mathematical, and other formal analytical ratiocination on the other hand". [Holton, 1973; p.57]. (Piatelli-Palmarini, 1980, p.3).
10.3.2.3. Is a Constructivist Theory of Mathematics Teaching Possible?

Problems of defending the epistemological realism of ACM metatheory have been at their most acute when research conducted under its aegis has encompassed mathematics as well as science education. The 'technical eclecticism' (epistemological pluralism) tolerated by PCP and relativist-methodological-constructivism and, thence, by a Personal Construct Pedagogy is at its least satisfactory when applied to mathematical knowledge. Quite simply the 'objects' of mathematical knowledge have never been found: to what does a triangle refer, why does Pythagoras' theorem work?

This notwithstanding, I suggest that Lakatos' (1976) construal of the growth of mathematical knowledge as a series of 'proofs and refutations' (understood in specialised senses) represents the best attempt yet at a constructivist theory of mathematical knowledge. His (1976) work, already a classic amongst professional philosophers of mathematics, is written in a form which is particularly accessible to (would-be) constructivist mathematics educators, viz. a dialogue between a mathematics teacher and his sceptical pupils!

I would also strongly recommend Davis and Hersh's (1981) introductory text on the history and philosophy of mathematics.
10.3.2.4. Epistemology is a Feminist Issue!

In Chapter 4 I argued that objectivist epistemology may be construed as a predominantly male pursuit, by appealing to Easlea's (1983) notion of 'compulsive masculinity syndrome'. If this view is accepted, then ACM metatheory (including its emerging theory of teaching), in particular, and educational theories, in general, should also be construed in terms of sexual politics. I would particularly recommend Head's (1985) work on this subject.

10.3.2.5. Functions of the Quotation

Assumptions concerning the relationship between oral comments and constructive structure are already made and explicitly recognised within ACM research (see, e.g. Bell, Brook and Driver, 1985). These assumptions should be examined further.

Complementing this, I have become interested in a slightly different issue, namely, the different functions quotations may serve within a (formal) argument. By way of a preliminary elaboration of this subject, I propose the following 8 roles for quotations which I have discerned in the literature:

1. 'Summary',
2. 'Debt',
3. 'Instantiation',
4. 'Authority',
5. 'Analogy',
6. 'Accountability',
7. 'Ingratiation',
8. 'Null Quotation',

- 10.35 -
10.4. Retrospect and Prospect

The Alternative Conceptions Movement in educational research may be seen as just one contribution to, and reflection of, a profound incipient revolution in Western thought, offering unprecedented possibilities for wisdom in a social context.

Such a revolution would require only the existing institutions to bring it about: as such it would be a peaceful, and in all ways, desirable, revolution.

 Whilst the ACM has many allies amongst 'citizens' initiatives', there is nothing inevitable about this intellectual revolution: there are already powerful conservative forces at work to prevent or lessen its impact.

I conclude that within education what is urgently needed is an open but sophisticated debate on epistemological commitments informing approaches within educational research. It must be remembered, however, that epistemology cannot supply the key to resolving all differences and difficulties: it is not sufficient for a theory of teaching. So it is essential that the epistemological debate be conducted alongside a discussion of what we each think education is for. This returns us to 'philosophy of education' in the good (i.e. non-analytical) old fashioned sense. Thus, whilst I concede that many working educators may perceive their personal interests and expertise to lie outside academic
epistemology, I suggest that there is no reason to suppose (as some have) that their contributions to the "philosophical debate" might not continue to be as committed and as valuable as those of educationalists.

I offer this thesis as my initial contribution to such a debate.
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Appendix 1. Paper by Gilbert and Swift (1985)
Towards a Lakatosian Analysis of the Piagetian and Alternative Conceptions Research Programs

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Introduction

A review of the recent literature of science education is likely to lead the reader to conclude that the ideas about psychology of Jean Piaget dominate that field. His work, and that of his immediate co-workers, apparently constitute the "received view" for most teachers-educators and, consequently, student-teachers. A high point of acceptance was reached, for the UK, in the mid-1960s. It was then possible to open respected textbooks on educational psychology (e.g., Stones, 1966) and find the Piagetian "stages" listed, apparently as "facts," without authorship being attributed in the index.

More recently, some books on classroom practice (e.g., UNESCO, 1980) have still presented Piagetian psychology without question or alternative. We suggest that such practices tend more towards indoctrination than towards education. Moreover, as Piagetian tests are turned into a technology (e.g., Shayer & Adey, 1981), the likelihood of their being used in classrooms increases enormously. These technologies are easy to apply. Their use can be so interpreted as to seem to promise the production of simple prescriptive answers to complex problems in teaching and learning. Apparently derived from Piagetian theory, and in keeping with its ethos, their ready use sidesteps the burgeoning academic criticism of Piagetian doctrines, which we discuss briefly later in this article.

Discussing one of Piaget's central ideas, that of equilibration, Richmond (1970) has pointed out that

... the equilibrium principle is closely related to the concept of readiness for learning. Readiness for this or that educational experience is another way of saying that the equilibrated structures can accommodate to a given experience. ... if intellectual activity is not best described by means of an equilibrium model then this view of readiness will not stand.

The uncritical use of Piagetian technologies may thus supply a scientistic rationale for a policy of restricted access to science education on the basis of "readiness." In times of resource scarcity, such as now, this possibility moves Piagetian technologies from the academic into the general political arena.
The consequences of resource shortfalls extend into the educational research field. The climate of opinion so created lends support to Kempa’s (1976) contention that what is needed, it would appear, is more decision-orientated research, i.e., research which gives rise to findings on the basis of which positive decisions about instructional strategies, curricular content, and other issues concerning the effectiveness of science education can be taken.

The appearance of Piagetian technologies apparently meets this demand, but one consequence may be the suspension of critical discourse about their underlying assumptions. Innumerable papers on Piagetian themes have appeared in the literature. The outcomes of such research is now almost completely predictable and, indeed, many theses appear to be based on the application of algorithms. Funding agencies may be tempted to pour their scarce resources into activities with a guaranteed “practical” outcome, even if the understanding of student learning is not thereby significantly advanced.

This is happening at a time when the validity and utility of a technology based on Piaget’s ideas are coming under increasingly severe attack (e.g., Brown & Desforges, 1979; Erikson, 1982a). Simultaneously, a new and potentially fruitful approach to research is emerging, based on a range of allied notions variously called “alternative frameworks” (Driver & Easley, 1978); “alternative conceptions” (Osborne & Gilbert, 1980), “preconceptions” (Ausubel, 1968), and “misconceptions” (Helm, 1980). These are meanings for words used in science which differ from the standard interpretations. The present situation in which workers exploring this approach find themselves may be compared analogously to Kuhn’s (1970) “preparadigmatic” phase in science, viz., so far the efforts of individuals have tended to be uncoordinated: terminology has not been agreed upon, a common methodology not shared, ultimate aims not stated, and classroom implications not explored. Despite this, such work is commanding increasing attention from within the community of educational researchers. It contributes to the increasingly constructivist orientations of educational research (Magoon, 1977), yet can be distinguished from work conducted under the aegis of Piagetian doctrines. In short, an “invisible college” for what we have termed the “alternative conceptions movement” (ACM) appears to be gradually emerging. The problems we address in this paper are how to identify and articulate what, if anything, is common within this “invisible college” and how it differs from the Piagetian School (PS).

In this approach we adopt a model, due to Lakatos (1970), for the development and evaluation of research-programs, movements, or schools. Prior to describing this specific model and our reasons for choosing it from a range of possible others, we shall consider the general case to be made for the use of models for science education research. Doran (1978) has argued strongly in favor of the use of such models, seeing each one as “a conceptual framework describing the relationships amongst key variables present in the field of enquiry.” He makes four main points. First, the use of explicit models helps clarify the initial planning of research efforts. Thus a model of the Alternative Conceptions Movement (ACM) would focus attention on key researchable features of its field. Second, models help organize data collection and assist in the formulation of variables and hypotheses. Thus researchable problems in ACM would be addressed and answered within a model of it. Third, a model may help clarify the nature of individual
research efforts. Thus a model of ACM should help individuals to more readily relate their efforts to those of other workers, while a model of the Piagetian School (PS) would assist in identifying individual work in relation to both the ACM and the PS. Fourth, lack of models limits the impact of educational research on classroom practice, and permits, apsychological teaching programs to be implemented by default. Thus a model of the ACM may show its classroom implications, and allow them to be compared to those derived from a model of the PS.

We have therefore sought an overall, general model which would facilitate analysis and a comparative appraisal of the two approaches. The principal appeal of Lakatos’ methodology for us is that it promises to facilitate this in a way which avoids Kuhn’s (1970) problem of proponents of rival approaches forever arguing at cross purposes. Our comments below are intended to substantiate this judgment.

Lakatos’ Methodology of Scientific Research Programs

Feyerabend (1974) has summarized Lakatos’ approach as follows:

The methodology of research programmes develops standards for the evaluation of (scientific, or, more generally, conceptual) change. The standards apply to research programmes, not to individual theories; they judge the evolution of a programme over a period of time, not its shape at a particular time; and they judge this evolution in comparison with the evolution of rivals, not by itself.

Before discussing the component parts of a Lakatosian research program we would first like to draw attention to some features of his methodology which we found appealing to our present application. We shall do this by elaborating on Feyerabend’s comments above. First, while Lakatos was a philosopher of science primarily concerned with the evaluation or research programs within the physical sciences, his methodology does not deny a more general application, e.g., to the social sciences, as is the case with this study. Second, judgments about the worth of any one program is always based upon a comparative evaluation with another, rival, program. For Lakatos, the history of science has rightly been a history of competing research programs—the more the better. Comparative evaluation and theoretical pluralism are principles which we endorse for reasons which will become apparent following our elaboration of the main tenets of the ACM later. Third, Lakatos does not provide “instant rationality.” Choice between rival research programs follows an appraisal of their relative worth. The resultant “breathing space” is especially welcome to newly-emerging research programs—such as the ACM—which might be prematurely rejected by other methodologies.

A Lakatosian research program has three component parts: the Negative Heuristic, the Protective Belt, and the Positive Heuristic.

The Negative Heuristic, or “hard core,” consists of a set of fundamental assumptions judged irrefutable by all those who operate within a research program. The function of the “hard core” is, by sanctioning basic assumptions, to liberate workers for empirical enquiry without having to constantly question the assumptions underlying their work. To abandon or alter any of the assumptions comprising the hard core is to participate in a different research program: the hard core is the major consideration in establishing the identity of research programs. Programs which share a hard core but have differences in
their remaining component parts (described below) are different versions of a particular research program. 

The Protective Belt consists of a changing set of "auxiliary hypotheses" which are philosophically compatible with the hard core. The auxiliary hypotheses are the "refutable variants" (Lakatos, 1970) of a research program and they serve the dual purpose of "operationalizing" and "protecting" the hard core. They operationalize the hard core by making specific predictions within the domain of enquiry. However, when an auxiliary hypothesis fails an empirical test, the resultant anomaly (initially) casts doubt only on that auxiliary hypothesis. Thus the auxiliary hypotheses "protect" the hard core by distancing (or at least delaying) philosophical and experimental attention from it. In this way the protective belt avoids the premature rejection of the hard core. The formulation and testing of auxiliary hypotheses make up the bulk of research effort in any program.

The Positive Heuristic represents research policy with respect to the auxiliary hypotheses comprising the protective belt. It is evoked in order to provide guidance when an auxiliary hypothesis falters or fails. Lakatos (1970) has summarized its nature and purpose, as follows:

(The positive heuristic is) a partially articulated set of suggestions or hints on how to change, develop, the 'refutable variants' of the research-programme and how to modify, sophisticate, the refutable 'Protective Belt'.

From the above, a Lakatosian research program may be characterized overall as "an evolving succession of theories" (Papineau, 1979). Continuity of research activity is preserved throughout this succession by the inviolability of the Negative Heuristic.

Lakatos' methodology cannot only be applied between two rival research programs, but also within a single research program. This latter, "intra" mode of application considers two rival versions of a single research program; that is, it enables a comparative evaluation of two rival research programs which share the same negative heuristic.

The basis for the comparison of research programs lies in their relative placement on an evaluative continuum between progressive and degenerate:

A research programme is said to be progressing as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with some success ('progressive problemshift'); it is stagnating if its theoretical growth lags behind its empirical growth, that is, as long as it gives only post-hoc explanations either of chance discoveries or of facts anticipated by, and discovered in, a rival programme ('degenerating problemshift'). (Lakatos, 1971a; original italics)

In this presentation of Lakatos' ideas we have so far discussed the "comparative evaluation" of rival research programs only in broad terms. This is because there exists within Lakatos' account(s) of his methodology an ambiguity over who he was addressing and over the principal purpose intended. We shall discuss these issues, since they have a bearing on our present purpose for, and manner of, employing his methodology.

On the one hand, Lakatos seems to be proposing a prescriptive account, addressed principally to the scientist and involving rules for rational (objective) theory-choice. We shall call this the "prescriptive-eliminative" interpretation. On the other hand, Lakatos seems to be proposing what might be called a "retro-scriptive" account, addressed principally to the historian/philosopher of science and involving rational (objective) standards.
for appraising theory-change. We shall call this the "normative-appraisative" interpretation.

Lakatos' methodology can be applied in either inter or intra mode in each interpretation, thus giving rise to four possibilities of audience-and-purpose.

Since our use of Lakatos' methodology is not a strict application on either the prescriptive-eliminative or the normative-appraisative interpretation, we shall, as a necessary prelude to elaborating and justifying our departure, consider them both.

Evidence for the prescriptive-eliminative interpretation arises from one of Lakatos' major claims:

... I give ... rules for the 'elimination' of whole research programmes ... If a research programme progressively explains more than a rival it "supersedes" it, and the rival can be eliminated (or, if you wish, "shelved"). (Lakatos, 1971a).

As we have already noted, one of the principal appeals of Lakatos' methodology for us is that it does not impose instant rationality. This becomes clearer if we elaborate Lakatos' criteria for a progressive problemshift.

A progressive problemshift must, in Lakatos' idiom, exhibit a "consistently progressive theoretical problemshift" (i.e., it must keep predicting novel facts) but need only display an "intermittently progressive empirical problem shift" (i.e., at least some of the predicted novel facts are empirically corroborated). This second requirement has the consequence that: "One may rationally stick to a degenerating research programme until it is overtaken by a rival and even after" (Lakatos, 1971a; original italics).

Critics have been quick to argue that this liberal proposal undermines a prescriptive-eliminative interpretation: "(Lakatos) standards ... have practical force only if they are combined with a time limit" (Feyerabend, 1970; original italics); "(Lakatos) ... must specify criteria which can be used at the time to distinguish a degenerating from a progressive research programme ... Otherwise, he has told us nothing at all" (Kuhn, 1970; original italics).

In his response to such criticism, Lakatos himself appears to endorse a rejection of a prescriptive-eliminative interpretation of his methodology:

The arguments my critics produce have made me realise that I fail to stress sufficiently forcefully one crucial message of my paper. This message is that my 'methodology', older connotations of this term notwithstanding ... presumes to give advice to the scientist neither about how to arrive at good theories nor even about which of two rival programmes he should work on. (Lakatos, 1971b; original italics)

In its stead he appears to embrace a 'normative-appraisative' interpretation:

(My methodology) only appraises fully articulated theories (or research programmes) ... whatever they have done. I can judge: I can say whether they have made progress or not. (Lakatos, 1971b; original italics)

Normative appraisals of theory-change are carried out by reference to standards which are, of course, identical to those earlier identified as criteria for theory-choice on the prescriptive-eliminative interpretation, i.e., progressive versus degenerating problemshift.

In developing his methodology, Lakatos observes that:

While there has been little agreement concerning a universal criterion of the scientific character of theories, there has been considerable agreement over the last two centuries concerning single achievements. (Lakatos, 1971; original italics)
An example, proposed and discussed by Lakatos (e.g., 1971a), of considerable agreement over a specific achievement in science is the superiority of Einstein's theory over Newton's anno 1916. Such examples represent: "... basic appraisals of the scientific elite, ..." (Lakatos, 1971a; original italics). Lakatos uses these "basic normative judgements" (Lakatos, 1971a) both as the material to which he attempts to apply his methodology (provide a rational reconstruction for) and to test it. While Lakatos does not hold that basic normative judgments can falsify his methodology in any simple way, testing is effected by means of his proposed meta-methodological criterion for acceptability:

... progress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational. (Lakatos, 1971a; original italics)

As we suggested earlier, Lakatos' methodology has received considerable interest and debate from within the community of philosophers of science. Zahar (1973a,b), for example, present an interprogram evaluation for "Einstein vs. Lorentz" and Musgrave (1976a) the same for "Oxigen vs. Phlogiston." More recently, Michod (1981) has evaluated "Evolutionary Biology" in what amounts to an intraprogram mode.

Within these applications as well as in certain more abstract criticisms (e.g., Koertge, 1976; Musgrave, 1976b) there appears to be a general view that Lakatos' methodology can provide advice to scientists, albeit of a primarily heuristic kind. Again, among those authors already cited there appears to be a prevailing scepticism towards Lakatos' liberalism and some attempts are made to "strengthen" his critical standards.

It remains a question for debate whether Lakatos himself altered his interpretation of his methodology (his meta-methodological criteria would allow, even encourage, rival versions of his methodology!) or whether he conflated theory-choice with theory-change as Berkson (1976) seems to argue.

We see our present application of Lakatos' methodology as being founded upon what we perceive to be a basic normative judgment analogous to those employed by Lakatos, viz. that in recent years there has been a progressive and substantial reduction of interest in Piaget's ideas within the community of educational researchers. During this period, the ACM has emerged in such a way that might be tentatively described as a rival Lakatosian research program to that of the PS.

In this paper we intend to follow neither a prescriptive-eliminative nor a normative-appraisative interpretation in a comparative appraisal of PS and ACM. This is for two reasons. First, in our own view, the ACM can hardly be described as a "fully articulated research program"—and it was only to this sort of program that, as we have seen, Lakatos intended his methodology to apply. Second, and far more important, Lakatos' conception of progress places more emphasis on empirical criteria than we feel is compatible with the ACM.

We suspect that Lakatos' empiricism would similarly conflict with the PS. In each case the conflict has to do with the chosen subject matter, viz., persons, i.e., cognizing systems. Lakatos' preeminent interest, by contrast, was to rationally reconstruct judgments made by scientists concerning natural (i.e. inanimate) phenomena—small areas of the "new physics" notwithstanding. This does not mean that we shall ignore Lakatos' empirical conception of progress in our comparative appraisal of the PS and ACM. On the contrary, empirical criticisms shall play an important role—as a means of drawing attention to the
different educational/(research) values and attitudes we perceive workers in the ACM to hold. We hope that these values and attitudes encourage a further shift of interest and support from the PS towards the ACM. Thus, by employing Lakatos methodology we are not attempting to appropriate a rationale for prescribing elimination of the PS, or for a normative appraisal against it. In this paper, the primary role we intend for Lakatos' methodology might be described as facilitative-appraisative.

In our view, Lakatos' methodology facilitates appraisal by providing an excellent "organizing frame" for, say, complex or fragmented research activities (e.g., the PS and ACM, respectively). In particular, we feel that Lakatos' methodology might render visible the "invisible college" of the ACM and thereby help some educational researchers appraise the historical situation as we see it. Finally, we suggest that Lakatos' methodology has great potential as a didactic instrument when used in this manner.

A Lakatosian Research Program for the Piagetian School

Negative Heuristic

Donaldson (1978) claims that, for Piaget,

the key question is: how do animals adapt to their environment? Human intelligence is then considered as one means of doing this.

Central to Piaget's theory of intelligence is that living organisms strive for equilibrium states of adaptation: they autoregulate. This is achieved principally through the mechanism of equilibration. Now Piaget (e.g., 1970) views intellectual development as analogous to epigenesis: equilibration is mediated by a sequential unfolding of organs of regulation. We have taken Piaget's hypothetical construct of equilibration and the few essential features of epigenesis to constitute the hard-core of the PS research program. This is because we believe that alteration or rejection of either of these two notions would result in participation in a different research program.

Equilibration consists of two complementary adaptive processes: "assimilation" and "accommodation." They act on the symbolic representations of events held within an individual (schemas) characteristic of each stage. Assimilation is the process in which a cognitive encounter with an external event results in an active processing of a representation of the event and its absorption into existing schemas. Where an external event cannot be construed in such a way as to fit an internal schema, the schema will be modified, by the process of accommodation, to accept the new representation. During an interaction with the environment, temporary dynamic balances, or equilibrium states, are found as a result of the operation of assimilation and accommodation. These equilibria are characterized by varying degrees of application to objects or events, their mobility or extent of transferability across data types, and stability or inclination to cope with presented demands without change.

Epigenesis has four essential features (Kitchener, 1978). First, the process of psychological development involves a causal sequence of events, with successive steps being dependent on those preceding. Second the sequence involves increased organization, differentiation and complexity, there being a transformation from homogeneous to heterogeneous states and from general to specific functions. Third, in the process towards complexity, something new emerges at each step: qualitatively different structures appear. Fourth, there is
a stepwise growth through a series of stages, each stage being marked by qualitatively different emergent structures. In short, Piaget sees psychological development as proceeding through a series of stages, each qualitatively different, in an invariant order. It is important to differentiate this assumption of stages from their theoretical manifestation in the Protective Belts.

Protective Belt

Piaget operationalizes his hard core by means of a specific set of empirically testable, i.e., potentially refutable, theories. These theories, which are normally considered without reference to the underlying biological model of epigenesis from which they are derived, constitute his Stage Theory of Intellectual Development.

There are four stages: sensori-motor, said to be found in children 0-2 years; preoperational, said to be found in children 2-7 years; concrete, said to be found in children aged 7-11 years; formal, said to be found after age 11 years. The details of these stages will not be presented here, but are readily available elsewhere (Gruber & Voneche, 1977). Movement between stages is effected by equilibration. We feel that the increasing sophistication, i.e., alteration of, and additions to, these stages, over the years in which Piaget's theory has developed, supports our assertion that they represent the refutable variants of his program.

Positive Heuristic

There are many anomalies to stage theory (see Brown & Desforges, 1979). Attempts have been made to overcome some of them. Arlin (1975), for example, cites research evidence to suggest that "... progressive changes in thought structures may extend beyond the level of formal operations." She postulates an additional "fifth stage" to further seek and account for these new structures. We view her paper as a positive heuristic, a conscious attempt to redirect research attention. Arlin proposes her fifth stage in order to help "digest" (Lakatos, 1970) the accumulating counter-evidence now threatening the fourth stage and to help Piagetians with the deepening "ocean of anomalies" (Lakatos, 1970) they appear to face.

A Lakatosian Research Program for the Alternative Conceptions Movement

Few, if any, research programs have only one version. In newly emerging programs, such as that of the ACM, there may be little agreement about which is the "standard" version; indeed, there may be almost as many versions as there are researchers! With this in mind, we urge the reader to consider our elaboration of "the" ACM research program, below, to be a very personal statement: we do not wish to distort or artificially constrain the development of this new approach.
Negative Heuristic

The Hard Core may consist of the following assumptions:

(1) The world is real.
(2) All observations are theory-laden.
(3) Individuals use personally appealing explanatory hypotheses to cope with events in their environment.
(4) The individual tests these hypotheses through interaction with reality against personally appealing criteria.
(5) Reality provides guidance as to the adequacy of these hypotheses so tested.
(6) When hypotheses are judged inadequate by such testing, either the hypotheses or the test criteria by which they were judged are modified or replaced.

The statements above establish that the ACM, like the PS, adopts a constructivist orientation. The negative heuristic is compatible with the philosophical stance of constructive-alternativism elaborated by George Kelly in his "Personal Construct Theory" (Kelly, 1955).

Protective Belt

The ACM is only just beginning to develop refutable variants for its program. At their present stage of development they are less suggestive of a pedagogy than those of the PS since their predictions are comparatively vague and general. The theories of the ACM have fewer anomalies than those of the PS.

Gilbert, Osborne, and Fensham (1982) have postulated a series of consequences of children's views of science for science teaching. They advance what amounts to a taxonomy for alternative-conceptions. Based on the notions of "children's science," "teacher's science," and "scientist's science," the meanings for words held by these respective groups, they are as depicted in Figures 1 to 5. Of particular note is the conjecture, expressed by Figure 1, that a child's personal view of science can persist unchanged by science teaching. This extreme position on a continuum suggests that what the child already believes may be much more resistant to change and have a far larger influence on learning than has previously been supposed. Teacher's beliefs and expectations with respect to both "the teaching-and-learning process" and science have also been commented on by Gilbert, Osborne, Fensham (1982). See Figures 6 to 9. We recognize that the predictions made by the elements in these taxonomies are closer to empirical generalizations than to explanatory hypotheses. This is due to the newly emergent nature of the ACM research.
The Two Perspective Outcome:

\[ S_{ch} + S_{c} \xrightarrow{\text{Teaching}} \text{Learning} \xrightarrow{\text{Learner}} S_{ch} \]

Learner Teacher Learner

**Figure 2.** Science teaching can result in a second view being acquired for use in school, but the original children's view persists elsewhere.

program. We suggest that Hewson's (1981) ideas on "conceptual exchange" may have great potential relevance to the ACM. Moreover, they represent a development that is consistent with the hard core and which is at least approaching testability.

**Positive Heuristic**

As mentioned earlier, Lakatos specified that the Positive Heuristic should function to provide suggestions to change and/or develop the Protective Belt. With respect to ACM, the following statement may provide guidance on how to modify and sophisticate the protective belt: An individual will test the adequacy of his/her hypotheses against the criteria of prediction and control of events. Further guidance will likely be specified as the research program evolves.

**A Comparative Appraisal of the Piagetian and Alternative Conceptions Research Programs**

There is no doubt that the ACM and PS are rival programs: educationalists working within either approach share a common background problem, viz., "why do students fail to learn the things we want them to learn?" Each program is founded upon a constructivist philosophy but their explications are different. That the ACM and PS embody different research programs is shown by the differences between their negative heuristics.

As an introduction to the differences we see between the work of the ACM and that of the PS, we shall summarize the diverse criticisms that the latter has attracted. This summary of criticisms is not intended to be a scholarly review allowing evaluation of the claims to be made within the present paper—this has been done many times before. We shall primarily be describing only the conclusions of such critical studies, conclusions that we (and, perhaps, ACM workers generally) accept, having given the original studies a rigorous evaluation earlier. Where we advance a criticism of the PS that is not self-contained, or where we do not cite a reference, the reader may refer to Brown and Desforges (1979) for a more detailed treatment.

**Figure 3.** The original children's view is strengthened by science teaching which now is misapplied to support it.

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A1.10
The Mixed Outcome:

Figure 4. Science teaching resulting in a mixed outcome where children's science and teacher's science now coexist.

Vast effort has been expended on successive elucidations of the Piagetian Stages. This has usually been done by a structured interview technique focussed on provided tasks, normally having a physical science content. It can be argued that, inevitably, interviewers have so firmly cast their questions, and interpreted the answers, within the Piagetian framework that they have overlooked or ignored much other data. It might be enlightening to analyze a videotaped Piagetian interview with another model of man in mind, as a prelude to a dialectic discussion. Piaget's demand for an invariant sequence of development through his postulated intellectual stages is problematic. There is evidence of very young children showing behavior anticipated in far older people without their having demonstrated prerequisite stages of intellectual development.

There is considerable ambiguity over Piaget's notion of intellectual stage: should its characteristics be regarded as something that are acquired during it, so that they are attained by its end, or should they be present at its outset? This paradox may account for the wide divergence of results.

There is also the question of competence and use: if a student can perform in a given manner, should it be expected that this performance will always be demonstrated, i.e., what predictive use have stage ascriptions? There is extensive evidence that competence and use diverge for a given child in different circumstances. Piaget's stage ascriptions all assume a high degree of correlation between thought and verbal expression—what is thought can be expressed. There seems no way, a priori, of checking this. We would describe attempts to "de-verbalize" elicitation techniques as "Piagetian technology" and express doubt on their fidelity to Piaget's original ideas and intentions.

Many research studies in the Piagetian technology would overlook the fact that each stage and its subdivisions is only a theory, capable of being refuted. Such studies render Piaget's theory of stages irrefutable by methodological fiat. In Lakatosian terms this amounts to committing auxiliary hypotheses to the hard core. The instruments developed are susceptible to many, if not all, the criticisms of intelligence tests. Alas Piagetian technologists currently appear to contribute the bulk of the enquiry supposedly conducted within the ethos of the Piagetian program. When Piaget's stage theory is treated in this way it is not possible to talk meaningfully of there being a positive heuristic for it, since this cannot operate. Piagetian technologists thus preclude theoretical progress from their work.

The Unified Scientific Outcome:

Figure 5. Science teaching which extends children's science and teacher's science to a more unified science view.
Without theoretical progress there can be no empirical progress either. Lakatos emphatically states that: "Empiricalness (or scientific character) and theoretical progress are inseparably connected" (Lakatos, 1970; original italics). This requires that a research enterprise demonstrates at least theoretical progress—or else it can be rejected as "pseudoscientific" (Lakatos, 1970). The work of the Piagetian technologists shows no "problem shift" (Lakatos, 1970, added emphasis) because their "problems" can only remain the same. Lakatos would deny there is a research program in his sense because of an effective lack of its component parts, i.e., testable theories. Such work is necessarily degenerate and pseudoscientific.

The concept of equilibration has been heavily criticized. Bruner (1959), for example, argued that the notion lacked sufficient precision to delineate the adaptations of thinking to specific changes in the environment. There is also evidence, albeit ambiguous in nature, concerning the effect of training on Piagetian stage transitions, yet no explanation has been forthcoming on the functioning, in these circumstances, of assimilation and accommodation.

With these criticisms of the PS in mind, we shall now turn attention to the ACM. Researchers within the ACM are currently directing most of their efforts to the identification and elaboration of individuals' alternative-conceptions: the constructed realities of, and their personal meanings to, the observed individuals constitute the primary focus of research attention. This might also be expected to loom large in the research priorities of the PS due to its philosophical commitment to constructivism. Yet somehow it seems to have been sidestepped. In the late 1960s Ausubel, asserted that: "The most important single factor influencing learning is what the learner already knows. Ascertain this and teach him accordingly" (Ausubel, 1968). This assertion appeared to be generally accepted by the community of educational researchers. This acceptance was perhaps not surprising since, as we have already argued, Piaget's theory appears to constitute the "received view." Yet 10 years later, in a review article, Smith (1978) was able to conclude "... the perspective of the pupil seems an unexploited stance."

Individual research papers which convey this sentiment in their title and contents are increasingly prevalent, e.g., Erikson's (1982b) "Students' Beliefs About Science Con-
The "Student Dominance" Assumption:

Figure 8. Science teaching which recognizes that learners often do hold strongly entrenched theoretical views that persist in the face of teaching.

cepts: A missing ingredient in the instructional process." On re-reading the research literature on the PS, which makes up a considerable proportion of published educational research, it is apparent that individuals' alternative conceptions have been subordinated to their ascribed (Piagetian) stage of intellectual development: it is the stage level of intellectual development of individuals and populations which have been the primary focus of attention. Put another way, alternative-conceptions appear to have been used diagnostically—as merely a means of identifying or clarifying individuals' stage level of intellectual development. The existence of alternative conceptions have often served only to demonstrate "stage (un) readiness" for specific concept learning. For ACM workers the existence of alternative-conceptions within individuals cannot be doubted, the existence of Piaget's postulated mental structures can (for primarily formal, meta-theoretical arguments against Piagetian structuralism—see Swift, 1984). Ironically, the work of the ACM may be characterized as a return to an earlier age of theoretical innocence which recalls Piaget's work of the 1920s.

Phillips (1974) suggests that educational research tends to fall into three categories: descriptive, predictive, and prescriptive. He adds that these categories appear to be held in increasing esteem (in the order listed, above) by educators. The burgeoning anomalies to stage theory suggest that Piagetian theory has failed as a predictive enterprise—yet the work of the Piagetian technologists increasingly appear to be assuming prescriptive responsibility for educational policy. This, for ACM workers, is premature and is possibly why Piaget himself wrote so little and so late on the educational implications of his theory. Piaget was foremost an epistemologist, only reluctantly a pedagogue. The educational research of the ACM is so far, and without apology, primarily a descriptive enterprise. It is primarily devoted to identifying and emphasizing the existence of alternative-conceptions. The importance and role of alternative-conceptions in personal intellectual development is emphasized without reference to postulated mental-structures. From an appraisal of the considerable academic criticism of Piaget's theory of stages (discussed earlier) ACM workers tend to agree with Brown and Desforges (1979) conclusion that "the notion of 'stage' creates more conceptual problems than it solves." They have consequently abandoned it. Herein lies the principle difference between the work of the PS and the ACM.

Teachers' Views of Science:

Figure 9. Strongly held teachers' views of science may persist or interact with the views in science curricula.
Finally, let us review the PS and ACM with Lakatos' notion of progress in mind. For such a vast enterprise the research program of the PS appears to show little of either theoretical progress or empirical progress. Indeed criticisms, such as those we have described above, are usually met with ad hoc explanations which do not advance its theoretical development—a practice initiated in the mid-1950s by Piaget himself with his so-called "theory" of décalage (décalage is a "content" decreasing manoeuvre: it predicts no novel phenomena); the generalizability of the Program shows little sign of improving. The appearance of the ACM suggests that the PS is gradually being abandoned. The standing of the ACM, on Lakatos' progressive degenerate continuum is not yet clear, but we have intimations that it will prove to be progressive. Certainly its overall coherence has advanced markedly from the atheoretical mid-1970s period of "misconceptions." The theoretical problemshift seems consistent if unspectacular. The empirical problemshift has been better than intermittent; new facts are being discovered daily, to judge from the columns of this, and other, journals.

Concluding Remarks

We believe that the relationship of the ACM to the PS shall remain problematic for some time. The challenge for ACM and PS workers alike is to resist the temptation to merely "translate" into their own terms the findings of the other research program. Instead, we urge workers to develop their ideas through use of a "pluralistic-Methodology," i.e., "... where one develops and evaluates one's personal Methodology through a dialetical consideration of alternative Methodologies." (Swift, Watts, & Pope, 1983).

The authors are very grateful to their colleagues in the Personal Construction of Knowledge Group at the University of Surrey for their help in preparing this paper. An earlier version was presented to the ADEPT/UBET/AUCET Science Education Conference, University of Oxford, September 1981.

References


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Appendix 2. Letter from Professor Geoffrey Brown (12.7.84)
Dear Dr. Swift,

Thank you for your paper 'Towards a Lakatosian Analysis . . . ' which I read with great interest. It will come as no surprise to you that I welcome your approach, particularly as within science education the Piagetian theme seems to be sustained unabated.

Your use of Lakatos' model is interesting, although I wonder whether it is perhaps slightly counter-productive in that a) it may frighten off those who need to think about it, and b) it presents a view of A.C.M. which implies greater coherence and theoretical integrity than it affords at the present time. As we have stated elsewhere, P.S. went too far in the search for universal cognitive structures and ignored organismic and situational variables. My feeling about the sources you cite for A.C.M. is that they have become somewhat repetitive in demonstrating that children's concepts are not always consonant with those of their teachers. Yet we still need to look for structures perhaps, not on the grand scale à la Piaget, but in the responses to individual curriculum content - across pupils and across situations.

Piaget argued that he was defining structure of cognition, but probably confounded structure of knowledge with structure of cognition. That doesn't mean there is no structure of cognition, and if there is, it may sometimes be at odds with structure of knowledge - hence the typical A.C.M. findings (such as those of Driver). So your signalling of A.C.M. as an alternative programme leading to a different theoretical interpretation may be premature, I hope it's also prophetic.

Good luck with the paper's publication.

Yours sincerely,

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Appendix 3. Formal Aspects of Personal Construct Psychology

(After Kelly, 1955)
Appendix 3. Formal Aspects of Personal Construct Psychology

A.3.1 Introduction

The terms and their meanings presented in this appendix are closely based on those articulated by Kelly (1955) and are grouped and labelled as by Bannister and Fransella (1980, pp. 192-195).

A.3.1 Formal content of personal construct theory

Fundamental postulate: A person's processes are psychologically channelled by the ways in which they anticipate events.

Construction corollary: A person anticipates events by construing their replications.

Individuality corollary: Persons differ from each other in their construction of events.

Organisation corollary: Each person characteristically evolves, for their convenience in anticipating events, a construction system embracing ordinal relationships between constructs.

Dichotomy corollary: A person's construction system is composed of a finite number of dichotomous constructs.

Choice corollary: A person chooses for themselves that alternative in a dichotomised construct through which they anticipate the greatest possibility for the elaboration of their system.

Range corollary: A construct is convenient for the anticipation of a finite range of events only.

Experience corollary: A person's construction system varies as they successively construe the replication of events.

Modulation corollary: The variation in a person's construction system is limited by the permeability of the constructs within whose range of convenience the variants lie.

Fragmentation corollary: A person may successively employ a variety of construction systems which are inferentially incompatible with each other.

Commonality corollary: To the extent that one person employs a construction of experience which is similar to that employed by another, his or her processes are psychologically similar to those of the other person.

Sociality corollary: To the extent that one person construes the construction process of another they may play a role in a social process involving the other person.
A.3.2 Formal aspects of constructs

Range of convenience: A construct's range of convenience comprises all those things to which the user would find its application useful.

Focus of convenience: A construct's focus of convenience comprises those particular things to which the user would find its application maximally useful. These are the elements upon which the construct is likely to have been formed originally.

Elements: The things or events which are abstracted by a person's use of the construct are called elements. In some systems these are called objects.

Context: The context of a construct comprises those elements among which the user ordinarily discriminates by means of the construct. It is somewhat more restricted than the range of convenience, since it refers to the circumstances in which the construct emerges for practical use and not necessarily to all the circumstances in which a person might eventually use the construct. It is somewhat more extensive than the focus of convenience, since the construct may often appear in circumstances where its application is not optimal.

Pole: Each construct discriminates between two poles, one at each end of its dichotomy. The elements abstracted are like each other at each pole with respect to the construct and are unlike the elements at the other pole.

Contrast: The relationship between the two poles of a construct is one of contrast.

Likeness end: When referring specifically to elements at one pole of a construct, one may use the term 'likeness end' to designate that pole.

Contrast end: When referring specifically to elements at one pole of a construct, one may use the term 'contrast end' to designate the opposite end.

Emergence: The emergent pole of a construct is that one which embraces most of the immediately perceived context.

Implicitness: The implicit pole of a construct is that one which embraces contrasting context. It contrasts with the emergent pole. Frequently the person has no available symbol or name for it; it is symbolised only implicitly by the emergent term.

Symbol: An element in the context of a construct which represents not only itself but also the construct by which it is abstracted by the user is called the construct's symbol.

Permeability: A construct is permeable if it admits newly perceived
elements to its context. It is impermeable if it rejects elements on the basis of their newness.

A.3.3 Constructs classified according to the nature of their control over their elements

Pre-emptive constructs: A construct which pre-empts its elements for membership in its own realm exclusively is called a pre-emptive construct. This is the 'nothing but' type of construction - 'if this is a ball it is nothing but a ball'.

Constellatory construct: A construct which fixes the other realm membership of its elements is called a constellatory construct. This is stereotyped or typological thinking.

Propositional construct: A construct which carries no implications regarding the other realm membership of its elements is a propositional construct. This is uncontaminated construction.

A.3.4 General diagnostic constructs

Preverbal constructs* A preverbal construct is one which continues to be used, even though it has no consistent word symbol. It may or may not have been devised before the client had command of speech symbolism.

Submergence* The submerged pole of a construct is the one which is less available for application to events.

Suspension: A suspended element is one which is omitted from the context of a construct as a result of revision of the client's construct system.

Level of cognitive awareness: The level of cognitive awareness ranges from high to low. A high level construct is one which is readily expressed in socially effective symbols; whose alternatives are both readily accessible; which falls well within the range of convenience of the client's major construction; and which is not suspended by its superordinating constructs.

Dilation: Dilation occurs when a person broadens their perceptual field in order to reorganise it on a more comprehensive level. It does not, in itself, include the comprehensive reconstruction of those elements.

Constriction: Constriction occurs when a person narrows their perceptual field in order to minimise apparent incompatibilities.

Comprehensive constructs: A comprehensive construct is one which subsumes a wide variety of events.

Incidental constructs: An incidental construct is one which subsumes a narrow variety of events.
Superordinate constructs: A superordinate construct is one which includes another as one of the elements in its context.

Subordinate constructs: A subordinate construct is one which is included as an element in the context of another.

Regnant constructs: A regnant construct is a kind of superordinate construct which assigns each of its elements to a category on an all-or-none basis, as in classical logic. It tends to be non-abstractive.

Core constructs: A core construct is one which governs a person's maintenance processes.

Peripheral constructs: A peripheral construct is one which can be altered without serious modification of the core structure.

Tight constructs: A tight construct is one which leads to unvarying predictions.

Loose constructs: A loose construct is one which leads to varying predictions but which retains its identity.

A.3.5. Constructs relating to transition

Threat: Threat is the awareness of an imminent comprehensive change in one's core structures.

Fear: Fear is the awareness of an imminent incidental change in one's core structures.

Anxiety: Anxiety is the awareness that the events with which one is confronted lie mostly outside the range of convenience of one's construct system.

Guilt: Guilt is the awareness of dislodgement of the self from one's core role structure.

Aggressiveness: Aggressiveness is the active elaboration of one's perceptual field.

Hostility: Hostility is the continued effort to extort validational evidence in favour of a type of social prediction which has already been recognized as a failure.

CPC cycle: The CPC cycle is a sequence of construction involving in succession, circumspection, pre-emption and control, leading to a choice precipitating the person into a particular situation.

Creativity Cycle: The Creativity Cycle is one which starts with loosened construction and terminates with tightened and validation construction.
Appendix 4. Teaching and Curricular Materials Collected as Part of a Lesson Observation

A4(a). Second Year Lesson 'Handouts'

These handouts comprise the second year digestion syllabus. An overhead projector slide based on the second handout was used in the lesson previous to the lesson observed.

A4(b). Textbook Reference used by Pupils in Lesson prior to L01

Pupils copied figures 18.6 and 18.7. Notes were dictated by the teacher.

A4(c). Textbook Reference used by Pupils

A4(d). Textbook Reference upon which Pupils' only previous Control Experiment was based
Copy out the following notes into the blanks and underline any words you put in. There is a list of words at the end to help you with the blanks.

**Digestion.**

When you eat food, it travels down to your stomach where it is changed into a ______ so that your ______ supply can carry it round your ______ to all the parts that need it.

You take food into your ______ and ______ it up into small pieces so that you can ______ it.

The food is made ______ by the saliva in our mouths this makes it ______ to swallow.

When you swallow food your ______ pushes it to the ______ of your mouth and down into the ______

Muscles push the food ______ the gullet and into your ______. It takes about six seconds for the food to travel from your ______ to your ______.

Inside your stomach juices work on the food and turn it into a ______, then it leaves the stomach and goes into the small ______. This is a long coiled ______ that soaks up the liquid food and gives it to the blood to take round the body.

Any waste food leaves the small ______ and goes into the ______ intestine. Here ______ that the body food is taken out and sent round the body. The ______ food leaves the body by way of the ______.

Words to help you:

blood, swallow, gullet, chew, liquid, easier, mouth, stomach, anus, back, food, intestine, tongue, tube, large, water, waste, down, wet, body.
DIGESTION

food → stomach

Food passes through the gullet to the stomach.

Liver → gall bladder → pancreas → small intestine → large intestine

Pancreas helps in digestion in the small intestine.

(Our of lesson immediately prior to 40.1)
When pepsinogen is set free in the stomach the hydrochloric acid present converts it to active pepsin. This pepsin cannot now digest the stomach walls because of their protective coating of mucus.

**Absorption in the ileum**

Nearly all the absorption of digested food takes place in the ileum, and certain of its characteristics are important adaptations to its absorbing properties:

(a) it is usually fairly long and presents a large absorbing surface to the digested food,

(b) its internal surface is greatly increased by thousands of tiny, finger-like projections called villi (Fig. 18.6 and Plate 19),

(c) the lining epithelium is very thin and the fluids can pass fairly rapidly through it,

(d) there is a dense network of blood capillaries in each villus (Fig. 18.7).

The small molecules of the digested food, principally amino acids and glucose, pass through the epithelium and the capillary walls and enter the blood plasma. They are then carried away in the capillaries which unite to form veins and eventually join up to form one large vein, the hepatic portal vein. This carries all the blood from the intestine to the liver, which may retain or alter any of the digestion products. The digested food then reaches the general circulation.

Some of the fatty acids, and glycerol from the digestion of fats, enter the blood capillaries of the villi but a large proportion may be recombined in the intestinal lining to form fats once again and then these fats pass into the lacteals. It may be that some of the finely emulsified fat is absorbed directly, i.e. without digestion, as minute droplets which subsequently enter the lacteals. The fluid in the lacteals enters the lymphatic system which forms a network all over the body and eventually empties its contents into the blood stream (p. 100).

**The large intestine (colon and rectum)**

The material passing into the large intestine consists of water with undigested matter, largely cellulose and vegetable fibres (the roughage), bacteria, mucus and dead cells from the lining of the alimentary canal. The large intestine secretes no enzymes and can absorb very little digested food. It does, however, absorb much of the water from the undigested residues. This semi-solid waste, the faeces, is passed into the rectum by peristalsis and is expelled at intervals through the anus. The residues may spend from 12 to 24 hours in the intestine.

**The caecum and appendix**

These are relatively small, probably vestigial* structures in man. In herbivores like the rabbit and the horse they are much larger, and it is here that most of the cellulose digestion takes place, largely as a result of bacterial activity.

* i.e. structures which have become apparently functionless through disuse in the course of evolution.
Investigation 6.29 How does food get into the body?

When food has been chewed and swallowed it passes through the body in a tube called the alimentary canal (or gut). It is a single, continuous tube running from the mouth to the anus. Examine the drawing in figure 6.13. If you consider the area labelled A which represents the inside of the gut you could describe any food present as within, but still outside, the body. How, then, does food get through the wall of the gut into the body? In this investigation we shall attempt to answer this question.

You will need
iodine/potassium iodide solution to test for starch
Benedict's solution to test for sugars
1% starch solution (AnalaR grade) containing 0.1% sodium chloride, 30 cm³
Visking (cellulose) tubing, 14 mm diameter - three pieces about 150 mm long
thread
wax pencil
saliva
paper clips
plastic syringe, 10 cm³
beaker, 400 cm³
three test tubes, 125 x 16 mm
three test tubes, 75 x 12 mm
thermometer, -10°C to 110°C
Suggestions for homework

1. Which elements seem to be present in foodstuffs? If these elements are present in other materials, why can’t we eat them as well? Would coal make a good food?
2. Read the Background Book, *Burning*.

B 9.3 What chemical changes take place in rusting?

The cause of rusting is investigated in a series of experiments. Control experiments are used for the first time.

A suggested approach

Everyone in the class will be familiar with rusting. They have seen rust on their bicycles, on cars, and on household and garden articles. The first question that you may ask them is “What causes rust?” Have they noticed whether there are particular conditions under which things rust more quickly? You may remind them at this point of their experiments in B 4.1. They may have noticed that their bicycles are more likely to rust if they are left outside. Things rust more readily in the bathroom or the kitchen rather than in the sitting-room. Another question is “What sort of objects rust?” They should soon come to the conclusion that only iron or steel objects rust (in the normal use of the word). It looks as though water and air may be responsible for rusting - ask the pupils now how they would test their ideas. If air is necessary, is it the oxygen or nitrogen which is active? Is water necessary and, if so, is pure water or water with a dissolved salt in it more corrosive? A discussion on how to carry out the experiments to test these theories should lead to experiments of the following type being performed by the pupils. Note that for the first time control experiments are being used.

Experiment B 9.3a

Apparatus
Each pupil or pair of pupils will need:
Page from *Laboratory Investigations*
Four test-tubes, 100 × 16 mm, fitted with corks
Test-tube rack
Spatula
Beaker, 100 cm³
Tripod and gauze
Bunsen burner and asbestos square

Petroleum or olive oil
Eight iron nails, about 3 cm long
Calcium chloride, anhydrous, small lumps

Procedure

1. For this experiment the pupil breathes out slowly and steadily through a piece of glass tubing or a drinking straw into some lime water in a 100 × 16 mm test-tube, and notes the time required for the solution to turn milky (this may be about ten seconds).

The pupil then draws air through some lime water using a filter pump, as in the diagram. The volume of lime water should be the same as that used in the breathing experiment, and the air should be drawn through at the same rate. The length of time which it takes for the milkiness to appear is noted (this may be about ten minutes). The filter pump is left on for the next experiment.

2. Carry out a similar pair of experiments with empty and dry test-tubes, this time placed in ice-cold water. Maintaining the same gas flow rates, the pupil should breathe into one tube and draw air through the other and record the times taken for the condensation of detectable amounts of liquid in each case. The liquid is identified as water by the addition of anhydrous copper sulphate or by using cobalt chloride paper.

3. To find out about the difference in temperature between inspired and expired air, a thermometer should be read while in ordinary air, and then again while the pupil breathes on to the bulb gently, the bulb being held about 5 cm from the mouth. Body temperature may also be recorded from the closed palm of the hand.
Cotton wool
Salt water (see Procedure)
Distilled water

Procedure

Investigation into rusting:
Take four test-tubes. In the first put two ordinary wire nails followed by
distilled water, enough to half cover the nails. The nails are in con-
tact with air and water, and will provide the control experiment. Then
place a cork loosely in the mouth of the test-tube to keep out dust and
prevent the water from evaporating unduly.

In the second tube place a few pieces of anhydrous calcium chloride,
followed by a small plug of cotton wool and then two nails, finally
corking the test-tube firmly. These nails will be in contact with air but
not with water.

In the third tube place enough boiled water to cover the nails com-
pletely. The water should have been boiled in a beaker for several
minutes to expel dissolved air. Then put the nails in, and place a little
Vaseline, or a few drops of olive oil, on the water by means of a
spatula. The Vaseline will melt and form an air-proof layer on the hot
water, solidifying as the water cools. Place a cork loosely in this test-
tube which now contains nails in contact with water only. The fourth
test-tube is like the first, but contains salt water instead of distilled
water.

Stand the four test-tubes in a rack and leave for several days, at the end
of which time only the 'control' nails and those in contact with salt
water will be rusty.

Experiment B 9.3b

Apparatus
Each pupil or pair of pupils will need:
Page from Laboratory Investigations
Test-tube, 150 × 25 mm
Beaker, 100 cm³
Spatula
Iron filings or steel wool
Wood splint

Procedure
Moisten the inside of the test-tube with water, sprinkle in about a
spatula measure of iron filings, and rotate the test-tube horizontally so
that the filings spread and adhere to the walls. Alternatively a small plug
of moistened iron wool may be inserted in the upper part of the test-
tube.

The test-tube is then inverted in a beaker about one-third full of water,
using the beaker lip to support the inclined test-tube. The water level
inside and outside the tube should be the same and this value noted.
The apparatus is then allowed to stand for a few days. During this time
the water may be replaced by distilled water, then after a few days
some air will have been used up, suggesting that oxygen rather than nitrogen has been
involved. The residual gas does not support combustion of a lighted
splint, confirming that the gas used up while the iron rusted must have
been oxygen.

Experiment B 9.3c

Apparatus
Each pupil or pair of pupils will need:
Page from Laboratory Investigations
Two test-tubes, 100 × 16 mm, fitted with rubber bungs
Test-tube rack
Bunsen burner and asbestos square
Tripod and gauze
Beaker, 100 cm³
Four nails
Distilled water

The teacher will require:
Cylinders of oxygen and nitrogen (delivery tubes attached to both)

Procedure
Boil some distilled water in a beaker for a few minutes to expel dis-
solved air. Place two or three nails in the test-tube, add boiled distilled
water, and then lead oxygen from a cylinder at a steady rate into the
test-tube through a delivery tube dipping into the water, until it is
thought that all the air is displaced. The delivery tube should be
removed, and the test-tube quickly closed by means of a well-fitting
rubber bung.

The entire operation should then be repeated but this time using
genre in place of oxygen.

The two test-tubes should then be left for a week at the end of which
time the nails in contact with oxygen will be found to be rusty, whereas
those in contact with nitrogen will not.
Molecules

particles to pass through. The model in Nuffield Biology, Text III, The maintenance of life, Chapter 8, (plop, beads and wire mesh) can be shown to illustrate this. It is not expected that classes should become involved with the properties of colloidal solutions.

Investigation 6.20

How does food get into the body?
The pupils’ knowledge of dialysis is used to explain in a simplified way how food gets into the body and to arrive at the concept of digestion - the breakdown of large food molecules into smaller molecules in order that they may enter the body through the gut wall.

Empty Visking tubes could be prepared beforehand. If the cellulose tubing is dry soak it in water for a few seconds, and then the thread firmly round one end to close it. In order to fill it with the starch solution, separate the sides of the tubing by rubbing between thumb and forefinger. If insufficient saliva can be collected, dilute it with a little distilled water to increase its volume before adding to the Visking tubing.

Details for preparing the starch solution are found in Technical manual 1.

In the discussion which follows, pupils should be able to see that their observations (in which a large molecule, starch, cannot pass through a membrane but a smaller molecule, sugar, can) is part of the dialysis pattern. In digestion something must convert starch to sugar. This ‘something’ is called an enzyme. Saliva contains an amylase which hydrolyses the starch to sugar (meltron). Maltose is a reducing sugar and will give a positive result with Benedict’s solution without the addition of dilute hydrochloric acid. If pupils are unfamiliar with this test, controls with and without reducing sugars should be performed. Thus pupils should be able to suggest that the large molecules of food are broken down (by enzymes) into smaller ones so enabling food to pass through the gut in the body by a process similar to dialysis (although this is, of course, an over-simplification). The name we give to this breakdown is digestion.

A chart showing something like figure 6.1 might be useful.

At this stage there is no emphasis on why food is necessary but it is explained in Investigations 7.21 and 7.22 when the assumption that food molecules (e.g. protein) are necessary in our diet because they are the building blocks of organisms is tested. That nitrogen is one of the elements in the protein molecule is also established in Investigations 7.21 and 7.22.

In Investigation 8.19 questions are asked concerning the source of molecule building blocks which will be necessary for growth. In animals this is obviously food and will have been established earlier. Plants, however, must synthesise their molecule building
Appendix 5. Versions of Written Exercise (WE)

A5(a). WE.v1

A5(b). WE.v2

A5(c). WE.v3

A5(d). WE.v4
Q1. List jobs which you would normally put under the headings of very 'scientific', or very 'non-scientific', or definitely 'both scientific and non-scientific':

<table>
<thead>
<tr>
<th>scientific</th>
<th>non-scientific</th>
<th>both</th>
</tr>
</thead>
</table>

Q2. Why do you think that I have asked you to answer Q1?

Q3. Think of something that you have done which you think was scientific and do a sketch of it.

- what is happening in your sketch?
- why is it scientific?

Q4. Carefully observe, then write down what you have observed.

(Q4a. Does the difficulty that some of you have had answering Q4 tell you anything about people who do experiments that have never been done before?).
Q1. List jobs which you would normally put under the headings of very 'scientific', or very 'non-scientific', or definitely 'both scientific and non-scientific':

| scientific | non-scientific | both |

Q2. Why do you think that I have asked you to answer Q1?

Q3. What is your most usual reason for putting a job in the scientific column?

Q4. Do a sketch of something that happened outside school in which you did something scientific.

- what is happening in your sketch?
- why is it scientific?

Q5. Carefully observe, then write down what you have observed.

(Q5a. Does the difficulty that some of you have had answering Q5 tell you anything about people who do experiments that have never been done before?)
Q1. List jobs which you would normally put under the headings of very 'scientific', or very 'non-scientific', or definately 'both scientific and non-scientific':

<table>
<thead>
<tr>
<th>scientific</th>
<th>non-scientific</th>
<th>both</th>
</tr>
</thead>
</table>

Q2. Why do you think that I have asked you to answer Q1?

Q3. What is your most usual reason for putting a job in the scientific column?

Q4. Have you found it useful to answer Q1?
   If yes, why?
   If no, why?

Q5. Do a sketch of something that happened outside school in which you did something scientific.

Q6. What is happening in your sketch?

Q7. Why is it scientific?

Q8. Carefully observe, then write down what you have observed.
   (Q8a. What difficulty have you had in trying to answer Q8?)
   (Q8b. Observations are often very important in experiments. Does the difficulty that you had in trying to answer Q8 tell you anything about people doing experiments that have never been done before?)
Please think of occupations which you would classify as being very scientific or very non-scientific or both:

<table>
<thead>
<tr>
<th>SCIENTIFIC</th>
<th>NON-SCIENTIFIC</th>
<th>BOTH</th>
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</table>
Appendix 6. Example of Completed Written Exercise: Mandy, Class 4, r.1
Mandy 15 24th June. 4th & Mr Arden.

1. scientific  non-scientific  both
   - Science teacher  secretary  Doctor
   - Engineer  Plumber  Dentist
   - Weatherman  housewife  nurse

To see whether we understand what scientific and non-scientific jobs mean,

Science teacher, because you have to know all about science, and it's very complicated.
Here I am smoking, but lighting the fog with a lighter.

The lighter is scientific, because inside it is complicated, and scientific.
No, it was not useful to answer question 1. Because we didn’t learn it, it was common sense or guess work, and it’s boring.
Appendix 7. Example of Completed Raw Practice Grid: Heather, Class RG1
<table>
<thead>
<tr>
<th></th>
<th>Rolls Royce</th>
<th>Mini</th>
<th>Aston Martin</th>
<th>VW Beetle</th>
<th>Ford Escort</th>
<th>Anglia</th>
<th>Porsche</th>
<th>Morgan</th>
<th>Vauxhall Car</th>
<th></th>
</tr>
</thead>
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<td>1 1 1 3 4 4 1 4 1 4</td>
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<td>B.</td>
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<td>C.</td>
<td>1 5 2 5 4 5 2 2 3</td>
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<td>D.</td>
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<td>Alike</td>
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<td>2 3 4 5 6 7 8 9 5</td>
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**NAME:** HEATHER SMALL

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<tr>
<td>E. Driven by Chauffeur</td>
<td>1</td>
<td>5</td>
<td>2</td>
<td>4</td>
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<td>2</td>
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<td>4</td>
<td>HARDLY EVER DRIVEN BY CHAUFFEUR</td>
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<tr>
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<td>3</td>
<td>4</td>
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<td>ONLY DRIVEN BY WEALTHY INDIVIDUALS</td>
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<td>1</td>
<td>4</td>
<td>3</td>
<td>4</td>
<td>1</td>
<td>1</td>
<td>3</td>
<td>NOT CAPABLE OF HIGH SPEEDS</td>
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Appendix 8. Example of Completed Raw Grid: Heather, C1RG1, G1
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<td>1</td>
<td>5</td>
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<td>4</td>
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<tr>
<td>2</td>
<td>B WOULD WORK IN A LAB</td>
<td>3</td>
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<td>2</td>
<td>1</td>
<td>5</td>
<td>2</td>
<td>5</td>
<td>1</td>
<td>5</td>
<td>NOT LIKELY TO WORK IN LAB</td>
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<tr>
<td>3</td>
<td>C WOULD NEED SOME KNOWLEDGE OF BIOLOGY</td>
<td>4</td>
<td>5</td>
<td>3</td>
<td>4</td>
<td>5</td>
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<td>1</td>
<td>4</td>
<td>NOT NECESSARY</td>
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<tr>
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<td>4</td>
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<td>1</td>
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<td>SHOP WORK</td>
<td>COMPUTER WORK</td>
<td>PHYSICIST</td>
<td>SECRETARY</td>
<td>MEDICAL WORK</td>
<td>DRIVER</td>
<td>BIOLOGIST</td>
<td>TEACHER AE</td>
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<td><strong>5</strong></td>
<td><strong>1</strong></td>
<td><strong>5</strong></td>
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</tr>
</tbody>
</table>

5 DIFFERENT

5. **Would have to be able to operate a computer**

6. **Would have to be able to convey scientific results to others**

7. **Ability to carry out scientific experiments is essential**

8. **Understanding of chemistry essential**
<table>
<thead>
<tr>
<th>Name: Heather</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>9</strong> It is essential to have studied science at least to A level</td>
</tr>
<tr>
<td><strong>10</strong> Work involves handling dangerous chemical substances</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Alike</th>
<th>Engineer</th>
<th>Shop Work</th>
<th>Computer Work</th>
<th>Physicist</th>
<th>Secretary</th>
<th>Medical Work</th>
<th>Driver</th>
<th>Biologist</th>
<th>PE</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>1</td>
<td>2</td>
<td>3</td>
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</tr>
</tbody>
</table>

5 different

1 5 1 1 5 1 5 1 4

An O'Level, 'CSE' or No Such Qualification is Necessary

2 4 2 1 5 2 3 2 5

Never have to do so
Appendix 9. Example of Grid Feedback Package: Heather, C1R1G1, G1
WHAT MAKES A JOB 'SCIENTIFIC', NON-SCIENTIFIC OR 'BOTH SCIENTIFIC AND NON-SCIENTIFIC'?

Last term you completed a special sort of questionnaire for me: a "Repertory Grid". I used it to gather information about your personal views on what made certain jobs very 'scientific', or very 'non-scientific', or definitively 'both scientific and non-scientific'. I also hope that your completion of the grid and reading of its analysis, which I am giving to you now, will help you to further explore your views and understanding of this subject.

I attach photocopies of both your grid and summaries of its analysis.

A computer program has "focussed" the information that you entered in the grid into clusters (see page 3). This program has called each of the job titles I gave you in the grid "elements" and each of your personal views on what makes them 'scientific' etc "constructs". To remind you what these were you will need to refer to page 4 or to the copy of your "raw", i.e. "unfocussed", grid.

When you filled in the grid I asked you to use a "splitting principle" to help you to form a number of personal constructs. The splitting principle required you to think of a way in which two jobs were alike and different from a third in terms of what made them 'scientific', 'non-scientific' or 'both scientific and non-scientific'. I specified the 3 jobs you were to consider from the 9 in the list when forming each construct.

Every construct has two "poles", each with the opposite meaning of the other. The poles correspond to the words or phrases that you wrote in the "alike" margin and the "different" margin of your grid. Each of your constructs consists of what you wrote for these poles separated by a dash:

e.g. "Skilled - Unskilled"

e.g. "Original thought - Mundane and repetitive"

When you had formed a construct you rated the jobs you had used on a scale 1 to 5. A rating of 1 was used to strongly indicate the presence of the "alike" quality or attribute you had identified. By opposite contrast, a rating of 5 was used to strongly indicate the presence of the "different" quality or attribute you had identified. Rating scores of 2 and 4 were used for less extreme positions relative to their respective poles, with 3 as a mid-point.
When you had rated the 3 jobs you had used to form a construct you then rated the remaining 6 jobs from the list using any score from 1 to 5.

On your analysis sheets you may see the expression "construct reversed". If this is the case it means that your original ratings for the construct(s) specified have been reversed, i.e. 1 is now 5 and 5 is now 1, a 4 becomes a 2 and a 2 is now entered into the focussed grid as a 4; a 3 remains the same. To preserve your original meaning of your ratings you must now reverse your poles for that particular construct. For example, the construct "Not meeting young people - Meeting young people" must now be read as "Meeting young people - Not meeting young people". The computer program reverses certain constructs to help it uncover relationships between your constructs.

To benefit from the analysis of your grid you should
(a). Note high relationships between pairs or groups of elements.
(b). Consider personal reasons why pairs or groups within the total set may be alike or dissimilar.
(c). Look at the clusters formed in order to try and work out if there are possible "superordinate" constructs.

Remember that your grid and its analysis only provide information on your views at the time you filled it in. Of course, your views may be the same now as they were then, but, equally, they may be slightly or completely different. The repertory grid should never be thought of as a test.

Thank-you for helping me,

David Swift
Grid 1: Heather

Supplied Elements (job titles):

<table>
<thead>
<tr>
<th>ENGINER</th>
<th>SHOP WORK</th>
<th>COMPUTER</th>
<th>PHYSICIST</th>
<th>SECRETARY</th>
<th>MEDICAL WORK</th>
<th>DRIVER</th>
<th>BIOLOGIST</th>
<th>TEACHER</th>
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</tr>
</tbody>
</table>

Formation of Constructs:

C.1: E.1,2,3*,
C.2: E.3,4,5*,
C.3: E.7,8,9*,
C.4: E.1,5,9*,
C.5: E.4,8,3*,
C.6: E.7,2,6*,
C.7: E.1,4,7*,
C.8: E.2,5,8*,
C.9: E.3,6,9*,
C.10: Full Context.

element number (E.)

Personal Constructs (personal views):

C.1: Need some kind of science qualification vs. Not necessary
C.2: Would work in a lab vs. Not likely to work in a lab
C.3: Would need some knowledge of biology vs. Not necessary
C.4: Maths is an essential qualification vs. Not necessary
C.5: Would have to be able to operate a computer vs. Doesn't need any understanding of computers
C.6: Would have to be able to convey scientific results to others vs. Not necessary
C.7: Ability to carry out scientific practices [sic.] is essential vs. Not necessary
C.8: Understanding of chemistry essential vs. Not necessary
C.9: It is essential to have studied science at least to 'A' level vs. An 'O' level, 'CSE' or no science qualification is necessary
C.10: Work involves handling dangerous chemical substances vs. Never have to do so

[Full context]
<table>
<thead>
<tr>
<th></th>
<th>ENGINEER</th>
<th>SHOP WORK</th>
<th>COMPUTER WORK</th>
<th>PHYSICIST</th>
<th>SECRETARY</th>
<th>MEDICAL WORK</th>
<th>DRIVER</th>
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<th>TEACHER P.E.</th>
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<td>1</td>
<td>A</td>
<td>NEED SOME KIND OF SCIENCE QUALIFICATION</td>
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<td>5</td>
<td>6</td>
<td></td>
</tr>
<tr>
<td></td>
<td>B</td>
<td>WOULD WORK IN A LAB</td>
<td>3</td>
<td>5</td>
<td>2</td>
<td>1</td>
<td>5</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td></td>
<td>C</td>
<td>WOULD NEED SOME KNOWLEDGE OF BIOLOGY</td>
<td>4</td>
<td>5</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td></td>
<td>D</td>
<td>MATHS IS AN ESSENTIAL QUALIFICATION</td>
<td>1</td>
<td>3</td>
<td>1</td>
<td>1</td>
<td>4</td>
<td>1</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>5 DIFFERENT</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>NOT NECESSARY</td>
</tr>
<tr>
<td>2</td>
<td>NOT LIKELY TO WORK IN A LAB</td>
</tr>
<tr>
<td>3</td>
<td>NOT NECESSARY</td>
</tr>
<tr>
<td>4</td>
<td>NOT NECESSARY</td>
</tr>
<tr>
<td>NAME:</td>
<td>Heather</td>
</tr>
<tr>
<td>--------</td>
<td>---------</td>
</tr>
<tr>
<td>ALIKE</td>
<td>1</td>
</tr>
</tbody>
</table>

<p>| 5. WOULD HAVE TO BE ABLE TO OPERATE A COMPUTER | 2 5 1 3 5 2 5 3 5 | DOESN'T NEED ANY UNDERSTANDING OF COMPUTERS |
| 6. WOULD HAVE TO BE ABLE TO CONVEY SCIENTIFIC RESULTS TO OTHERS | 1 5 1 1 5 1 5 1 5 | NOT NECESSARY |
| 7. ABILITY TO CARRY OUT SCIENTIFIC PROCEDURES IS ESSENTIAL | 2 5 2 1 5 1 5 1 5 | NOT NECESSARY |
| 8. UNDERSTANDING OF CHEMISTRY ESSENTIAL | 2 5 2 1 5 1 5 1 5 | NOT NECESSARY |</p>
<table>
<thead>
<tr>
<th>NAME: - Heather</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>9.</strong> IT IS ESSENTIAL TO HAVE STUDIED SCIENCE AT LEAST TO A' LEVEL</td>
</tr>
<tr>
<td>1 5 1 1 5 1 5 1 4</td>
</tr>
<tr>
<td><strong>10.</strong> WORK INVOLVES HANDLING DANGEROUS CHEMICAL SUBSTANCES</td>
</tr>
<tr>
<td>2 4 2 1 5 2 3 2 5</td>
</tr>
</tbody>
</table>

- **Engineer**
- **Shop Work**
- **Computer Work**
- **Physicist**
- **Secretary**
- **Medical Work**
- **Driver**
- **Biologist**
- **Teacher PE**

5 DIFFERENT

AN 'O' LEVEL, 'CSE' OR NO SCIENCE QUALIFICATION IS NECESSARY

NEVER HAVE TO DO SO
Appendix 10. Summaries of Analyses of Individuals' Grids (FOCUS-ed Grids)
• Would need some knowledge of biology. 3
• Would have to be able to operate a computer. 5
• Would have to be able to convey scientific results to others. 6
• It is essential to have studied science at least to 'A' level. 9
• Need some kind of science qualification. 1
• Ability to carry out scientific practice[s] is essential. 7
• Understanding of chemistry essential. 8
• Would work in a lab. 2
• Work involves handling dangerous substances. 10
• Maths is an essential qualification. 4
• Not necessary. 5
• Doesn't need any understanding of computers. 5
• Not necessary. 5
• An 'O' level, 'ESE' or no science qualification is necessary. 5
• Not necessary. 5
• Not necessary. 5
• Not likely to work in a lab. 5
• Never have to do so. 4
• Not necessary. 5

- Driver
- Shopwork
- Secretary
- Teacher
- Computer work
- Engineer
- Medical work
- Physicist

Hather - 3:14
<table>
<thead>
<tr>
<th>Description</th>
<th>Numbers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dealing directly with people</td>
<td>6 5 3 5 5 4 1 1 1</td>
</tr>
<tr>
<td>Unmedical work</td>
<td>2 5 2 2 3 2 1 1 1</td>
</tr>
<tr>
<td>Non-teaching</td>
<td>3 3 1 3 3 2 1 2 4</td>
</tr>
<tr>
<td>Meeting young people</td>
<td>4 5 2 1 1 2 1 1 4</td>
</tr>
<tr>
<td>Not meeting people at work</td>
<td>8 4 2 2 1 4 3 4 4</td>
</tr>
<tr>
<td>Using a machine</td>
<td>9 3 2 3 1 2 3 5 4</td>
</tr>
<tr>
<td>Using maths in job</td>
<td>7 2 2 2 1 4 5 3 3</td>
</tr>
<tr>
<td>Further education needed</td>
<td>1 1 1 2 1 1 3 4 4</td>
</tr>
<tr>
<td>No protective clothing needed</td>
<td>1 5 5 4 1 1 1 3 3</td>
</tr>
<tr>
<td>Not using plants and animals</td>
<td>5 3 5 1 1 1 1 2 3</td>
</tr>
</tbody>
</table>

- Not dealing with people directly
- Medical work
- Teaching
- Not meeting young people
- Meeting people at work
- Not using a machine
- Not using maths in job
- No qualifications
- Any type of protective clothing
- Using plants and animals

Diagram showing job distributions.
- Life
- Researcher
- No contact with people
- Work for mankind
- Men
- Mental knowledge
- Stationary
- Work involves travelling
- Outdoor
- Traditional job

- Machines
- Teacher
- Daily contact with other people
- Work for self benefit
- Women
- Physical [-ie] movement
- Physical [-ie] movement
- Work in one place
- Indoors
- 20th century jobs

- Engineer
- Secretary
- Teacher
- Physicist
- Computer work
- Biologist
- Medical work
- Shop work
- Mathematical qualifications needed.
- Need to be able to explain results or information clearly.
- Working independently.
- High risk involved.
- Working with harmful chemicals or machinery.
- Involved in some kind of research work.
- Qualifications needed in the services.
- Specialized medical jobs.
- No extras.
- Little or no contact with the public.

- Not essential.
- Conveying information easily not essential.
- Working with people.
- No risk.
- Working in clean, comfortable conditions.
- Some kind of work involved each day.
- Scientific qualifications not necessary.
- No special training needed.
- Are these perks with the job.
- In direct contact with the public.

15 - 16
- 13
- 12
- 11
- 10
- 9
- 8
- 7
- 6
- 5
- 4
- 3
- 2
- 1

10.4 -
- Needs mathematical skills.
- Must have a science based education.
- Needs at least one 'A' level in one of the science subjects.
- Needs a knowledge of atoms.
- Job involves pure science.
- Involves scientific instruments.
- Is there a risk of danger?
- Needs good practical and theoretical skills.
- Doesn't need to know about anatomy.

1 1 1 1 2 3 4 5 5
1 1 2 3 4 5 5 5
1 1 3 3 3 4 5 5
1 1 2 3 2 5 3 5 4
2 1 1 2 3 5 4 4 1
2 2 1 1 3 2 1 4 4
2 4 5 1 1 1 1 1

- Doesn't need a particularly good knowledge of mathematics.
- Doesn't need a science based education.
- Doesn't need science 'A' levels.
- Doesn't need to know what an atom is.
- May misuse some electrical instruments, etc., but no scientific instruments.
- No danger risk.
- Only requires basic skills.
- Needs to know the anatomy of the human body.

- Driver
- Shop work
- Secretaries
- Teachers
- Computer work
- Engineer
- Medical work
- Biologist
- Physicist
- Specific scientific qualifications needed. 4 1 1 1 1 3 4 2 3 5 5
- Scientific (well qualified). Normal high IQ 3 1 1 2 2 2 2 3 4 5 5 (No need to be well qualified intellectually)
- Mathematically scientific (intellectual). 1 2 2 1 1 3 3 5 5
- Scientific (not tool) (narrow range intellect). 2 1 1 2 2 1 4 5 4 3
- Literature based.

- Driver
- Shopwork
- Teacher
- Computer work
- Engineer
- Medical work
- Biologist
- Physicist
Demands a great need to want to learn
Demands high academic qualifications
Claims that he/she is a socialist.
Desire to know composition of things.
Needs a mathematical mind.
Has a knowledge of Co Sin Rule
Mechanical knowledge
Demands a skill to be developed unign mixes.

3  3  1  1  1  1  1  3  5  4
2  2  1  1  1  1  1  3  5  5
5  3  1  3  3  1  2  5  5  5
8  3  1  3  3  2  2  4  5  5
1  2  1  1  3  3  3  4  4  4
7  1  1  3  3  4  5  5  5  5
6  1  3  5  5  5  5  5  5  1
4  1  3  4  4  4  1  1  5  3

No ambition of knowledge needed
Does not require great qualification
Does not think of himself/herself as a scientist
Loves reading technical magazines.
A mathematical mind not needed.
Trigonometry does not come naturally.
No mechanical knowledge.
Fumble fumbers.

<table>
<thead>
<tr>
<th>DRIVER</th>
<th>Shop Work</th>
<th>Secretary</th>
</tr>
</thead>
<tbody>
<tr>
<td>Biology</td>
<td>Teacher</td>
<td>Computer Work</td>
</tr>
<tr>
<td>Physics</td>
<td>Engineer</td>
<td>Medical Work</td>
</tr>
<tr>
<td>CL RG1</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Is mathematical skill vital
Not much travel involved
Job involves paperwork
Not physically strenuous
Job in high wage bracket
Job requires high qualification.
Scientific way of thinking required for job.
Not a lot of personal contact involved.

Not vital
Travel involved.
Not a lot of paperwork.
Is physically strenuous.
Job in low wage bracket.
Job requires low qualification.
Unscientific way of thinking required for job.
Job requires being able to get on with people.
FOCUS-ed Mode Grid of C1RG1

- A10.9a

G9C7: Gee to wear a white lab coat
G4C4: Involved in some kind of research work
G5C2: Needs at least one 'A' level in one of the science subjects.
G8C5: Claims that he/she is a scientist.
G7C1: Complicated mathematical calculations involved.
G7C6: Science 'A' levels NOT required.
G1C9: It is essential to have studied science at least to 'A' level.
G1C1: Need some kind of science qualification
G7C10: Get flash initial after your name
G9C2: Job Requires high qualification.
[R] G5C4: Must have a science based education.
[R] G9C4: Job in high wage bracket
G5C1: Needs mathematical skills

13 1 1 1 2 3 4 5 5 5
12 1 1 1 2 3 4 5 5 5
9 1 1 1 3 3 3 5 5 5
7 2 1 1 3 3 3 5 5 5
2 2 1 1 1 1 3 5 5 5
3 1 1 1 1 1 3 5 5 5
10 1 1 1 1 1 4 5 5 5
4 1 1 1 1 2 4 5 5 5
8 1 1 1 1 2 2 5 5 5
5 2 1 1 1 2 2 4 5 5
2 1 1 1 2 3 4 5 5
6 2 2 1 1 2 3 4 4 5
11 3 3 1 2 1 3 4 5 4

Don't very often
Same kind of work involved everyday.
Doesn't need science 'A' levels.
Does not think of himself/herself a scientist.
Uncomplicated
Required.
An 'O' level, 'CSE' or no science qualification necessary.
Not necessary.
Don't
Job requires low qualification.
Job requires low qualification.
Job needs a science based education
Job in low wage bracket.
Doesn't need a particularly good knowledge of mathematics.
Long training period
Highly qualified
Skilled
Highly paid

Short training period
General education
Unskilled
Poorly paid

Engineer
Physicist
Biol ogist
Teacher
Computer
Driver
Shop Work
Secretary
Medical Work
Creativity
- Working to create new things: 1
- Trying to understand phenomena: 2
- Not make life easier for others: 4
- Understanding for the sake of understanding: 3

Not creative.
- Not working with old skills.
- Accepting the world as it is.
- Not making life easier for others.
- Understanding in order to apply.

Creativity vs. Affiliation

Teacher
- Driver
- Shopwork
- Secretary
- Medical work
- Engineer
- Computer work
- Physicist
- Biologist
High qualification: 1 1 2 2 1 3 4 4
Long apprenticeships: 2 1 3 2 2 1 4 5 5
High salary: 3 1 1 2 3 3 4 5 5
Working with machines: 4 2 1 1 5 5 3 3 5

Low qualification: 11 12 13 14 15 16 17
Short: 2 1 3 2 2 1 4 5 5
Low salary: 3 1 1 2 3 3 4 5 5
Working with people: 4 2 1 1 5 5 3 3 5

Work, Driver, Secretary, Medical, Teacher, Biologist, Computer, Engineer, Physicist
<table>
<thead>
<tr>
<th>Original thought</th>
<th>Exciting</th>
<th>Highly paid</th>
<th>Good prospects</th>
<th>Importance of qualifications</th>
<th>Much essential work</th>
<th>Boring</th>
</tr>
</thead>
<tbody>
<tr>
<td>5</td>
<td>4</td>
<td>1</td>
<td>6</td>
<td>1</td>
<td>7</td>
<td>4</td>
</tr>
<tr>
<td>1</td>
<td>2</td>
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<td>5</td>
<td>4</td>
<td>3</td>
<td>3</td>
<td>3</td>
<td>3</td>
</tr>
</tbody>
</table>

- Repetitive
- Boring
- Low pay
- Poor prospects
- Qualifications unimportant
- Much material work
- Interesting
FOCUS-ed Node Grid of C1RG2

**G5C5:** Interesting  
**G5C2:** Lengthy training  
**G6C1:** A lot of qualifications  
**G4C3:** Intelligence  
**G1C2:** High qualifications  
**G1C2:** Long apprenticeships  
**G2C6:** Non-scientific  
**G5C1:** Qualifications (high)  
**G2C5:** Scientist  
**G5C4:** Research work  
**G2C1:** Scientific qualifications  
**G8C3:** Long hours per week  
**G7C2:** Trying to understand phenomena  
**G6C2:** Large amount of research work  
**G4C3:** Lab work

**Boring**  
**Short training**  
**No qualifications**  
**No need of intelligence**  
**Low qualifications**  
**Short**  
**Scientific**  
**Low qualifications**  
**Non-scientific**  
**Monotonous work**  
**Other qualifications**  
**Short working week**  
**Accepting the world as it is**  
**No research work**  
**Office work**
Appendix 11. Written Exercise, Q2: Categories and Interpretation of Responses

A11(a). WE, Q2: Categories of Response

A11(b). WE, Q2: Interpretation of Responses
Q2. Why do you think I have asked you to answer Q1?

I have included this question in all three versions of the CE without altering its wording.

My purpose for this question was to elicit students' perceptions of my intervention. I was particularly interested to find out whether they accepted my introductory declaration that my investigation was exploratory and 'non-normative'. In an attempt to avoid 'tantologous' responses (responses in which students straightforwardly repeated or paraphrased my declared intention to 'find out what people in schools think') I stressed, in my verbal introduction to the question, that I wished students to speculate as to why I was interested to explore their personal views on this subject, that I had not intentionally told them my tentative views on this matter and that, again, there were 'no right or wrong answers'.

I present a summary of the categories of responses to WE1, Q2 that I created in the table below:
<table>
<thead>
<tr>
<th>No</th>
<th>Category of Response</th>
<th>Class 1</th>
<th>Class 2</th>
<th>Class 3</th>
<th>Class 4</th>
<th>Freq</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Tautologous</td>
<td>1,3,5,9,10,13,14,15,17,18,23,26</td>
<td>2,3,9,11,13,14,15,16</td>
<td>6,7,8,17</td>
<td>27</td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>Knowledge Test/Measurement</td>
<td>7,21</td>
<td>2,11,22</td>
<td>4,8,10,12</td>
<td>1,2,3,4,5,9,10,12,13,15</td>
<td>19</td>
</tr>
<tr>
<td>3</td>
<td>Career Guidance</td>
<td>1,2,11</td>
<td>4,8,11,16,18</td>
<td>5,7</td>
<td>16</td>
<td>11</td>
</tr>
<tr>
<td>4</td>
<td>Intelligence Test</td>
<td>3,9,10,16,17,18</td>
<td>10,11</td>
<td></td>
<td></td>
<td>8</td>
</tr>
<tr>
<td>5</td>
<td>Investigation of Cognition</td>
<td>7,20</td>
<td>10,12,21,23</td>
<td>11</td>
<td></td>
<td>7</td>
</tr>
<tr>
<td>6</td>
<td>Justification for School-Science</td>
<td>6,12,13</td>
<td>19</td>
<td></td>
<td></td>
<td>4</td>
</tr>
<tr>
<td>7</td>
<td>Raise Awareness Importance of Science</td>
<td>6,7</td>
<td></td>
<td>14</td>
<td></td>
<td>3</td>
</tr>
<tr>
<td>8</td>
<td>Contribution to Research</td>
<td>4,8</td>
<td>1</td>
<td></td>
<td></td>
<td>3</td>
</tr>
<tr>
<td>9</td>
<td>No Response</td>
<td></td>
<td></td>
<td>6</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>10</td>
<td>Uncategorized Data</td>
<td>20</td>
<td></td>
<td></td>
<td>1</td>
<td></td>
</tr>
</tbody>
</table>

Table: Showing Categories of Response of WE,Q2
I present a rationale for each of these categories together with examples of responses, below:

Categories of Response:

Category 1: Tautologous (n = 27)
Responses in which pupils paraphrased my previous declared intentions. Several pupils (7) perceived Q1 to be an 'opinion survey' and some of these even conjectured that I was keen to 'know the most popular answer' or 'make comparisons between schools'. None, however speculated as to why I might wish to do such things.

e.g. Class 1, r.22 We have been asked to do this because it is a research and what we think what jobs are scientific etc.

e.g. Class 3, r.9 Because you want to find out what our opinions are.

Category 2: Knowledge Test/Measurement (n = 17)
Responses in which a normative orientation was implied i.e. responses would, in some way, be judged against 'absolute' knowledge. These responses characteristically took one of two forms: (1) 'Test' (i.e. pass/fail), (2) Assessment.

Delineating these answers from the 'Tantalogous' category was often a subtle and equivocal task. I was guided by my experience from LOs, and SIs and IIs and the use of expressions such as 'how much' 'to see whether we knew the difference'.
e.g. Class 3, r.21 You have asked us this because you want to know how much we know about science and non-science.

e.g. Class 3, r.4 I think you have asked us that Q because to see if we are right or wrong if they are scientific etc.

Category 3: Career Guidance (n = 10)
Responses in which pupils perceived my intention to be didactic with specific reference to vocational guidance.

e.g. Class 2, r.19 Because if we wanted a scientific job it would show us which job we should choose, should not choose, and could choose.

e.g. Class 3, r.5 To try and see what you need for certain jobs.

Category 4: Intelligence Test (n = 9)
Responses in which the pupil perceived the CE Q1 to be an intelligence test or assessment. Reference was not usually made to the subject matter.

e.g. Class 1, r.10 I think he asked us to do that because he wants to put all our answers in a computer and see how intelligent we are. I also think it is a waste of paper.
e.g. Class 2, r.11 Because you wanted to find out if we were capable of figuring out which kind of jobs are scientific and to show us what kind of jobs we need science in.

**Category 5: Investigation of Cognition**

Responses in which pupils perceived my intention to be to investigate pupils thought processes. In these responses reference was not made to subject matter.

e.g. Class 2, r.21 I think that you asked us to do this so that you know how quickly (or slowly) we think and if we know how to classify things into different columns.

e.g. Class 4, r.11 I think he's asked us to answer No. 1 to see how our brain and thoughts work and to compare with other schools or classes.

**Category 6: Justification for School-Science (n = 4)**

Responses in which pupils perceived my intention to be didactic, viz. a justification for science in the school curriculum. Reasons were not elaborated in these responses but I suspect they constitute a sub-set of the 'Career Guidance' category.

e.g. Class 1: r.6 Telling us the reasons we do science at school. How many jobs need science qualifications.

e.g. Class 1, r.19 To tell us the reason why we do science at school.
Category 7: Raise Awareness of Importance of Science (n = 3)
Responses in which the pupils perceived my intention to be didactic viz. enlightenment as to the importance and ubiquity of science.

e.g. Class 2, r.6  To be able to realize that a lot of jobs have scientific connections.

e.g. Class 4, r.14 To found out if we know how much science is really involved in jobs today, even common jobs.

Category 8: Contribution to Research (n = 3)
Responses in which pupils perceived my intention to be that of eliciting their help to answer Q1 since I had failed to answer it myself. No reasons were advanced as to why I should want to answer the question.

e.g. Class 1, r.8  You asked us that question so that you could find out what are scientific jobs there are.

e.g. Class 3, r.1  You have asked question 1 because you dont no the answer and you are putting the question among the population to maybe find the answer.

Category 9: No Response

Category 10: Uncategorized Data (n = 1)
This pupil perceived the question to have been an exercise in problem-solving, a heuristic, worthwhile for its own sake. Specific reference to the subject matter was not made.
e.g. Class 2, r.20 To be able to put things into sets, groups etc, to get your minds working.

All (b) WE, Q2: Interpretation of Responses

<table>
<thead>
<tr>
<th>WE Q2: 'Why do you think that I have asked you to answer Q1?'</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cat. No.</td>
</tr>
<tr>
<td>---------</td>
</tr>
<tr>
<td>1</td>
</tr>
<tr>
<td>2</td>
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<td>3</td>
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<tr>
<td>4</td>
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<td>5</td>
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<td>6</td>
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<td>7</td>
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<tr>
<td>8</td>
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<tr>
<td>(9)</td>
</tr>
<tr>
<td>(10)</td>
</tr>
</tbody>
</table>

Table: Showing Categories of Responses for WE, Q2

In their WE responses to Q2, no student considered Q1 to constitute an investigation of their views on the nature (character) of the conduct of science. In light of the inferences I have made with respect to other sources of data, described both in this chapter and elsewhere, I suggest that this may be because many students considered this subject to be 'commonsense' and as unlikely to be the subject of my inquiry.

The largest single category of response that I created consisted of 'tautologous' responses (n = 27). I defined such responses in the following way:
'Responses in which students paraphrased my previously declared research intentions'.

e.g. Class 1, r.22 'We have been asked to do this because it is a research and what we think what jobs are scientific etc'.

This type of response occurred despite my attempts to avoid it through my verbal presentation of Q2 (cf. p.(4)-(5), above), and points, I believe, to a drawback to the method, viz., reduced opportunity for negotiation of purpose and for the development of mutual trust between the researcher and collaboratee (but cf. my remarks on my continuation of the investigation of this item in FIs, p. ( )-( ), below).

Within the remainder of responses I discern two major clusters of categories of response, viz. 'normative' and 'didactic':

(1) 'Normative' Responses (n = 38)

I judged all students in this category cluster, or 'superordinate' category, to view my intervention as an assessment of some kind. I comprise this superordinate category of normative responses of two categories:

- Category 2: 'Knowledge Test/Measurement' (n = 19).

I defined such responses in the following way:
'Responses in which the collaboratee implied a "normative orientation" would be adopted by the researcher, i.e., their responses to Q1 would, in some way, be judged against an "absolute" standard.'

Responses in this category themselves took one of two forms:

(a) 'Test' (i.e. pass/fail) (n = 9)

e.g. Class 3, r.4 'I think [sic.] you have asked us this Q because to see if we are right or rowng [sic.] if they are scientific etc.'

(b) 'Measurement' (n = 8)

I found delineating responses from those which I categorized as 'Tautologous' often to be a subtle and equivocal task. I was, however, guided by my experience from other aspects of my investigation (responses to other WE items, FIs, LOs etc) and the use such students made of expressions like 'how much' and 'to see whether we knew the difference'.

e.g. Class 1, r.21 'You have asked us this because you wants [sic.] to know how much we know about science and non-science.'

- Category 4: 'Intelligence Test' (n = 9)

I defined such responses thus:
'Responses in which the student perceived Q1 to be an intelligence test or assessment. Specific reference to a subject matter was not made to a subject matter'.

e.g. Class 1, r.10 'I think he asked us to do that because he wants to put all our answers in a computer and see how intelligent we are. I also think it is a waste of paper.'

(2) 'Didactic' Responses (n = 18)
I judged all students in this superordinate category to perceive my intentions to be didactic in some way. I comprise this superordinate category of 3 categories:

- Category 3: 'Career Guidance' (n = 11)

My criterial definition for these responses was:

'Responses in which students perceived my intention to be didactic, with special reference to vocational guidance'.

e.g. Class 2, r.19 'Because if we wanted a scientific job it would show us which job we should choose, should not choose and could choose.'

- Category 6: 'Justification for School-Science' (n = 4)

This category I defined in the following way:
'Responses in which students perceived my intention to be didactic in the sense of justification for science in the school curriculum. Where reasons were given these appealed to notions similar to other categories, e.g. vocational guidance.'

e.g. Class 1, r.6 'Telling us the reason we do science at school [i.e.?] How many jobs need science qualifications.'

- Category 7: 'Raise Awareness of Importance of Science' (n = 3)

I admitted responses to this category according to the following criterial definition:

'Responses in which students perceived my intention to be didactic with special reference to raising awareness of the general importance of science to occupational life'.

e.g. Class 4, r.14 'To find out if we know how much science is really involved in jobs today, even common jobs.'

Together the 'Normative' and 'Didactic' superordinate categories account for almost 70% of the responses and represent, in part, a rejection by the students of my earlier claimed 'exploratory' research intentions. Whilst most pupils appeared co-operative and relaxed during the CE, they still appeared to case me in the role of a teacher - at least, in this early stage of the CE. Indeed, one pupil even speculated this in his answer to Q2:

- All.11 -
'I think we had to answer Q1 because, you trying to find out if we know what jobs involve science and what jobs don't. Perhaps you're a science teacher?...'

I was either assessing them for something or else teaching them something.

I conjecture that the high proportion of normative responses suggests that pupils are not used to having their own opinions regarded as valid in their own right and may disbelieve it when it is claimed. Some pupils, however, responded favourably to what was apparently a novel or unusual experience for them. This was particularly clear in some FIs. For example, one 14 year old pupil asserted:

FI.1 (Marina)
278 M. ( ) what I mean is that at last someone decided they want to find out what everyone else thinks instead of what they think... I was pleased about that.

In conjunction with her later comments, given below, I to this to embody an informally expressed recognition that educational research conducted within an experimental paradigm tend to be 'conservative'. It is conservative through being primarily an analytic activity. An important side effect of this sort of approach is that there is no chance of negotiation between researcher and "subject" in either the initial formulation of the premises upon which the research is based or their later
modification. Once I has convinced Marina in the interview that my research was indeed exploratory she appeared to welcome her increased responsibility. (In her WE response to Q2, Marina had construed the exercise as an intelligence assessment - her CE response is given as the example on p.9 above: "r.10").

In the discussion which followed directly from her comment above, Maria went on to criticize the neglect of individuals that often occurs through the use of quantitative methods by teachers and researchers.

283 I. (...) do you think that perhaps other people in the class feel that teachers and researchers and government and so on dont ask children enough what they want then?

289 M. (...) they just take the average they dont go to one extreme or the other they just take the average I mean what about the other people who arnt in the average category?...

292 I. so.... do you think that education does not take account of the individual enough?

M. Yeah

I. ... people who are outside the average are perhaps not given enough attention?
296 M. Yeah... as the average well that's most people but there are people who. I mean they're still people and they're not taken enough notice of... I mean I know it's difficult for them to do things like that but I think you should take more notice of people outside the average.

Marina's allusion to difficulties, at 296, suggests that her advocacy of qualitative research methods was not altogether naive.

In summary, I believe that the students responses to Q2 suggest that most regarded the exercise as 'normative-didactic'. I believe this points to aspects of both the method of investigation (i.e. the WE) and the present education system which I regard as undesirable.

In connection with my investigation of students' personal meanings of a 'scientific job', I believe that the responses to Q2 give indirect support for the inferences I made in the grid investigation.
Appendix 12. Written Exercise, Q3: Categories and Interpretation of Responses

A12(a). WE, Q3: Categories of Response

A12(b). WE, Q3: Interpretation of Responses
### Q3 What is your most usual reason for putting a job in the scientific column?

<table>
<thead>
<tr>
<th>No.</th>
<th>Category of Response</th>
<th>Class 2</th>
<th>Class 3</th>
<th>Class 4</th>
<th>Freq</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Requires Qualifications/ Special Skills</td>
<td>6,18</td>
<td>1,3,4,5,7</td>
<td>2,6,12,13</td>
<td>17</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>8,9,11,14</td>
<td>15,16</td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>Tautologous</td>
<td>5,7,11,14</td>
<td></td>
<td>1,5,7,8,15,16,17</td>
<td>11</td>
</tr>
<tr>
<td>3</td>
<td>Mere use of Sophisticated Lab. Equip.</td>
<td>3,9,15,20</td>
<td>6</td>
<td>9,10,11</td>
<td>8</td>
</tr>
<tr>
<td>4</td>
<td>Mere use of Heat etc.</td>
<td>10,19,23,24</td>
<td>3,14,15</td>
<td></td>
<td>7</td>
</tr>
<tr>
<td>5</td>
<td>Is Complex or Difficult</td>
<td>3,6,12,18</td>
<td>16</td>
<td>1</td>
<td>6</td>
</tr>
<tr>
<td>6</td>
<td>Requires Mental Activity</td>
<td>2,20,21,22</td>
<td>12</td>
<td>9</td>
<td>6</td>
</tr>
<tr>
<td>7</td>
<td>Related to School-Science</td>
<td>4,12,16,19,21,24</td>
<td></td>
<td>6</td>
<td></td>
</tr>
<tr>
<td>8</td>
<td>No Response</td>
<td>17</td>
<td>3</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>9</td>
<td>Uncategorised Data</td>
<td>1,8,13</td>
<td>2,10</td>
<td>4</td>
<td>6</td>
</tr>
</tbody>
</table>

Table: Showing Categories of Response for WE , Q3

**Alt. (b): WE, Q2: Interpretation of Responses**

**General Comments**

1. Response categories are derived from only WEJL-4 (Σ = 57) due to the omission of this item from WEJL.

2. Despite my verbal instructions, some pupils appeared to interpret "most usual reason" as 'justify your best example'. I believe such responses are equally instructive with regard to my research interest.

- Alt. 1 -
Categories of Response

Category 1: Requires Qualifications/Special Skills (n = 17)

Responses in which the possession of qualifications was the preeminent criterion. Some pupils specified 'qualifications in science', others just 'qualifications' or 'more qualifications'. Qualifications were often linked to the perceived "complexity" of the job or the "intelligence" required to do it. In this category I also included responses in which "special skills" were required but in which their nature was not elaborated.

e.g. Class 2, r.18 The most usual reasons for deciding why a job is scientific rather than non-scientific is most of the scientific jobs need people with greater qualifications as the job seems more complicated.

e.g. Class 3, r.1 Because you have to have the qualifications to have a scientific job so you could be classed as a scientist.

e.g. Class 4, r.2 The reason I put those jobs in the first [= 'scientific'] column because you need science grades to get that sort of job.

Category 2: Tautologous (n = 11)

Responses in which a 'true definition' justification was advanced:

e.g. Class 2, r.5 Wherever or not science is yoused to do the job.
e.g. Class 4, r.5  My most usual reason for putting a job in the scientific column was because I though that the job was related to science in some ways.

Category 3: Mere Use of Sophisticated and/or Lab. Equipment (n = 8)
Responses in which the mere use of equipment was a criterion for classifying a job as 'scientific'. Responses in this category referred to sophisticated or complicated equipment (e.g. "high technology") or equipment which I judged to be normally - and stereotypically - associated with the conduct of science (e.g. "microscopes", "computers"). These responses discussed neither the way in which such equipment was used nor why it was required: mere involvement or use of it was enough.

e.g. Class 2, r.15  I think the most usual reason for a scientific job is in my opinion is whether or not the job involves high technology or using scientific implements.

e.g. Class 4, r.11  My most usual reason was to think of first, computers and technical equipment and think of which jobs go with them.

Category 4: Mere Use of Heat, Light, Sound, Electricity, Chemicals or Organic-Tissue (n = 7)
Responses in which the mere use of materials or physical properties normally associated with school-science was the preeminent criterion for scientificness. In these responses little or no attempt was made to elaborate the way in which they were used.
My usual reason is when chemistry, gases or electricity or biology are involved.

My usual reason is because it deals with electronics or needs special skills to do the job which involves science.

**Category 5: Complex or Difficult (n = 6)**

Responses in which scientific jobs were perceived to be complicated and/or difficult. This attribute was usually given within a compound answer. I suspect that 'complex', 'difficult' and 'requires mental activity' are used synonymously by most of these pupils. This category may therefore best be considered with category 6: 'Requires Mental Activity' (below). As with category 6, the opposite perceived characteristics (e.g. 'simple') were sometimes used by the pupil to characterize non-scientific jobs. The majority did not however, and in discussing these responses (sections 7.5.3. and 7.5.4). I had to make some assumptions as to these pupils' opposite poles. I believe that the potential dangers of such a practice were offset by triangulating a variety of sources of data.

I usually think of a scientific job as one needing more qualifications. The job always seem more complicated and usually the people in the scientific field become experts, like professors.

Science teacher, because you have to know all about science and it's very complicated.
Category 6: Requires Mental Activity (n = 6)

Responses in which scientificness was ascribed on the basis of the perceived amount and/or quality of mental activity entailed by the job. Little or no attempt was made to elaborate the nature of the mental activity stated. My general impression was that 'thinking', 'brainwork', etc, were used as synonyms for 'intelligence'.

e.g. Class 2, r.22 Scientific jobs need more brainwork than non-scientific jobs.

e.g. Class 3, r.12 Because they all involve brains and accurate measurement and all of that sort of thing.

Category 7: Related to School-Science (n = 6)

Responses in which scientificness was ascribed on the basis of a perceived similarity between, on the one hand, the job title and the activities perceived to be entailed by it, and, on the other hand, the same with respect to school-science subjects.

e.g. Class 2, r.4 If the job consists of a scientific subject that you know or about or have covered in a science lesson you will think that it is either a completely or partially scientific job.

e.g. Class 2, r.12 My reason for classifying the jobs the way I did was to see if we had done something similar in science lessons and what the job entails. Non scientific jobs are usually simple jobs or crafts.
Category 8: No Response (n = 2)

Category 9: Uncategorized Data (n = 6)

In 3 of these responses (R) pupils proposed criteria which, taken at face value, would seem incidental or irrelevant from the standpoint of any formal philosophy of science I have yet encountered.

R1 'Scientific = dependent upon practitioner appearance. Image mediated by mass-media

Class 2, r.1 Newspapers, television and the media influences us. When you hear the term physicist used. You immediately think of a scientist, with a white coat and glasses. Where if you hear the term 'milkman' you think of a man in uniform delivering milk. A rubbish collector you expect to see a grubby man with a dustbin on his back.

R2 'Scientific' = extraordinary activity (see 3 below)

Class 2, r.8 It depends on doing things which aren't part of everyday life.

R3 'Scientific' = a linguistic convention derived from similarities between ending of titles

Class 2, r.13 Most scientific jobs have an ending that is usually the same whereas normal jobs mostly sound different.
2 pupils claimed that scientific jobs were important:

e.g. Class 3, r.10  My usual one is a nurse. Because it is important.

The remaining pupil expressed a 'mixed bag' of largely tantalogous descriptions:

Class 4, r.4  Science teacher teaches us biology and chemistry physics they are scientific computer programmer has to know the machine operation and his numbers and things that are on the computer. Astronaut knows about space: we learn astrology in science lessons.
Appendix 13. Written Exercise, Q4: Categories and Interpretation of Responses

A13(a). WE, Q4: Categories of Response

A13(b). WE, Q4: Interpretation of Responses
Q4. Have you found it useful to answer Q1?
   If 'yes', why?
   If 'no', why?

I only included this question in the final version of the CE (n = 33).

My purpose for this question was to find out what, if anything, the students perceived themselves to have learnt from having answered Q1. I was particularly interested to find out if students considered that answering Q1 had helped them to clarify or to develop their thoughts concerning the conduct of science.

My reasons for including this item are intimately related to both my meta-methodological conviction (discussed in Ch. 5) that any investigation of a person's ideational world necessarily constitutes an intervention of some sort and to my complementary pedagogical conviction that teaching and learning should be construed as collaborative research. This item, then, was included to help appraise the pedagogic utility of students answering Q1.

I verbally introduced this question by saying something similar to the following:

"I am very grateful to you for answering these questions but do you, personally feel that you benefitted in any way by answering Q1? If you feel that it was personally useful try ot say why. On the other had, you may have found it a waste of
time answering Q1. If that was the case, feel free to say so and, if you can, also say why."

I grouped response categories under superordinate categories of either 'yes' (n = 12) or 'no' (n = 20). There were no "compound" answers given to this item.

Table: Showing WE, Q4: Categories of Responses, the Frequency of Responses within them and their Distribution between WE Administrations

<table>
<thead>
<tr>
<th>Category of Response</th>
<th>Class 3</th>
<th>Class 4</th>
<th>Freq</th>
</tr>
</thead>
<tbody>
<tr>
<td>&quot;Yes&quot;</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Career Guidance</td>
<td>3,5,14</td>
<td>4,5,15</td>
<td>6</td>
</tr>
<tr>
<td>Problem Solving</td>
<td>6</td>
<td>10,11,14</td>
<td>4</td>
</tr>
<tr>
<td>Helps Researcher</td>
<td>4,15</td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>&quot;No&quot;</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Common Sense</td>
<td>9,13</td>
<td>1,2,6,7,9,12</td>
<td>8</td>
</tr>
<tr>
<td>Irrelevant</td>
<td>7,8,12,16</td>
<td>8,13,16,17</td>
<td>8</td>
</tr>
<tr>
<td>Too Difficult</td>
<td>1,2,10</td>
<td></td>
<td>3</td>
</tr>
<tr>
<td>Uncategorized Data</td>
<td></td>
<td>3</td>
<td>1</td>
</tr>
<tr>
<td>No Response</td>
<td>11</td>
<td></td>
<td>1</td>
</tr>
</tbody>
</table>
Categories of Response

A. "Yes"

Category 1: [Yes] Career Guidance (n = 6)

Responses in which the pupil claimed that his experience of having answered Q1 would inform his choice of job when he left school. I feel it is significant that no pupil claimed that answering Q1 had helped raise his awareness of his views on the nature of science. My impression, drawn from all sources of data in this study, is that each of these pupils assumed his conception of science to be the commonsense one. Working from that premise they them perceived the benefit of the exercise to have been autodidactic - it had facilitated raising their self-awareness of the scientific status of different jobs.

e.g. Class 3, r.3 Yes, because it helps you to see what certain jobs are.

e.g. Class 4, r.4 I have found it useful because it can help me decide what job I would like to have.

Category 2: [Yes] Problem-Solving (n = 4)

Responses in which pupils recognized that although in everyday life they may have unproblematically ascribed the labels 'scientific' etc, the degree of self-consciousness brought to this practice by the question made them question the adequacy of their demarcation criteria (conscious or unconscious). The benefit which these pupils thought they had gained is difficult to ascertain. I feel that most regarded it as an exercise in problem-solving, the benefits of which were independent of any subject such as 'the nature of scientific activity'.
It is useful to answer Q1 because it is not as easy as it sounds and makes you think.

I have found it useful because when you look at the question it looks easy but when you try to do it, it takes a lot more thinking. I think it must be useful for the examiner.

Examinations? Researcher?

Category 3: [Yes] Helps Researcher (n = 2)

Responses in which the pupil saw their personal benefit from having answered Q1 purely in terms of having helped me (i.e. the researcher).

Class 3, r.4 Yes because it helps you to see that we think about the central thing.

Class 3, r.15 Yes, because it helps you to see what we think about certain jobs.

B. "No"

Category 4: [No] Common-Sense (n = 8)

Responses in which the nature of science was assumed to be commonsense and so the exercise was not perceived to be personally useful. Several of these pupils added that the exercise was boring because of this. Responses in this, the largest of CE Q4 categories, demonstrate the prevalence of the assumptions that the nature of science is not an issue. I believe that this assumption underlies responses in category 1 ([Yes] Career Guidance), above, and category 5 ([No] Irrelevant), below.
e.g. Class 3, r.9  No, because I havent found anything out that I
didnt already know.

e.g. Class 4, r.12  Not really! I dont think that setting out jobs
on a table like that is any good. If the
question was to find out if we knew what jobs
involve science - well most people have a good
idea anyway!!

Category 5: [No] Irrelevant (n = 8)

Responses in which pupils caimed not to have derived any benefit
from having answered Q1 because it was irrelevant. I suspect that
since the choice of occupations and the number of examples was left
entirely to the individual pupil, each pupil in this category was
able to draw unproblematically from his personal history and common
lore. The perceived absence of any difficulty perhaps made the
exercise seem futile.

e.g. Class 3, r.7  No I think there's no point in this.

e.g. Class 4, r.8  No I havent found it useful to answer question
Q1 cos it's not gonna help me for anything.

One pupil appeared to view the exercise as a failed attempt at
career guidance:

Class 3, r.12  No because you dont really need to know if a
job scientific or not to applie for it.
Category 6: [No] Too Difficult (n = 3)

Responses in which pupils perceived Q1 to be too difficult for them to gain any personal benefit from attempting to answer it. In my verbal introduction of the DCE to Class 3 I have claimed that 'some of the questions in the exercise that you are about to do have baffled philosophers and scientists for hundreds of years and they still disagree amongst themselves today.' I said this in an attempt to add further credibility to my other expressed, and genuinely felt, claims as to the non-normative, exploratory nature of the exercise. My additional remark to this class clearly 'backfired' with Class 3 r.l (below) and possibly with the others in this category. Pupils appear to be suspicious to the point of disbelief when a relativist approach is adopted. I see this as an indictment of our education system.

e.g. Class 3, r.1 No because if you have intelligent people working on the question I can't see how I can help!

e.g. Class 3, r.10 No because it is hard to think what to put in the scientific column and even hard to put.

Category 7: [No] Uncategorized Date (n = 1)

This pupil claimed that answering Q1 had not been personally useful but he failed to elaborate his answer in a way in which I could draw conclusions.

Class 4, r.3 No, because you might not what

Category 8: No Response (n = 1)
A13 (b): WE, Q4: Interpretation of Responses

'Have you found it useful to answer Q1?
If yes, why? If no, Why?'

The two most frequent types of answers in which pupils claimed not to have derived any personal benefit from having answered WE Q1 apparently because it was either 'commonsense' or 'irrelevant':

Q4; Category (c)4: [No] Commonsense

e.g. Class 4, r.12 Not Really!, I dont think that setting out jobs on a table like that is any good. If the question was to find out if we knew what jobs involve science - well most people already have a good idea anyway!!

Q4; (c)5: [No] Irrelevant

e.g. Class 4, r.8 No I havent found it useful to answer question Q1 cos its not gonna help me for anything.

Amongst the minority that did claim benefit, the most frequent type of benefit claimed was seen in terms of vocational guidance:

Q4; (c)1: [Yes] Career Guidance

e.g. Class 4, r.4 I have found it useful because it can help me decide what job I would like to have.
My impression, corroborated by many sources of data in this study, is that each of these pupils assumed his conception of 'scientific' to be the commonsense one. Working from that premise they then perceived the benefit of the exercise to have been auto-didactic only in the sense that it had raised their self-awareness of the presence or absence of science in different jobs.

The 'Career Guidance' category of response also featured prominently in answers to CE Q2. However, the most frequent non-tantalological type of answer to Q2 suggested that pupils regarded Q1 as some kind of assessment of their personal knowledge:

Q2; (c) 2: Knowledge Test/Measurement

e.g. Class 3, r.4 I think you have asked us this Q because to see if we are right or wrong if they are scientific etc.

I suggest that this category of response indicates that pupils receive the meaning of 'scientific' to be, in some way, know for sure.

All the categories of response I have cited so far sidestep a consideration of the meaning of 'scientific'. I conjecture that this is because pupils did not regard the meaning of 'scientific' to be an issue. Each pupil appeared to assume that he not only personally understood the meaning of the term but also that his personal meaning matched the 'true' and accepted meaning. Consequently, for most pupils answering Q1 was a simple exercise in cataloging personal benefits, if any, had to come from elsewhere.
(e.g. vocational guidance). In Q4, only 3 of the 33 pupils who answered the question claimed that they had not derived any personal benefit from having answered WE, Q1 because it was "Too Difficult" (Q4, c. 6).
Appendix 14. Alphabetical List of Occupations depicted within an attempted Interview-about-Instances Card Deck for 'Scientific Occupation'
1 Accountant
2 Archeologist
3 Artist
4 Biologist
5 Biology teacher
6 Chemist
7 Chemistry teacher
8 Computer Operator
9 Computer Programmer
10 Doctor
11 Geologist
12 Historian
13 Laboratory Technician
14 Librarian
15 Machine Operator
16 Mathematician
17 Nurse
18 Ornithologist
19 Physicist
20 Physics Teacher
21 Pilot
22 Psychiatrist
23 Train Driver
24 Typist
Appendix 15. WE, Q7: Categories of Response
<table>
<thead>
<tr>
<th>No</th>
<th>Category of Response</th>
<th>Class 1</th>
<th>Class 2</th>
<th>Class 3</th>
<th>Class 4</th>
<th>Freq</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Mere use of/Involvement with Physical Phenomena</td>
<td>6,14,20,21</td>
<td>1,5,9,20,21</td>
<td>6,14,16</td>
<td>6,8,12,13</td>
<td>16</td>
</tr>
<tr>
<td>2</td>
<td>Related to School-Science</td>
<td>3,4,6,8,12,17,19</td>
<td>4,6,16,18,24</td>
<td></td>
<td>7,13</td>
<td>14</td>
</tr>
<tr>
<td>3</td>
<td>Involved Principle</td>
<td>15</td>
<td>8,10,11,12,13</td>
<td>2,10</td>
<td>10,17</td>
<td>10</td>
</tr>
<tr>
<td>4</td>
<td>Mere use of Equip. Invented by Sc.ists</td>
<td>7,13,20,22</td>
<td>3,4</td>
<td></td>
<td></td>
<td>6</td>
</tr>
<tr>
<td>5</td>
<td>Complex/Inexpicable</td>
<td></td>
<td>7</td>
<td>1,4,5,11,15</td>
<td></td>
<td>6</td>
</tr>
<tr>
<td>6</td>
<td>Tantologous</td>
<td>1,7,1,8,14,15</td>
<td></td>
<td></td>
<td></td>
<td>6</td>
</tr>
<tr>
<td>7</td>
<td>Required Cognitive Skills</td>
<td>1,2,5,2</td>
<td>3</td>
<td></td>
<td></td>
<td>5</td>
</tr>
<tr>
<td>8</td>
<td>Required Personal Qualities</td>
<td>10</td>
<td>15</td>
<td>3,15</td>
<td></td>
<td>4</td>
</tr>
<tr>
<td>9</td>
<td>Involved Writing</td>
<td>8,18</td>
<td></td>
<td></td>
<td></td>
<td>2</td>
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<tr>
<td>10</td>
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<td>3</td>
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<td>11</td>
<td>Uncategorized Data</td>
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<td></td>
<td></td>
<td></td>
<td>7</td>
</tr>
</tbody>
</table>

Table: Showing WE, Q7 Categories of Response, the Frequency of Responses within them and their Distribution between WE Administrations

- A15.1 -
Categories of Response

Category 1: Mere Use of/Involvement with Physical Phenomena (n = 16)

Responses in which unelaborated involvement with physical properties, such as heat, light, sound, electricity, chemicals, or organic tissue was the criterion by which scientificness was ascribed to the incident. In these responses little or no attempt was made to elaborate the way in which they are used.

Class 4, r.12

Q4: I'm pulling in and switching on a radio/tape recorder.

Q4: This is scientific because electricity and sound is involved along with electronics.

Class 2, r.9

I am fixing my record player. Because it is working with electricity.
Category 2: Related to School-Science (n = 14)

Responses in which a claim was made that the scientific activity had either been conducted in a school science lesson or else bore a resemblance to a school science activity. It is likely that the frequency of responses in this category is slightly inflated due to my wording in the first version of WE.1: I did not specify that the scientific incident must have occurred outside school.

I regard these responses as a form of tantalogous response (see Category 5, below).

It must, however, also be noted that the activities described typically involved the "mere" use of physical properties or organic tissue as per category 1, above.
I am mending a plug by putting different wires into little slots and pushing the plug together.

I think this is scientific because I have done something like it in a science lesson.

Me doing an experiment at school.

* I think it is a scientific activity because it was a scientific experiment at school.
Category 3: Involves Principle (n = 10)

Responses in which the scientific activity used or demonstrate a principle.

Three pupils described a physical law or effect such as might have been encountered in school-science e.g. Class 2, r.10 (see below). Another pupil, Class 2, r.11 (see below), drew a person falling from a tree and advanced the following common-lore principle by way of justification "Because I was demonstrating that what comes up must come down".

Two respondents justified the scientificness of their incidents by claiming that it showed how something worked e.g. Class 2, r.8 (see below). 2 more pupils identified finding out about something as their criterion.

One pupil claimed the principle of 'survival' to be his criterion, another 'invention'. 
Class 2, r.8

Cutting a trout.
Because I could see what was in a fish and how it worked.
Category 4: Mere Use of Equipment Invented by Scientists (n = 6)

Incidents in which the demarcation criterion was that the equipment used had itself been invented or used by scientists. I included responses in which the criterion was use of a specific piece of apparatus without reference to science or scientists but in which the apparatus concerned was of the sort which I judged to be available in a school-science lab. (e.g. a microscope, see Class 2, r.14, below).

Class 1, r.13

Class 1, r.22
Category 5: Complicated/Inexplicable (n = 6)

Responses in which the criterion appeared to be activities which the pupil found complicated or difficult. I also included responses where I judged that the pupil had included an incident because it was inexplicable for him e.g. Class 4, r. 15, below. Without exception the incidents cited by pupil responses in this category referred to physical events – affective issues were conspicuously absent.

Class 4, r.1

Class 4, r.11
Category 6: Tantalogous (N = 6)

Responses in which the incident was declared scientific by fiat. Most of these responses may be regarded as a sub-set within category 1 ('Mere use of/Involvement with Physical Phenomena'), since the incidents cited described the mere use of or involvement with heat, light etc. Within this category I also included responses in which an occupation e.g. meteorology was declared scientific without a rationale.
Class 3, r.1

23. Because I drew it as a human head.

27. Because it is scientific.

Class 3, r.11

Q.5. Drawing a plug

Q.7. Because it is scientific.

Class 3, r.4

Q.6. Nadie a T.V.

Q.7. Nadie a T.V. is scientific because we use it.

Class 4, r.14

Q.5. Nadie a T.V.

Q.7. Nadie a T.V. is scientific because we use it.

Q.8. Nadie a T.V. is scientific because we use it.

Q.9. Nadie a T.V. is scientific because we use it.

Q.10. Nadie a T.V. is scientific because we use it.
Category 7: Required Cognitive Skills (n = 5)

Incidents which had required intelligence (e.g. "brains") or mental operations (e.g. comparison, logic). Most of the incidents in which such cognitive skills were applied described involvement with heat, light etc.

Class 2, r.2

Class 1, r.2
Category 8: Required Personal Qualities (n = 4)

Incidents which required personal qualities such as endurance, patience or initiative. The incidents in which such qualities were applied all described involvement with heat, light etc.
Category 9: Involved Writing (n = 2)

Incidents in which the pupil was writing. Each pupil justified the scientificness of writing by reference to their perception that writing was the preeminent or sole activity within school-science lessons.

The responses within this category may be regarded as contributing a sub-category within category 2 ("Related to School-Science, see pp 40-41 above). This notwithstanding, it must be remembered that in the first version of WE I did not specify that the scientific activity had to have occurred outside school, (both responses in this category came from Class 1).

Category 10: No Response (n = 3)
Two pupils gave no incident but offered an "explanation".

I found these responses saddening. Class 3, r.11 offered no explanation except that he couldn't draw. Class 3, r.12 comment was written on a page full of a series of erased drawings of incidents which he had rejected.

A further five pupils described an incident (i.e. drawing and/or description) but were unable to explain why the incident was scientific. These incidents typically involved physical properties (i.e. as per category 1) or equivalent invented/used by scientists (i.e. as per category 4). I felt, however, that my chain of inference would be overstretched if I classified these responses
under those categories unless I had supporting evidence from interviews with the individuals.

Finally, one pupil from the same WE class (i.e. Class 1) as those included in category 9 ("Involved Writing", above) and who shared the same science teacher, also perceived writing to be the dominant activity in her science lessons ("she just makes us copy from books"). However, the moral he drew from this perception was different from those of his classmates in his view writing was "not scientific" and os he "started talking". I was not able to find out whether this pupil regarded "talking" as a scientific activity. I take this illuminative incident as an indication that the advances in the practice of science-teaching since the 1960s have been more apparent than real - at least with respect to the class specified:

Class 1, r.11

+ Name altered to preserve anonymity
Appendix 16. Written Exercise Q8, Q8(a) and Q8(b): Categories of Response and Interpretation of Responses

A16(a). WE. Q8, Q8(a): Categories of Response

A16(b). WE. Q8, Q8(a): Interpretation of Responses

A16(c). WE. Q8(b): Categories of Response

A16(d). WE. Q8(b): Interpretation of Responses
### A16 (a) WE, Q8, 8(a): Categories of Response

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<thead>
<tr>
<th>No</th>
<th>Category of Response</th>
<th>Class 1</th>
<th>Class 2</th>
<th>Class 3</th>
<th>Class 4</th>
<th>Freq</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Sophisticated</td>
<td>1, 2, 4, 5, 6, 7, 8, 9, 11, 12, 13, 14, 15, 18, 19, 24</td>
<td>3, 4, 13, 16, 18, 19, 123, 24</td>
<td>7, 8, 10, 14</td>
<td>3, 5, 12, 13, 15</td>
<td>34</td>
</tr>
<tr>
<td>2</td>
<td>Naive</td>
<td>3, 16, 17, 22</td>
<td>1, 2, 6, 7, 8, 9, 10, 11, 3</td>
<td>1, 6, 7, 8, 9, 10, 11, 17</td>
<td></td>
<td>31</td>
</tr>
<tr>
<td>3</td>
<td>Sophisticated ?</td>
<td></td>
<td>12, 15, 21</td>
<td>1, 3, 15, 16</td>
<td>4, 14</td>
<td>9</td>
</tr>
<tr>
<td>4</td>
<td>No Response</td>
<td>10, 20</td>
<td>5, 17, 22</td>
<td></td>
<td>2</td>
<td>6</td>
</tr>
<tr>
<td>5</td>
<td>Uncategorized Data</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0</td>
</tr>
</tbody>
</table>
A16 (b) WE, Q8, 8(a) Interpretation of Responses

Category 1: Sophisticated Responses - "What to Observe"? (n = 34)
Responses in which I judged that the pupil had shown at least some recognition of the basic flow in the question viz. the unrestricted scope for observation rendered the question unanswerable in either practice or principle:

e.g. Class 2, r.18 I think the question is silly because anyone could observe hundreds of different things.

e.g. Class 4, r.12 The difficulty is that you have said Carefully Observe, observe what?!! This is a badly laid out question no one could give you a decent answer!! You have not told us what to observe.

Category 2: Naive Responses (n = 31)
Responses in which pupils straightforwardly described idiosyncratically chosen phenomena:

e.g. Class 2, r.9 Chalk dust on board, people moving around, Tony scratching his armpit.

e.g. Class 2, r.20 I have observed the fish swimming in the tank.

Category 3: Sophisticated? Responses
Responses which I felt unwilling to categorize as 'naive' but whose 'sophistication', as earlier defined, was either in doubt or inapplicable. These responses were themselves of 3 types:
'Sophisticated (1)?' (n = 4)
Responses in which the pupil claimed, without elaboration, to have been able to answer Q8 because he had not understood it:

e.g. Class 3, r.3 I have not been able to answer Q8 because I do not understand it.

That pupils who gave such responses may yet have understood the flow in the question but may not have made their understanding clear in their written answer as supported by two of my structured interviews FI(p')5, FI(p')6.

'Sophisticated (2)?' (n = 2)
Responses in which the pupil claimed to be unable to answer Q8 because the question was, in some way which they did not elaborate, inadequately expressed.

Class 2, r.15 I have observed the chalk on a blackboard and a thick question.

Class 2, r.21 I have observed the question is not explained properly.

'Sophisticated (3)?' (n = 2)
Responses in which the pupil claimed Q8 to be a 'trick' question through being self-referential.
Class 2, r.12
The blackboard with the writing, carefully observe, and write down what you observe, written on it, with Mr. Swift walking in front of it grinning and thinking that he's got us all fooled!

Class 4, r.4
It is a trick question, you write down the question.

Although only these two pupils expressed a belief that the question was self-referential, 17 others chose to observe the question (,) in some way. With these, however, I had felt obliged to classify them as 'naive' despite a possibility that some pupils may have privately regarded the question as a trick question in the sense elaborated above.

e.g. Class 2, r.10
I observed the question: carefully observe the write down what you observed?

e.g. Class 4, r.8
I have observed questions about scientific, non-scientific and both jobs.

Category 4: No Response (n = 6)

Category 5: Uncategorized Data (n = 0)
### Table: Showing WE, Q8 (b): Categories of Response, the Frequency of Responses within them and their Distribution between WE Administration

<table>
<thead>
<tr>
<th>No</th>
<th>Category of Response</th>
<th>Class 1</th>
<th>Class 2</th>
<th>Class 3</th>
<th>Class 4</th>
<th>Freq</th>
</tr>
</thead>
<tbody>
<tr>
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<td>Unelaborated</td>
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<td></td>
<td></td>
<td></td>
<td>15</td>
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<tr>
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<td>6,13</td>
<td></td>
<td>10</td>
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<td>3</td>
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<tr>
<td>3</td>
<td>Need for Instructions</td>
<td></td>
<td>14</td>
<td>4,5</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>Should Observe More</td>
<td></td>
<td>15</td>
<td>12,12</td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>5</td>
<td>No Response [but answered 8a]</td>
<td>2,3,4,6,7,9,10,11,12,16,17,14,21,22</td>
<td>2,4,6,9,13,15,16,14</td>
<td>2,6,7,9,14,15,16,14</td>
<td>1,3,4,5,49,10,11,13</td>
<td>6,7,8,9,6,7,8,9</td>
</tr>
<tr>
<td>6</td>
<td>No Response [8a &amp; b]</td>
<td>10,20</td>
<td>5,17,22</td>
<td></td>
<td>2</td>
<td>6</td>
</tr>
<tr>
<td>7</td>
<td>Uncategorized Data</td>
<td></td>
<td></td>
<td></td>
<td>16</td>
<td>1</td>
</tr>
</tbody>
</table>

**Figure:** Appendix 16 (c) WE, Q8 (b): Categories of Response
Q8 (b) Categories of Responses

Category 1: [No] Unelaborated (n = 15)
Unelaborated negative responses

e.g. Class 1, r.1 No.

Category 2: [No] Irrelevant to Science (n = 3)
Elaborate negative responses. These pupils claimed that their experience with answering Q8 (A) told them nothing about novel experiments or science. It is not clear whether these pupils felt they had benefitted in any other way.

e.g Class 1, r. 13 No, its nothing to do with science.

Category 3: Yes [Need for Instructions] (n = 3)
Responses in which pupils drew attention to the need for clear instructions. I believe that the possibility implied by these responses i.e. instructions received rather than created, reflects an important aspect of the present nature of school science.

e.g. Class 3, r.14 Yes it tell me that when doing experiments you should have full instructions.

[Answer to Q8 (a) = 'Sophisticated'].

e.g. Class 4, r.4 He might not be clear explaining what to be done.

[Answer to Q8 (a) = 'Sophisticated (3)?'].

Category 4: Yes [Should Observe More] (n = 2)

Responses in which pupils concluded that experimentalist should observe more.

e.g. Class 3, r.15 Yes you should observe more and try to understand.

[Answer to Q8 (a) = 'Sophisticated'].

e.g. Class 4, r.12 Scientists have to be aware and observe anything new all around.

If anything these responses tend to the opposite of the epistemological moral that I wished them to draw.

Category 5: No Response neither Q8 (a) nor Q8 (b) (n = 6)

Category 7: Uncategorized Data (n = 1)

Class 4, r.16 In science you are looking for reactions that are obvious, e.g. change of colour, chemical state, heat etc.

[Answer to Q8 (a) = 'Sophisticated'].

- A16.7 -
The meaning of the pupils' meaning of the work "obvious" is not clear. On the one hand he could mean 'obvious' in the sense of 'constrained by the experimental design'. On the other hand he could mean obvious in the sense of 'changes which are so apparent that they cannot be missed'.

I suspect the latter interpretation.
Appendix 17. Sample from Example Transcript of Focussed Interview
with Student: Andrew, FI(St)3
I 001  Right... well what I'd like to do Andrew to start off by asking you whether you've ever heard of the word unscientific

A 003  Yeah

I 004  Mm?

A 004  Er ... well you get things like . er . if your approach to sort of analysing something they can say you haven't done it right you say you haven't used scientific method its done in an unscientific manner .. its usually ..I heard it a lot about things like . er . sort of arty science type things where they come out with great fantastic results and the scientists say rubbish because you haven't ... its unscientific the way you've done the experiment or unscientific viewpoint to take

I 009  hmmm

A 009  So you in that respect I've heard it a lot

I 010  So would I be right in saying from what you've said so far that you've heard this expression .. used mainly by scientists ..

A 011  hmmm

I 011  inverted-commas scientists your phrase scientists .. er .. applied to arts

A 013  well applied to -

I 013  academics or whatever?

A 013  well applied to .. within .. to other scientists and .. and on a lot of occasions to the sort of .. er .. like the psychology people and things like that which is a sort of never-never land between science and art. It can't seem to make up its mind which its going to be and I think most scientists say its not a science and most artists say its not an art and .. it seems you know to get done over by both of them really.

I 018  ...I see .. hm .. what do you think they mean by unscientific?

A 018  ...hm ... well I suppose its like any subject there are sort of rules about how you're supposed to go about things and .. er .. say for an experiment there are certain criteria you've got to have if you haven't got them then you may be using a correct method but a lot of people may consider it unscientific .. the way of going about things and .. er .. in that respect really ..
I 023 So .. I would be right in saying that in your view unscientific is used in a sort of a negative way ... that they haven't used or .. rules .. hm .. what was the other thing you said ... correct method ..

A 026 hm

I 026 They haven't used these things rather that they have used some other .

A 027 Well I think unscientific I mean its either non-scientific which it describes things like don't even try to be scientific like approach things in a completely different way .. I mean .. you either if you've got a thing you either approach it in a sort of philosophical sort of manner or you try to approach it in a scientific which is either non-scientific or its scientific and if its done not the way the institutions and everything would like to do it it'll be considered unscientific.

I 033 ... I see. So ... so from what you've said so far .. er .. unscientific is like a failed attempt to be scientific

A 034 yes

I 035 Scientist's view usually

A 035 Yeah I'd say usually like that yeah.

I 036 And you went on to say that non-scientific is where the approach doesn't even pretend to be scientific and the non-scientific view you said tends to be philosophical?

A 038 Well .. as .. as a sort of example philosophical perhaps. You can I mean you can approach a problem from a number of angles ...

I 040 Yeah

A 040 and one you know its like ... if you see a flower then you can go and dissect it scientifically say exactly what it is or you can approach it from a philosophical point of view and say this flower is beautiful sort of thing and I mean both of you are just trying to describe a flower one's describing it scientifically and one's describing it from a more sort of philosophical sort of way it's two ways of approaching the same thing.

I 044 Ah ... Do you think, do you think Andrew you could sort of elaborate a bit more what you mean by scientific? You've just given an example of analysing a flower.

A 047 I think science tends to ... the whole basis of what it tries to do is ... you ... it sees a problem or .. or .. you know .. or a thing it wants to analyse and it sort of .. poses sort of different questions from the unscientific in that .. er ... I've
had this sort of talk before ... now which .. er .. was it .. er.

I 050 Who did you have this talk with?

A 051 Er . well I've had these sort of talks with .. er .. religious people who're always going around saying because you're a scientist you're unreligious and .. er .. you see they say they don't coincide because religion, let's say religion and philosophy say it is there and you know why isn't it great you know why is it here and .. er .. science will say you know how it happens so they'll see a problem they'll say this is how it occurs this is how we progress whereas philosophers and religion .. er .. from that sort of point of view will say why is it here and why does it do this.

I 058 I see . I see ... er .. you ... you described the religious and philosophical type of approach you described the scientific type of approach er ... I'm not clear whether that's ... which ... or both you align yourself with.

A 061 I align myself with both of them because as I say I consider it two different ways of viewing things. I think science tends to say how something is ... and break everything down into ... little chunks smaller and smaller blocks so that you give a ... a ... thorough view of it but still once you've actually got an answer you're not .. you can't really say that's the full answer in effect I suppose you've got to say why is it here and from a scientific point of view you can try and go so far as to say why its here but even then you'd have to sort of tend to a philosophical point of view .. I think in that way you can go farther because I think the scientific method restricts you in that you've got to have proof for everything whereas philosophically you can perhaps have just as much proof but in a ... in a sort of philosophical sort of manner you can just argue things out with very little proof providing your arguing is correct then you've got a sort of conclusion ... with science I don't think you can do that you can argue through but even then its only a hypothesis till you've actually gone out and proved it, which in a lot of things perhaps you can't ... you can never do, so ...

I 073 hhmh (agreeing) so .. would I be correct in saying from what you've said so far that ... er ... that there are different ways of answering as it were the same question or the same problem ... there can be the philosophical way there can be the scientific way ... er ... both are different ways but ... and here's the bit I'm not quite sure about ... you're not saying one is in any absolute sense better than another.

A 078 no that's always the big thing the two are always sort of against each other ... I don't look at it from that point of view at all. I would say that the two together you get a much fuller picture of the whole idea and that way they ... they do contradict in a
way but then providing you bring them both together and then look at it as an individual you can get a much fuller picture. People are always sort of saying you know always fighting against each other scientists against non-scientists I think that's where a lot of sort of problems come into being really, neither of them trusts each other and both consider each other idiots yet together they've probably got a great deal more knowledge if they worked together. One one of the things is experimental psychology ...

I 086 yeah

A 086 I mean that's one of the things where the psychologist's considered art and science, the pure scientist looking at the same problem and the psychologist looking at exactly the same problem and by themselves none of them will sort of get anywhere really but together they can go an awfully long way which is I think they're trying to do it if in Sussex they're trying to do that ...

I 091 You think that they've actually set up certain departments --

A 091 -- yes yeah they do seem to have --

I 091 -- experimental psychology, cognitive studies ... so there's a few to start off which --

A 093 -- which brings the arts and the science together, and in that way sort of perhaps because I'm looking at it from a different angle they'll get farther together.

I 094 and is there a sort of dialogue between the different departments or does the dialogue between the two approaches as it were get conducted within the particular departments we're talking about?

A 097 I don't know an awful lot about it but there seems to be a certain amount at least of er cooperation in jargon and everything where its now set as a subject which they're both approaching and you know they're now no longer a psychologist they're now experimental psychologist and you're no longer a biologist, you're now an experimental psychologist even though you may be looking at it from two different angles and you're then grouped as an experimental psychologist and it therefore has its own sort of jargon. It seems to me to be the way things are going. I don't know a great deal about it but that's how it struck me.

I 103 right hm could I just pick you up on something you mentioned a little earlier scientific method you you said something about scientific method could you elaborate and tell me a little more about what your personal meaning of scientific method is?
A 107  hm ... I always get it the way there are certain ... if you're going out to ... you decide what problem you've got and then you're going to try and prove it and there always seems to there are certain rules and regulations that you've got to sort of go through ... you're experimenting in order to be proved scientifically --

I 112  -- aha --

A 112  -- and things like .. you know .. when you're in the fifth form and everything they always say you've got to use a control for your experiments and you may know what ... you may have such a good idea that you may think its not worth having a control because its going to happen anyway but you know if you don't use it then you've been unscientific because you haven't used the scientific method. That's the way it's always struck me.

I 116  hm ... now when you say the scientific method .. hm .. do all scientific methods ... er ... does the scientific method always use a control ... hm ... in its research?

A 119  some of them ... of one sort or another ... yes hm ... you know its sort of scientific method the big thing .. it sort of has to evolve for each sort of specific ... as each subject becomes more specialised its sort of method has to evolve a little bit ... I should have thought as a whole you know that applies to all of them

I 123  hm . and perhaps .. could you just tease out a few more details about this ... what the characteristics of this method that applies to .. to all the approaches are? er .. I realise within physics chemistry and biology experimental psychology whatever there are specific techniques if you like --

A 128  hm . (agreeing)

I 133  -- that you know are very highly related to their .. er .. interests .. be they experimental psychology physics chemistry or biology ... but is there anything that's common in the scientific method ... er ... between all of those things?

A 133  (pause) there is ... er ... the way I mean the whole thing is you've got to prove something and therefore scientific method you have to prove something usually by actually going out and doing it say you know whereas ... er ... from an arts point of view you can prove it by writing a paper on it and not actually doing anything but by your thoughts sort of process your thoughts sort of saying this is what it is but in science you've got to go out and actually do it and then generally it it has to be sort of verified and be experimented .. and unless the experiment is performed in a sort of correct and proper manner as .. you know .. other people looking ... scientists looking at it saying this is ... this is done wrong or he should have put this in in order
to be absolutely certain ...

A 142

A 142 and so you know that tends to be the sort of scientific point of view ... I suppose you've got to ... the experiment's got to be foolproof ... you've got to actually cover for every eventual possibility ... you've got to ... this is the thing ... that ... scientific method ... what its trying to do when you come up with a sort of hypothesis about why something happens you can be sort of misled by ... because you're so sure that it is then you can direct your experiment just to prove that you're right even though it might not actually prove that what you've actually proved may not be right ...

I 148

A 148 and if the scientific method says O.K. you've got a hypothesis now ignore it test it ... also test it to see if you're wrong ... I think that should be the whole point of it really ... you can be led away by the fact that I am right and therefore this experiment is going to prove it one way or another and ... you ... sort of disregard certain results say or --

I 152

A 152 -- you know this sort of thing ... I mean this happens quite a lot when scientists have disregarded a freak result which they regard as experimental error and gone on and in the end its been repeated and it wasn't an experimental error at all ... but sort of fault in his hypothesis ... the whole idea was wrong or slightly wrong but he refused it and I think this scientific method tries ... er ... tries to remove that sort of human error.

I 157

I 157 aha so scientific method is about trying to remove human error --

A 159

A 159 -- as in possibly ambition or whatever ... you gave the example of somebody ignoring a freak result ... now I'm just trying to again tease out things which are the characteristics of this method ... hm ... you said that you've got to try and prove it and that that was generally seen in terms of actually going out and doing it.

A 163

A 163 hm ... I think its got ... er ... that's ... er ... that's the sort of thing that scientists think that you've got to actually go out and do it ...

I 165

I 165 hmmm (encouraging)

A 165 .. because otherwise ... er ..

I 165 and what does actually going out and doing it ... er ...
I think it's one of those things... seeing is believing... you've got to try and reconstruct... reconstruct... I mean it's like you know the big bang... er... how did the world begin and everything... how did you get all those organic things... --

-- yeah

well you can put as many hypotheses of it as you like but nobody's going to believe it until you go out and try to reconstruct it... reconstruct the conditions and then go out and once people see that then they tend to sort of take it a little bit more seriously your point of view... it tends to be scientists... for scientists its seeing is believing... you can write down as much as you like but unless you actually go out and do it experimentally or at least set about proving it nobody'll believe a word you say

I see... er... (pause) a couple of things... er... hm... first of all would I be right in saying that... er... that... you start with a hypothesis... that's where you start...

start with your problem heah...

start with a problem --

some form of hypothesis yeah --

-- form a hypothesis that... how that relates to your problem...

hm (agreeing) and then --

sorry... how is it related to your problem? hypothesis?

well you've got the problem you know how does x go to y and your hypothesis is x goes to y via n... and then the next stage is to go out and prove it...

yeah

and then you... if you prove it... if you can't prove it conclusively then I suppose it becomes a theory because it hadn't actually been disproved but nobody's actually proved it conclusively... and if it's proved conclusively beyond a shadow of doubt the it becomes a law although even laws can in effect quite often just be theories...

well I was just going to ask you that... er... what actually constitutes conclusive proof?

er (pause) I think conclusive proof tends to... I mean it should be without a shadow of a doubt in no possible way can this can it be anything else but this... it tends to be a sort of... if
proved enough for me to believe that this is right and do it
the general populous of scientists to then believe that this is
right..

I 193 aha .. aha

A 193 -- and tends to go on like that but I mean I think there's been
quite a few old laws that have been sort of changed and altered
or sometimes perhaps turned round completely..

I 195 yeah .. yeah

A 195 but they've been laws because everyone believed them they'd then
be proved in certain ways and you know there was perhaps a shadow
of a doubt but everybody'd disregarded that and you know went on.
this is now law because everyone believes it .. I think a theory
is where .. er .. its open to a little bit of doubt but generally
we think that the basics are right..

I 200 aha .. aha .. so (pause) could I .. could I just tease out from
what you've said that the difference between a theory and a law
.. right? .. actually we've had three terms now haven't we ..
we've had hypothesis theory and law and .. or perhaps it would be
better if you could just recap on the differences or similarities
between these three terms --

A 205 well all three terms are sort of trying to make a solution to
the problem and I think a hypothesis tends to be .. you sit
back and you think about what it is with nothing but just
pure thought and it tends to be more or less an individual sort
of thing or perhaps just a small group..

I 209 hmmh

A 209 and then that is now the hypothesis of you or your small group
and then the next step really is to then because you think you're
right you've then got to prove it to everybody else and if they
don't believe that you're right you go about proving it by
experiment and all sorts of other things perhaps and --

I 213 proving it is .. is seeing is believing when you (pause)

A 214 yeah .. you've got to do experiments that you know you got a
result which is tangible and therefore people will look at it
and say "well look at the experiment" and say "yes" you know this
.. this is .. er .. on words alone it won't carry through but if
you prove it experimentally then you --

I 219 -- but .. but what is experimental proof? You've said .. you
mentioned seeing is believing but what is it that you see?

A 220 well you see evidence for what .. for your theory there's
evidence for it and if its say a biochemical pathway (coughs) ..
er ... to ... er ... you know you've read your isotopes and carried it through and it goes the way you said and ... you have sort of radio-isotopes which are end pieces where you thought they would be ...

I 225

hmm

A 225

and then ... this is your experiment ... people can see these isotopes and they can see the results of the experiment and you know this is now evidence for your hypothesis and ...

I 228

-- your hypothesis yeah --

A 228

and if some other groups people start believing it and start seeing the same things and then other groups usually go on and do more work on it trying to disprove or trying to prove it even more ...

I 231

yes

A 231

and then once ... or after say a number of years perhaps the evidence has accumulated or tends to point to this hypothesis then the hypothesis becomes a ... goes up in scale and becomes a theory ...

I 234

right ...

A 234

everybody believes it and then if it goes to the test of time then it eventually becomes a law because there hasn't been anyone ever been able to cast any doubt on it perhaps so it becomes a law ... but laws quite often say perhaps after a hundred two hundred years perhaps technology's improved that much that they can see where the faults are in this lot ... like Newton's Law really which was adapted by Einstein up to a sort of universal level while his was merely a global level ... nothing really changed it was just a technological step you know the farsightedness increased and (?) blew up

I 242

right ... now when you say nothing really ... nothing really changed er ... what do you mean?

A 244

(pause) (inaudible/giggle)

I 244

er ... well when you said Newton's Laws and ... or ... there were sort of ... increased technology did you say?

A 246

hm (affirmative)

I 248

well ... yes ... farsightedness you were no longer looking at a globe ... you were looking at a universe ... when I say nothing's changed I mean Newton's Law as regards the globe which he was actually applying it to remain the same but you know on a universal level errors crept in and sort of where he calculated
and sort of gave a rough estimate when there was a far more accurate way of doing it perhaps incomplete you know reversal this sort of thing but the essence of Newton's Laws are correct and you know people still use them . . but on a much larger level his laws fall to bits really because they weren't designed for that sort of thing so although it was a law it wasn't an infallible law because it was only applied to a certain restrictive sort of field and what Einstein and other people have done is sort of applied these laws to much greater fields . . therefore have changed the laws in effect

I 259 are . . now . . did they just say "are here are Newton's three laws of motion" or whatever and then they applied it to a larger application like the universe instead of just a global . . and . . that's where Einstein's different from Newton . . or did Einstein actually . . . you said in eaaefect the laws changed . . .

A 265 well . . er . . I mean --

I 266 -- a new application or has he actually rewritten the laws as it were?

A 267 well what Einstein did I think was . . he tends to be a little unusual anyway . . . was to just go out and figure out what he wanted to do on a . . on a . . he started off on a universal level . . I don't think he probably even started off thinking about what Newton said . . work through all this and when the theories came up it you know sort of showed up that Newton had been slightly wrong as regards universal theory and you know Einstein is not always correct . . I don't think he went out to alter or disprove Newton's theories in the first place I think . . having the whole idea . . the reason why he made the breakthrough was because he didn't start to look at the global theory he just looked at it as a big vast thing to start off with . . I think that's why in effect he made the breakthrough while everyone else didn't . . no one else did I think try to correct Newton's Law you'll never get any further because you've got to sort of look at it laterally . . you've got to think in a different way . . you've sort of got to open up and things like that . . .

I 280 when you say "trying to correct Newton's Law" . . er . . in your view were there . . were there observable errors . . were there errors which were known in Newton's time?

A 283 well I don't think . . --

I 284 I . . I'm just trying to see how that fits with your other statement that . . or . . you said that you didn't think that Einstein really paid any attention to what Newton had said

A 286 I think looking at Newton's Laws and trying to sort of see if there are any errors from Newton's standpoint I don't think they're . . I don't think . . perhaps now with foresight . . you
I know looking back there are but I think the errors are so minute and because you Newton managed to get those laws because he didn't look at the earth he looked at space and in that respect could then apply everything on to the what we would consider you know the sort of (?) big thrill at all you sort of look out at space and say "well perhaps there's a dozen out there" and then sort of brought them in its the way you look you've got to think laterally get away from your surroundings and try and think clearly because so many things er influence you normally that er you've got to have a clear uncluttered brain if you go and actually do something positive and that's why Newton got his laws set out I think where Einstein's a step better is that he one step further is because he meant sort of took his uncluttered brain out from into space into the whole universe and tried to apply the whole thing to that and er I think that's where the difference is if you try and look at Newton's theory from Newton's point of view you can't really go any further but if you look at from Einstein's point of view then obviously the mistakes are from Newton when you try and scale them up in effect they become far more obvious

I 307 I see so looking at anything entirely through Newton's point of view through one person's point of view or another person's point of view is actually going to bring about would this be correct to say er systematic mistakes that (pause) you'll find it actually difficult to break free of that way of looking

A 314 yeah --

A 314 -- because you're restricting yourself to --

I 314 well I mean that Einstein sort of proved that himself because of the way he actually er went about things he refused to actually to put anything into memory everything every bit of information he got he just put in books you know he was always surrounded by books but he didn't actually know a thing so you know he wasn't restricted by all these points of views all these theories and everything he just looked at something from a sort of childlike sort of er naive point of view and figured out a theory and then went back to the books to actually then go through the calculations to see what it was and probably about nine times out of ten you know his thought was completely stupid because it was obvious that a couldn't go to b or something by doing that you know he actually managed to make some fantastic breakthroughs because he wasn't sort of er held down by all these points of view I mean once the big thing was that light travels through an aether I mean that went around for years and why was that because well its a wave and waves had to go through something you can't have it going through nothing and there was no actual proof so people actually made proof for it and because everybody you
know was all talking together and all had these same theories inside their heads you know that they'd been taught at school to have these same theories in their heads . . that they actually grew to believe it although no actual proof ever showed up for it it became one of these enigmas where everybody else says "it is so let's go along with it . ." and then someone came along and says "well this is absolutely stupid . . look at it I mean if it goes through an aether you travel through a plane you won't be able to see out of the windows" (laughs) and you know . .

I 339 well could I just put it to you Andrew . . just to be awkward as it were . . you were saying that the wave theory of light is an enigma because here people had quote 'made their own proof' and had come to believe it . . now if we go back a little bit and think about what you were saying about scientific method you were saying that there you need proof of the 'seeing is believing' sort . .

A 346 yes . . I think the thing is again what I said about theory . . theory is something generally believed by an awful lot of people . . and providing you can actually make them believe it then . . I mean . . seeing is believing - persuading is believing as well I think to a certain degree if you can persuade people that you are . . that you are correct and they bring up an argument and you crush them with your conviction then eventually people will start to believe it . . I mean . . there was a guy - what was it -- I think it was Kelvin or something during the French Revolution you know when they first found dinosaurs and he said its a lizard . . you know . .

I 354 yes

A 354 and you know everyone else said yeah its a lizard . . great . . you couldn't argue with the guy because he knew more about it than anyone else . . he was very good at it and as the others came up they weren't lizards but they didn't know as much about it as he . . he sort of got it up . . everybody because of his forceful personality they wouldn't argue with him because they knew damn well that he was better than them though he was wrong

I 360 so . . . perhaps from that example you are actually putting it a little bit more strongly . . you're actually saying that . . er . . personality is believing as it were . .

A362 hm (affirmative)

I 362 you said 'to some degree' . . in fact perhaps from that example of Kelvin and the lizard/dinosaur its really to quite a large degree . .

A 365 I think it tends to be . . I mean . . the big one was . . take the example of . . er . . Pauling when . . in the search for DNA . . well you know Pauling was really a big guy because he'd made so many
big discoveries and been proved right . . . and the same applied to Kelvin . . . he had made so many discoveries and been proved right . . . and you know by this time he . . . you know . . . he was a big guy in the chemistry world and you didn't want to argue with him because he was considered you know second to God

I 372 yeah

A 372 so when he came up with this 3 ring structure for DNA at first people believed it and it was only afterwards when it sort of went to Crick and Watson and they were sort of looking at it and saw that some of the chemical bonds were sort of childishy inaccurate and I mean it just couldn't be and you know one puff and the whole thing would just fall down . . . for a while it engaged an awful lot of popularity and people sort of . . . they didn't . . . they didn't really go all out to try and crush it because it was Linus Pauling he was the big guy . . .

I 380 so by force of personality --

A 380 hm and I think by reputation more than force of personality his reputation was such that you didn't want to make a fool of yourself and get proved wrong because its one of these things I think . . . its like sort of human . . . again human error that . . . er . . . if you're proved wrong once then people are going to take a slightly lesser view of you next time . . . and . . .

I 386 I see . . . I see . . . could I just tackle you now . . . er . . . on this seeing and believing thing versus the force of personality which by your example of the . . . er . . . electromagnetic wave and also . . . er . . . the DNA structure . . . Pauling's theory and also Kelvin's dinosaur/lizard . . . er . . . now . . . in your view . . . er . . . are the cases where the personality has actually . . . the force of a strong view put forward by a strong personality I should say . . . er . . . has actually overridden what people have seen in an experiment?

A 398 yes certainly because I mean when you take seeing is believing you make an experiment and you've set it out and you get your result and the result goes with your hypothesis . . . the next thing then is for people to disprove it . . . to try and disprove your hypothesis . . . well now if you're a big respectable man and you've got force of personality and everybody thinks . . . you know . . . you're a fantastic chemist or whatever then people are going to be more reluctant to actually prove your hypothesis is wrong because they're not going to want to lay themselves open for attack from you and if you're a big guy they know damn well they are very vulnerable for attack from you --

I 407 -- well --

A 407 I . . . sort of . . . credibility counts --

- A17.13 -
but I think Andrew there are two things here ... on the one hand you can be intimidated into not doing an experiment because of the force of a personality .. you know .. the dominant personality that's putting forward a view which is against yours, and then there's another issue .. the one that I'm trying to now get at and that is where for some reason you're not intimidated .. you do the experiment you get something which you say I can see and if you do this experiment or read my report about it you can also see ... and what you see is contrary to the belief put forward by this dominant personality ... now what happens then?

usually eventually your theory .. if your theory is right your theory will get through .. but quite often this has happened where people you know have tended to believe the big guy stuff and its taken an awful long time say if he dies and .. you know then people will start .. you know .. open up his theory without any fear of retribution ---

-- yes .. yes --

-- and at that point then the theory will collapse quite often because of that sort of thing you can in effect hold back a field .. I mean there's an awful lot of things where human nature sort of overshadows the sort of scientific method and everything ---

-- aha .. aha --

-- there was that guy who (pause) .. I can't remember his damn name now .. that .. that .. priest guy who did all this thing with peas and ..

Mendel

Mendel .. yeah .. well he came up with this theory and everything and he .. I think he tried to get it published and everything but because he was a priest in a sort of little monastery miles away in nowhere nobody would ever you know listen to him .. I mean

(chuckles)

-- this guy who'se going to tell us all about peas and everything .. I mean wow .. and it took you know an awful lot longer before people actually rediscovered his work and it was only because these were the people who were credited scientists that people would listen to them ... so there's an awful lot of situations where human nature is overridden by this whole sort of scientific ...

O.K. so .. it looks like these big guys that you're talking about .. er .. they're scientists but they're not using scientific method .. is that right?

well they think they're using scientific method .. I mean

- A17.14 -
there's no way you can overcome your human nature. I mean they try to go about things scientifically but perhaps they do make a mistake. I mean life for them. I mean there was a big race for DNA. the person who discovered that got you know rocketed you know headline job and everything Nobel prize here we come there was a big race for that and to a degree that Linus Pauling was... did drastically make a stupid error in that. he he was convinced he got it right and he wanted to get it right as soon as possible so he just threw this thing together and then probably by force of reputation tried to get... I mean everybody's out for the (*) really and if you got this you er set up for life really you know... you're down in all the history books...

I see. but you see now I'm a little bit confused about something because on the one hand you you've just said that these big guys they all think that they're using the scientific method. this seeing is believing approach... on the other hand a little bit earlier you said... even if you're a complete unknown with no sort of reputation... providing you use scientific method... if I understood you correctly... and you actually said providing its right you will actually win through in the end... be it in a hundred years or whatever if necessary but you will win through... now... I'm trying to... to... find out... as it were... where this third view has come from... there's... the scientific method and all the big guys think that they're using it... and then there's you as a little guy that's also using scientific method but providing you're right you will win against the big guys... now presumably you as the little guy who's doing this experiment that's contrary to the big guys think that you are using the scientific method... so where does this ability to suddenly think he is right after all... bear in mind that everybody... thinks they're using scientific method...

you use your scientific method which is your attempt to do things properly... to absolutely prove something properly... will very rarely will one experiment prove to be absolutely correct beyond a shadow of a doubt. I mean it usually takes a number of experiments... now what happens is you make your hypothesis... you (?) you've got to make your hypothesis... let's say the little man is right but the big man is wrong but both think they're right and both do experiments to prove their hypotheses... now one experiment could actually prove a wrong hypothesis to be... say perhaps be right... it may need further work on it to prove it wrong... and the other one's only going to prove that it is... you know... from this experiment's point of view then yes it does go along... so both of them are going on... now who's going to attack who? I mean you're the scientist in the middle... the person who's going to attack is the little one
because he's more open to attack while the big one .. if you .. 
you try to attack him then you're open to an awful lot of 
heckling because I mean he's so big that if you try to attack him 
and fail you'll look a right idiot .. and .. so .. for actual .. 
er .. because everybody I mean wants to go up in the world ... 
then if you're going to attack a theory then you're going to 
attack the little guys first ..

I 503 so what you seem to be saying is that its a sort of self 
correcting system in a way --

A 505 hm (agreeing)

I 505 -- for all the wrong reasons you attack the little guy because 
he's easier to attack but by so doing you look at his seeing is 
believing evidence --

A 508 -- and eventually prove him right

I 508 -- and if he's "right" .. if he is right then you'll actually 
.. er .. your attempt to .. to .. discredit him will fail and as 
more and more of your attempts to discredit him fail so as it 
were the movement against the big guy will eventually gather 
steam

A 514 hm (agreeing)

I 514 I see now (pause) do you .. bearing in mind all you've been 
talking about .. strong personalities .. Pauling Kelvin .. er .. 
so on and so forth do you think that ..er .. that there is such a 
thing as objective scientific method?

A 521 (pause) I think everybody tries to be objective .. er .. but 
again .. I think its now impossible for .. if you think you're 
right to then .. er .. accept from an experiment that you're 
wrong .. I think its very difficult to do that .. it takes a 
peculiar personality to be able to do it .. I mean if its 50/50 
then you're going to accept that you're right and you need say to 
do another experiment or just sort of left it at that and let 
other people disprove it ... I think it takes a peculiar 
personality to let human nature overrule objectivity and sort of 
say I'm wrong ... that's where a lot of the trouble runs in .. 
for as I say everything wins through in the end I think its just 
--

I 534 - well that may or may not be my view .. that's just roughly how 
I was trying to .. that's the sort of sense I made of what you 
were saying I thought .. and is that what you are saying?

A 538 yeah that's the sort of thing ..

I 538 O.K. .. er .. I'd like to go back to your example of Einstein and 
Newton again .. er .. because I've now .. bearing in mind what

- A17.16 -
you've said about seeing is believing and so on . . like to point out something which a lot of historians tend to agree on at any rate and that is that Einstein in his 1905 paper on (?) relativity finally put it forward and it started catching on . . it had quite a big influence people took it quite seriously . . or . . but he didn't actually get this seeing is believing bit until 1919 when there was an expedition to the polar icecap and observed stars when there was an eclipse . . right . . and they found this bend that Einstein said would occur in the observation of the stars . . now . . from what you were saying er . . I find it a little bit difficult now to explain why anyone why anyone took Einstein seriously at all before —

well its the sort of thing really . . if someone is basically wrong and somebody else comes up with a new theory I think most people have inklings that perhaps there is something wrong here I mean . . I don't know about you but . . if I get the feeling that something is slightly skew-whiff . . perhaps I'm not going to say outright this is absolutely wrong but if somebody comes along with an alternative view then I'll look at it seriously and sort of go along with it . . then you sort of get the discussion bit and everything and everybody attacks it from a different angle and if it holds up although there hasn't been any actual proof for it yet . . I mean mathematics in a form is proof itself . . I mean O.K. seeing something bend round like that is actually seeing is believing proof but mathematics in itself can in a form be seeing is believing . .

I see . . I see! this seems to be a little bit of a departure from where we started . . the going out and doing phrase —

well in a formula there is an experiment . . if you've got your hypothesis and then you go out to prove it right or wrong . . now perhaps you don't go out and do an experiment . . you know — the volume of equipment to the north pole and everything . . but you actually do all the mathematics for it and see whether your hypothesis is right . . and in effect seeing is believing is in inverted commas . . I mean you don't have to actually see I mean you can't see DNA but you know its there . . and so . . in effect . . maths can be a sort of proof all by itself . . people did actually have to go out . . just to prove to themselves because I mean maths is notorious for you know . . a slight little error that nobody can see can let things happen you know . . typical ones the bumble bee that can't fly . . and the only thing you can take in is that in the summer warm air rises and that's why it flies (?) through it . . but . .

— mathematically its a failure really in flight it isn't . .

yes . . so from a mathematics point of view you know everybody tried to attack it but found it was right and the maths seemed to go so people went along with it but they still . . I mean they still came through that seeing is believing they had to go out in
the end and do it .. I mean everybody was just sort of saying to Einstein "are you worried about this? are you going to sleep at night?" and he said "I'm right.."

I 605 he was absolutely sure?

A 605 yeah

I 606 you said that .. as a way of explaining the sort of discussion and interest of Einstein's work prior to this 1919 expedition which a lot of people say "ah yes here was the seeing is believing proof .." now you're saying that it takes an inkling .. you have an inkling that something is not quite right like Newtonian mechanics .. it might have been an inkling that it wasn't quite right .. and then along came Einstein and that's why they took a big interest in him ..

A 617 I don't know if it was quite a few of the scientists who were working on the same sort of lines as Einstein because they had the same sort of .. perhaps things are'nt right .. I mean if they immediately went along .. I mean the same with Darwin .. when Darwin came up with his theory --

I 622 -- well if we could stick to Einstein .. but you are quite right .. there were other people working on very similar .. er .. mathematics as spatial relativity .. Henry (?) for example .. but I'm just wondering er .. as there wasn't the seeing is believing bit yet .. er .. where did this inkling that Einstein might be a little bit skew-whiff .. to use your phrase .. where did this inkling come from?

A 632 what .. that Newton was skew-whiff?

I 633 hm (affirmative)

A 633 er .. you said Einstein!

I 633 sorry!

A634 er (pause) I don't know .. I mean .. where do people get inklings from? er .. I suppose he was just .. I mean .. looked at .. I mean Einstein as a person .. as I said before .. I don't think he really did look at it like that .. he was a very unusual person .. I mean he .. er .. didn't really go to the proper channels to become a scientist .. I mean he didn't go to university but did science straight away and everything like this --

I 644 -- so he cheated in a way .. but successfully .. I mean not dishonestly but he cheated the system of scientific method if you like ..

A 648 well he didn't cheat the system of scientific method .. he just
didn't look at everything the way scientists.. I don't think.. he wasn't sort of bogged down by sort of the.. the tradition of the science of that time.. the physics of that time whereas you didn't argue with Newton because his laws had been standing for so long.. and.. because I don't think he probably.. you know.. disregarded this and sort of went off on his own track and he came out of it.. how the other people did it I don't know.. I mean that's a sign of a great scientist when you can disregard everything else and sort of try and see if there is any mistake

I 660  

could I now ask you about your progressive distinction between hypothesis.. theory.. and law.. now if I've understood you right there is only as it were a difference that's qualitative in terms of the amount of verification.. of seeing is believing.. not qualitative difference.. really a theory is no different from a hypothesis except it's got more quantity of verification and then a law's got even more and it's mixed up with things like.. how long it's been accepted.. the force of personality etcetera --

A 675  
it does tend to be altered slightly en route as well but in essence I mean remains the same as you say there's just more evidence that accumulated..

I 678  

HM.. now given that.. er.. laws are overthrown and so on.. er.. is there any problem do you think with this view that.. I mean do you feel that it's right or wrong.. correct or incorrect that something can go from being a hypothesis to a law by increasing verification?

A 689  

well I think it's got to really because you've got to have laws.. because although.. I mean.. you've got to accept.. I mean the difficulty is really you've got to accept that perhaps they're wrong but with so much evidence they are probably right.. because unless you actually go from laws I mean the whole thing is like a building block.. I mean.. if you lose all the background.. if you lose all the knowledge of three centuries then you start slap bang right at the bottom again.. you've to build up from that.. so the whole idea I think you've got to do is that you say this is the law and it's probably right except that it may be wrong.. but work use it and see if you can develop further and that's how it goes.. I mean you can't.. the problem is that quite often after quite a while people say it is right it is right don't argue with it.. it is right.. and it then.. sort of.. the hiccups in the building sort of occur..

END OF SIDE 1

Tape Side 2

I 2/000  

hm.. the development from a theory.. from a hypothesis to a law being like building block.. er.. the blocks of which are like verification..
yeah ... I mean therefore science is the building block ... and you've got to use laws in order to do the building ... you've got to base —

Oh so the blocks are laws themselves?

Yeah ... well I was still looking at it from the point that ... that you ... if you're going to develop things farther ... if you're going to develop science farther then you've got to have background knowledge in the first place to actually work from and that is a law which you've got to accept as being right —

ah that is a law that you must have background knowledge or ... the back ... you must have background knowledge which is a law ... background knowledge ... you see the difference that I'm saying? if you're going to be a scientist as it were its a law for being a scientist that you have background knowledge or were you saying that to be a scientist you must have background knowledge which is itself composed of laws?

(pause)

d'you see what I'm getting at?

not particularly (giggles) no ..

yes well I think maybe I've .. er .. misunderstood you a little bit .. er .. I just want to be clear that what you're saying is to be a scientist you must have background knowledge —

hm (agreeing)

and that background knowledge must be composed of things like laws to work on .. now that's the first understanding ..

yeah

and you agree with that?

yeah

there was another way in which I perhaps misunderstood you .. er .. and that was that .. that .. as it were .. a law of being a scientist is that you must have background knowledge ..

well no not necessarily .. I mean the whole thing —

O.K. .. so I think that was what I was inferring from what you were saying ..

you can .. you can.. I mean you can start off .. if you start off with no background knowledge and you try to solve a problem then
you've got to start from rock bottom and you've got to then build up. Half the time there are laws and formulations and theories have been bouncing around for a long time and if you start off using a few laws and everything that are already there, then you can start half way up there. So much to your problem and that is the whole way science develops otherwise nobody would get anywhere because everybody would be starting at rock bottom and only get so far up each time. The whole idea everything goes shooting up is that you use the accumulation of knowledge by other people. If you use laws, I mean you use laws because they're accepted as knowledge that knowledge is right. There's a probability that it's wrong but because it's a law it's probably right. So you're using correct knowledge. If you get knowledge from say a hypothesis which you then add to the theory to all these laws then you start going places. Then you start going up more so you use so your problem is you set up your hypothesis. You use background laws which you accept as being probably right. You add your hypothesis and see how far more higher up you can go.

I 2/034 I see now just in case we haven't been talking about the same thing. Could I now ask you what do you understand by the expression "advancement in scientific knowledge"?

A 2/037 well more or less what I've just been saying just then --

I 2/039 ah that's why I wanted to be. I've written down the phrase "advancement in scientific knowledge". I just wanted to be sure that we were talking. I suspected that's what you meant by when you were talking about. But adding your hypothesis to previous laws. Things like that. But I wasn't quite sure so could you just in a nutshell to be completely sure restate what you understand by "advancement in scientific knowledge"?

A 2/044 well as I said before you you've got the frontiers of knowledge. Frontiers of knowledge which is based on laws and everything (?) which are have been proved to be more or less probably right and your advancement is then when you use those laws at the frontiers and then you add your own theories and hypotheses. Hypothesis on to it and therefore advance the frontiers of knowledge a little bit more and perhaps after a number of years your hypothesis gets proved more. You know more and more verification for it. It becomes a law and somebody else can use that to go even further.

I 2/052 and what happens to to these original laws you've built up from?

A 2/053 well either they can as as in Newton's case remain fundamentally correct but altered slightly as you as you
advance further into a wider sphere or.. or.. as your
hypothesis goes on you can see that the law that you've been
using is perhaps totally wrong and that law crumbles.. or quite,
often a hypothesis.. a number of hypotheses can be built on that
law before it realises that the law itself is fundamentally wrong
and then the whole thing tumbles down..

I 2/058  ah .. I was just going to .. to ask you about that .. in your
view .. er .. Einstein added some bits on to Newton .. right? ..
but I was just going to say what about .. er .. other
theory-stroke-laws like phlogiston for example .. instead of
oxygen .. er .. no one accepts that .. that .. to use your
expression has crumbled away now..

A 2/063  yeah

I 2/063  hm .. now .. I want to know really what's happening to .. er ..
Newton's laws .. I mean .. as far as .. you were saying that
Newton perhaps .. er .. restricted himself to looking at the
global level .. I think most historians tend to disagree with
that actually .. but that's .. you know .. let's for the sake of
argument say that Newton looked at the global level .. er ..
really .. what's .. what's going on when .. er .. Newton added
his .. er .. his things .. er .. bearing in mind that you .. you
could say well no Newton was trying to be as universal as
possible and so was Einstein .. and Einstein's laws theory of
relativity or whatever .. er .. are different to Newton's ..

A 2/073  hm .. (pause) I think what you can sort of say is that Newton
used .. I mean the mass thing I mean that Newton looked at .. he
did look at the universal level and that .. as the laws tended to
be universal but he looked at it from a .. I mean his evidence ..
all that he could use was the planets system and things like this
and sort of .. and sort of .. older knowledge so his idea of the
vast beyond was sort of limited in that respect that's what I
mean by sort of Einstein pushing on further .. but er .. I mean
his mass thing he didn't .. perhaps realise it as mass .. you
know as speed increases mass increases —

I 2/080  -- yes .. yes --

A 2/080  -- and .. I mean .. once you realise that then of course his laws
are bound to be out slightly ... but its only a very tiny thing
.. I mean .. if you live on this planet who'se going to believe
that things get heavier when mass .. as speed gets greater ..

I 2/083  I quite agree with you Andrew .. the only thing is .. and this is
now going to seem a little bit like splitting hairs in this
particular example of Einstein and Newton .. but that is ..
that's O.K. .. for all practical purposes living on this earth
there are .. for measuring ability of most of our instruments and
most of our needs er .. Newton's work may be quite O.K. .. in
fact I mean it was Newtonian mechanics that allowed us to send
rockets to the moon and Voyager to Mars . . which wasn't relativity . . er . . we could have done it by relativity and this is . . this is the question that I'm putting to you . . er . . in terms of utility . . in terms of what we can actually do with them . . er . . Newton is actually easier mathematically to apply if you want to calculate the trajectory of a ping pong ball . . much easier to work with Newtonian mechanics than relativistic mechanics . . but you can use relativistic mechanics . . now given that relativistic mechanics is actually different . . you remember we were talking about looking through different points of views . . Newton's and Einstein's and you were saying that Einstein was more child-like and so forth . . er . . well . . given that Newton's laws are actually incorrect as compared with Einstein's . . most people accept that Einstein is . . correct or more correct and that Newton is wrong or less correct . . now could you just tease out what actually happened to Newton's laws when Einstein became generally accepted?

A 2/104 well . . er —

I 2/104 did they become wrong or did they . . are they . . er . .

A 2/105 earlier I said that Newton's laws . . they didn't crumble but they were proved to be . . er . . had errors in them . . the reason they didn't crumble was the errors for every day use were so minute as to . . I mean not even for every day use . . as you said firing things to Mars and everything —

I 2/108 — sure . . sure —

A 2/108 — the errors inside them were so small . . that's what I meant by sort of global . . sort of solar system type of use . . if you want to send something to Andromeda say then you'd have to apply Einstein's thing because on a much larger scale the errors in Newton's are going to come out . . so . . er . . I think when Einstein's theories came out they took a fair while to actually get . . er . . to actually gain . . er . . belief . . in effect . . I mean . . the reason why they had to go out and do this star thing at all . . a lot of people couldn't really understand them because they were so complex and everything because . . I mean . . to say things like "time is relative" you know everybody relies on time and you've always thought that time is a nice stable thing and he comes along and says its all relative and I think . . there wasn't . . an awful lot of people who perhaps still don't understand Einstein's theories . . they . . they now just accept them and in effect that would . . I mean his theory of general relativity stood for quite a while before it was proved wrong . . and I think one of the reasons for that was the same as for the special . . its so complicated that ....

I 2/122 — ah . . wait a minute . . you said his general theory of relativity stood for quite a long time before it was proved wrong?

A 2/123 well . . before certain aspects of it were proved to be wrong ..
I 2/124 I see .. can you elaborate on that at all?

A 2/124 well .. he only used four dimensions I think it was for his general theory when it was proved that now there were seven .. at the beginning there were twelve .. that the big bang .. if you accept the big bang theory there were twelve .. and that knocks a lot of his results out of it

(Continued ... )
Appendix 18. Example Transcript of Focussed Interview with Teacher:

Dr. O, FI(T)4
I 004...do you think we could start by you giving me just a very rough. er. synopsis of your personal academic background before you came into teaching...during...whatever...?

Dr. O 006 chemistry degree...three years research...two and a half years working abroad for an American firm...one year post-docking...and then I came into teaching...

I 008 which...so...you actually got a doctorate in chemistry?

Dr. O 009 yeah

I 009 and then worked in industry?

Dr. O 009 well yes...um...it was a support...it was an applications lab...so I mean I was doing work very similar to the research work but...as a support to a firm that sold spectrometers...

I 012 uhuh...

Dr. O 012 and and dealing with customers on a scientific basis rather than on a sales basis (laughs)

I 013...I see...er...ch...or chose to go into teaching?

Dr. O 014 I'd always intended to go into teaching from when I was about...oh. I don't know 9 or 10... (laughs)

I 015 uhuh and so this research and so on was sort of a long digression was it?

Dr. O 016 well I wanted to find out a bit more about chemistry. I enjoyed what I was doing so I went on with it for a bit longer...I had the opportunity and it seemed silly to turn it down....

I 018 yep...so you always intended to go into teaching...if you always intended to go--

Dr. O 019 well no that sounds a bit strong. The idea had always been there I mean I hadn't...I'd considered other things when I was in secondary school...the idea of teaching had always been at the back of my mind....

I 021 yeah

Dr. O 021 I wouldn't say I went through secondary school I am going to be a teacher that wouldn't be true but...the idea had been there from an early age...as one of the probable things I would do...
I 022  yep .... can you remember what it was that attracted you to it. I mean .. ?

Dr.O 024 initially no. But it was certainly when I was in secondary school .. erm. I used to teach people in my own class I can remember holding little maths lessons in the cloakrooms. and things like that .. I just enjoyed expl .. helping people to understand things ...

I 027 I see ... um .. well going on from that ... can you tell what . in broad terms .. you're trying to achieve through your science teaching .. ?

Dr.O 029 ..... I suppose in one sense to pass on sort of knowledge that I've gained and pass on understanding of how the world is made and how atoms interact together and how that explains .. and an . e[xplanation] an understanding to the things we observe normally .. um .... but in a broader sense I think science teaching is . teaching a particular type of .. way of thinking ......

I 033 a particular type of way of thinking?

Dr.O 033 yeah

I 034 err . do you think you could say roughly .. what in your view that particular .. way of thinking is?

Dr.O 035 ..... well technical jargon particularly in this SCISP school would be to say problem solving ... (laughs)

I 037 yeah

Dr.O 037 ... umm .... I think that was one of the things that attracted me to .. to science is that you can ... you observe something ... and then you try and find an explanation for why that happens ...

I 039 uhuh

Dr.O 039 and so this idea of sort of . deduction ... finding a possible explanation for something and then testing out to see if that explanation is true ......

I 041 I see so ... so .. is there a sort of a sequence here .. you've mentioned err . observation .. deduction .. testing to see if its true?

Dr.O 042 I think I think there is .. and I'm a bit surprised that the . people that actually run SCISP don't agree that this is the genuine scientific approach .. they ... they .. I . don't know they have a slightly different philosophy which I .. I Don't
find true ... I mean I found even in research ... that this was what you were doing ... although they tell me it wasn't ... umm ... where you where you came across your observation that sets you off can be by ... can be completely by accident or chance or anything but you've still got to observe that something happens and then and then try and explain it ... um ... people say that the ... that science isn't a logical sequence

I 050 uhuh

Dr.O 050 umm ... in the sense that ... a lot of the great discoveries and breakthroughs have apparently come from nowhere but they've come from somebody having their eyes open while they were doing their work and noticing something that was a bit odd ... and then looking for an explanation for it ... so ... given that the original observation may not fit into a logical sequence I think that ... having noticed something then the sequence ... follows ... that you look for an explanation ... and you test out your explanation ... and having got an explanation you make predictions on the basis of it and then test those predictions ... I find that ... 

I 058 and you found that even in your doctoral research? ... roughly speaking?

Dr.O 058 yes ... I'm ... what I was doing ... fitted into that sort of scheme ... I mean I wouldn't say I was doing the whole lot ... but what I was doing fitted into that sort of scheme ...

I 060 yeah

Dr.O 060 given that the original ... observation didn't necessarily come out of a logical sequence ...

I 062 yeah ...... you've mentioned um ... er ... having your eyes open and making an initial observation ...

Dr.O 062 mm .

I 063 by chance did you say ... at one point?

Dr.O 063 I said it ... m ... this is me trying to ....

I 064 or ... or it c ...

Dr.O 064 trying to fit my ideas with the ideas of ... of other people ... who have a different approach to science theory than me ... and ...I'm trying to say well mean you ... we can't put ... I don't think they can be completely wrong ... and I think possibly the point is that I just ... they're asking where the original observation comes from ... and I'm just saying well that's O.K. just comes out of the air but there is then the logical sequence ... and that original observation might be part of a ...
logical or partly logical approach to looking at a problem or it might be an accidental observation.

I 071 ... I see when you say accidental do you mean that you that it just literally just by accident hits you or ...

Dr.O 072 well like um like Becquerel discovering that the photographic plate in the drawer had um become marked ...

I 074 uhuh so you ...

Dr.O 074 he then followed that up (laughs)

I 074 Yes.

Dr.O 075 why had it become marked and therefore and discovered that the rock was giving out radiation ..... 

I 075 yes do you believe that um er I'm just trying to see go through this sequence that you roughly described do you believe that er what was his name Becquerel or whatever?

Dr.O 077 mm ...

I 078 err do you believe that that observation that the plate was fogged um was just a sort of a purely accidental thing that he thought hm that's odd and then went through this sequence of or of of deduction and testing and so on or do you believe that he that to have noticed er that it was fogged he must have had some sort of idea about it in the first place?

Dr.O 083 .... I don't think he can have done because I mean he thought he was taking out a new plate out of the drawer which had which was you know completely new film which shouldn't have had anything on at all and was amazed to discover it wasn't that there was in fact a fogging on the plate already

I 087 yyes ...

Dr.O 087 so that I mean that was completely accidental I suppose the diff I he could have thrown it away and thought it was a mistake ...

I 087 uhuh..

Dr.O 088 he followed it up ..... 

I 088 yess er that's an interesting point actually cos there were plenty of people before him who had actually noticed that their plates were fogged but had just thrown

- A18.4 -
I 088 them away ... um .... why do you think that Becquerel didn't ......?

Dr.0 091 well suppose the scientific idea is that there is an explanation for things (laughs) . ..

I 092 hmmm ... but these other people who were .. sort of throwing them away they ... they were . in inverted commas scientists .. as far as I know ... um ... but they didn't actually go through this sequence ...?

Dr.0 094 well they probably had got something else on their minds at the time ....

I 094 uhuh ..

Dr.0 094 or didn't even . or didn't trust themselves that they hadn't at some point let it get fogged or that somebody in their lab hadn't ... they were .. they were thinking too much upon that one particular problem that they were facing at that time ..... 

I 098 uhuh .... um .. is this this sequence that you've described roughly speaking scientific method in your view ---

Dr.0 098 yes...

I 098 ... to put a name as it were ...

Dr.0 101 I've never done any philosophy and theory of it I've just developed it from doing science ...

I 101 well you .. I mean you've actually done .. research science err .. so so you know .. there's no need to put er . fancy names on it ... I'm just interested in the sequence that you feel you've actually used yourself as a research scientist at one point ... and also .. the sort of you that you're trying to put over through your teaching ...?

Dr.0 106 trying to put that sort of view through some of my teaching but I think . the practical limitations of the classroom and of the courses that one runs .. don't allow one to put that over as much as I would like um ...

I 108 which roughly speaking that sequence that you've...

Dr.0 108 yes

I 108 yes

Dr.0 108 I am happiest when I am teaching along those lines and I .. I don't always find that possible ...
I 110 and you mentioned limitations of the classroom and also the curricula materials ...?

Dr.0 111 yes ..

I 111 um .. could you elaborate a little bit on how they constrain?

Dr.0 111 well when you .. well when you go to a school you're given a syllabus to teach ... um ... you've got exams that um . the kids are going to take ..

I 114 uhuh

Dr.0 114 um ... I find the .. internal exams are the most constraining ones . when I don't write them myself ...

I 116 uhuh

Dr.0 116 umm ... and most syllabuses .. tend .. well . not everybody would agree with me .. but I find that a syllabus tends to have an inbuilt constraint within it . as to ... something to do with the sort of philosophy of the people who have written the syllabus

I 121 uhuh ...

Dr.0 121 and if you've got a syllabus which is incredibly factually based its not always easy to teach by discovery method

I 121 I see

Dr.0 121 and to teach the sort of deductive processes

I 122 ... I see .. so . could I make another sort of jump ... you've given me this er . this sequence and you've said that roughly speaking what you view as scientific method?

Dr.0 124 mmm

I 124 is . is that in . in your view what you'd also call discovery method of teaching ... like .. is that the sort of approach to scientific method which is embodied in ..... Dr.0 126 well you see I'm not used ... I'm not used to all the sort of technical jargon ... certainly .. I am happiest when I can teach that type of ... that type of sequence

I 129 uhuh ...

Dr.0 129 um with um . and its possible particularly with the younger kids

I 129 yep ..
Dr. O 129 um .. I find that when you get on with older ones, the again, the constraints of how much fact you have to get through, don't always let you take the sequence through.

I 132 yes ...

Dr. O 132 and sometimes also the facts that you're .. that you're trying to teach .. um .. and the theories that you're trying to teach it isn't possible to actually .. do all the practical work or the experimental work and to follow that sequence through ... um ... for example ... atomic theory.

I 136 yeah

Dr. O 136 we can't do any .. virtually any of the experiments in school. we do a few. but. but very little..

I 136 uhuh

Dr. O 137 but I enjoy that part of teaching because at least historically I take the kids through the sequence .. that people had discovered that these experiments had been done .. these observations had been made .. they couldn't fit them together .. then something else happened and they gradually began to fit it together ... 

I 140 uhuh uhuh

Dr. O 140 and I always teach the .. atomic theory section .. historically.

I 140 I see

Dr. O 140 because I think you can show that development ....

I 140 I see so ..-

Dr. O 142 a lot of sporadic .. a lot of um .. various observations in different parts of Europe.

I 142 yes

Dr. O 143 which people gradually managed to ... link together and out of that came a theory which has since been tested and refined ....

I 143 uhuh ..-

Dr. O 143 so I like doing that .... I find that with .. fourth year kids if they have not been taught in this way ... it's very very difficult to get them thinking in that way if you .. if you start with them younger.
then you can have a lot of fun because you've developed that idea from 11 and 12 year olds and... they will then come through with you... some topics you can teach in that way... other topics you have to say well look... other people have shown and this is how it works but... we can't actually go through the full sequence ourselves... but if you take over kids at 14 and 15 who've not been taught in that way...

um... it's very very difficult to get them thinking like that...

I see... what er... could you say what you find the main problems with such kids are?

they're just... completely unused to sort of putting ideas together and looking for explanations...

because they've tended to have everything... put to them on a plate in the past....

uhuh uhuh

and that that type of deduction... doesn't occur in many other subjects if any...

uhuh

I mean I believe that there are certain... with certain ways of teaching... history and some sections of humanities which can use that approach... but of course it depends very much whether that is done in the school where you are

and also... kids find... sometimes find it difficult to transfer a method from one subject to another....

but if they've not met that approach before... um... it takes umm... a lot of time... and by the time you've got to fourth and fifth year you've got the CSE exams and you've the O level exams... the... 12 plus has meant that for the weaker kids you're trying to take them through a five year what is effectively five years of education in four years...
so the time pressures are even greater ....

I see now ... you've mentioned the time pressures of courses .... you also mentioned the factual content

I think at one point you said that the amount of the factual content . given the time that you've got to get it in

actually .. goes against .. your sequence?

mm

Dr.O 170 mm

I 172 ..I think at one point you said that the amount of the factual content . given the time that you've got to get it in

Dr.O 172 mm

I 172 actually .. goes against .. your sequence?

Dr.O 172 yes

Dr.O 174 as it were trying to teach that?

I 174 uhuh uhuh

Dr.O 174 Than it does just to present them with it has been shown that ...

I 176 I see . I see . and again .. could I put it to you ... would I be right in saying .. er .. without getting too tied down in the actual jargon or whatever .. that that sequence that you've described is .. roughly speaking .. equivalent to discovery method of teaching?

Dr.O 179 I've always assumed that that was what was meant by these terms but then .. sometimes you find that pe . people are using the same words and meaning different things ..

I 181 sure . sure yeh ... talking about .. using the same words and using them in a different way ... er .. you mentioned that sometimes you find it difficult er . wh . when you're teaching your kids in a certain way and then there are internal exams .. which .. I think you .. you may have suggested at one point that they .. they might have been written with a different sort of approach in mind ..

Dr.O 186 mm

I 186 and that that sort of . conflicts ... um .. could you say what the different approach . if indeed there is one ... is ?

Dr.O 186 yes. I mean there . I mean I . I . have described to you the way I am happiest teaching ...
Dr. O, which isn't the way in which I teach most of the time at the moment...

I: yes...

Dr. O: mm...... there is always the way where you say that what you've got to do is to teach the kids a whole lot of facts or explanations and things and if an exam is geared incredibly to very tight recall...

I: yes.

Dr. O: um...... it may not be possible to teach this other way partly because of time...

I: yes..

Dr. O: mm...... if one goes to real extremes...

I: uhuh

Dr. O: you can get exams which are virtually expecting the kids to have heard a particular form of words for a conclusion to an experiment...

I: uh...

Dr. O: and if they haven't actually got that right sentence down in their books and memorised that right sentence then they're going to find the exam difficult... and I find...... I find those sorts of constraints unjustified.... but if they are there they are there...

I: uhuh... and... would I be right in saying that... in your view if kids have been taught according to this sort of sequence of observation, deduction, testing... um... they're more able to cope with examinations... they're less constrained by forms of words?

Dr. O: no not necessarily... because it depends on the type of examination

I: uhuh uhuh

Dr. O: mm

I: ... well thinking about SCISP and er... Nuffield that's taught in this school... do you feel that your sequence is in
sympathy with those.. mm.. you know like the philosophies of those curriculum projects?

Dr. O 213 ...... -.. very much in with the.. the Nuffield yes ....

I 213 yes

Dr. O 213 ... umm .... I'm trying to think of the actual form of the SCISP exams um ... yyes I suppose it is I don't think quite as noticeably as with the ........... but yes I think it is a better approach but its not always possible to teach that way

I 219 uhuh ....... could I just dig a bit more with er .. with .. you were saying internal exams are sometimes sort of .. collide a bit with your approach .... roughly how do they collide? ...... you mentioned examinations .. I wasn't sure whether its in the internal sense or in an external sense of requiring a form of words .. erm ... was that the sense in which you ... Dr. O 225

mm I perhaps meant that the internal exams .. collide with your approach?

Dr. O 226 .. yes

I 226 yes?

Dr. O 226 and .. its also one of the problems where you're .. um ....... where you are teaching a course as opposed to a syllabus

I 229 uhuh ...

Dr. O 229 with a syllabus you've got the freedom to teach it in your own way ..

I 229 sure ..

Dr. O 230 where you're given a course I'm probably .. I'm probably at the moment tend to stick a little too much to the course because a course always has a philosophy inwritten into it I uhuh

Dr. O 233 and where it isn't .. [interruption] O.K. .. and where it isn't your own philosophy ...

I yyes .?

Dr. O then it isn't always quite so easy whereas you could probably teach the same material .. slightly different way ... um ...

I 236 I see ..... could I just ask you about the last bit of your sequence .. the testing bit?

Dr. O mm.
and just in general terms ... could you tell me what constitutes a test?

Dr. O 239 .. well the sequence that I was using fair enough cannot be applied in every section of a syllabus

I sure

Dr. O um .. but it hopefully can be applied enough times that the kids can see that the other bits are sections of that sequence .. um .......... well the one obvious case that I quote fairly often is when you're um ... is when the Nuffield handbook of chemistry at the very beginning

I uhuh

Dr. O 245 where they look at what happens when they heat substances .. they find that you get a loss of weight or a gain of weight ..

I oh yes .. I remember ....

Dr. O or it can stay the same . all three things are possible .... you don't look at the implications .. and you say well if its gaining weight it must have added particles to it somehow .. and you go on and investigate that

I uhuh ...

Dr. O 250 if its lost weight there must be a gas .. it must have given off something .. so possibly something we can't see perhaps a gas .... you then check to see if a gas is given off

I yeah

Dr. O and you find out ... from there that you're both increasing your knowledge ...

I uhuh

Dr. O 253 at the same time because they have then discovered that when you heat X Y and Z such and such happens but you've discovered it within a much better setting ....

I 254 I see

Dr. O and the kids can go on and .. and predict that certain things will happen ... that they c... if something has lost weight then they can predict that a gas is given off they can then collect the gas .. find out if it is a gas .. discover something about what sort of gas it is

I uhuh uhuh I see so ...
It's only a simple example.

... so in that example ... um ... I'm lucky actually because I remember on my PGCE actually..

had to teach that ... um ... the test is actually making a prediction ..... and then .. and then what?

well finding out if the predictions true .....

I see and if the prediction .. is true ..

umm. what does that enable you to say?.. just in general terms?

well you . well that is your sort of justification for the theory that you've put forward ...

and if you .. if if you've got a theory which .. er .. I'd thought this was what science is about . if you've got a theory which enables you to predict what is going to happen in a situation .. um .. then your knowledge has increased ...

the theory which doesn't enable you to predict anything really isn't a lot of use .. because it hasn't um ...

so what ... its only any use as a stepping stone to something else ...

so a theory ... but there again if you want a more advanced example again if we go back to atomic theory

um . I can't take the kids .. the A level chemists right through it because they're not all physicists .. but you can get as far as the Bohr theory of the atom ....

and you then can actually say to them ... well if we've got charged particles moving round .. physics has .. has discovered the way in which charged particles behave .. if we apply those laws .. if we apply those calculations
I 278 yes .. yes.

Dr.O 279 we find that there would be a force pulling the electron into the nucleus ...

I yep ...

Dr.O therefore that particular theory of the atom ... isn't good enough ...

I uhuh ...

Dr.O we've got a theory ...

I yes

Dr.O we've applied our knowledge to it we've predicted that the electron would fall into the nucleus and we know that doesn't happen ... therefore that theory needs to be refined ... I see Dr.O and I won't go through with them the detail but I'll say that the modern . or one of the more modern theories is the quantum theory

I uhuh uhuh

Dr.O 286 er . and then talk a little bit about quantised energy ... in a sense producing that theory out of a hat ...

I uhuh ...

Dr.O 287 but still fitting its bits to the logical sequence ......

I 288 I see you mentioned er . the Bohr model of the atom needing refinement .... erm ... is it refined in the sense as it were . of ore being refined down to . a precious metal or is it refined in the sense of being changed completely . like the .. is there a sort of er .. a continuum between the Bohr model of the atom and the quantised model in your view or is there a ... change?

Dr.O 294 the the contin .. the continuum is in . the continuum is in the .. the sort of steps towards an understanding of the atom ...

I uhuh uhuh

Dr.O 296 umm . people have put forward an explanation which doesn't actually quite fit the facts so you adjust it ...

I uhuh ...

Dr.O er it may . it may eventually over two or three twists become changed out of all recognition but it doesn't necessary have to
be .. but that . that's surely the progression of knowledge?

I

I see and do you feel that you could always follow the sequence back .. if you're .. a historian or something ... that you could actually always follow ... as it were ...

Dr.O

as its building you ought to be able to follow it through but it might seem a rather twisted ....

I 304

but would it be .. but would it follow your logical sequence as it were if you actually wanted to make the case from going from the Bohr model of the atom to the quantised you know cloud model or whatever? .... um .... could you actually fit it into your sequence do you think of sort of observation deduction testing you know new observation or whatever?

Dr.O 310

well I mean I think so yes but I mean its possible that other people would say that um Planck had to produce something out of the air ... um ... but that was still .. that is still .. to my way of thinking all part of the search for the theory that explains the facts ....

I 314

the theory that explains the facts?

Dr.O

um ..

I

uhuh

Dr.O

and enables you to go on and predict ...

I

yep ... fresh .?

Dr.O 316

mm ..

I

yep .... um ..... when we were talking about testing and er ... a prediction actually, you know sort of .. succeeding as it --

Dr.O

mm .. mm

I

-- were because the things it predicts .. if I've understood you --

Dr.O

umm

I

-- correctly .. actually occur ... and then you . you have er . I think you said support for your theory or something like that did you or ..? or that your theory is true I can't remember quite what you said ...... that you test ....

Dr.O 323

well you test and if the theory .. if the predictions or the what you predict as measurable quantities from your theory fit with the experimental values
then O.K. ... you haven't learnt anything but you've .. that theory still seems to be alright .. if it doesn't fit then you've learnt something and you go on and try and amend your . amend the theory .. and some

its when the theory ... its when the theory and the practice don't meet that you're making .. that science is making progress ..... 

ah ... its when they don't meet that you're actually making progress .?

yes ..

I see so ... er ... if you make a prediction and it turns ou alright .. you're not actually advancing knowledge?

because that's when you have to adjust the theory

and that's when .. the present explanations aren't good enough and you're hopefully on the move to a better explanation ......

... [interruption] well ... g . given what you've just said, Janet,... do you think erm . that when you're testing a theory its more fruitful to set out to er .. to get a prediction which would agree with the theory or to actually set out to get a prediction which would actually ... conflict with the theory

..... it depends how new the theory is .

when the theory is very new .. then you .. you would obviously test it in the simplest case where you expect to get agreement first of all ...
I uhuh

Dr. O it will explain certain situations ...

I yes ..

Dr. O and then you try and see whether it will ex. whether it will fit slightly more difficult situations and you then push back ...

I um. so initially you set out for prediction. to seek um. things which would actually agree with it. whether they actually agree with it. whether or not you can't tell until you've done it..

Dr. O no ..

I but .. and once you've got some that have agreed with it then you go on to actually .. developing its. its application is that right .. and seeing if there's places where it doesn't er.

Dr. O you then look into some of the other .. the .. more interesting areas where it .. may or may not fit and see .. to what extent .. good example. lattice energies .. whether they whether the so called experimental values fit the theoretical values .. and then the assumptions within the theory of .. calculating lattice energies are that you've got pure ionic bonding ..

I uhuh

Dr. O and so in the compounds where .. the theoretical values and the experimental values give a good fit

I yep

Dr. O then you've got .. we would then say that your assumptions within that theory of ionic bonding are very valid ..

I yes .. yes ..

Dr. O but if you look at another set of compounds you find that the fit is not very good ..

I uhuh uhuh

Dr. O 374 and therefore the assumptions upon which the theory is based .. are not valid in those cases .. and then you look at those particular compounds. compare the two sets of compounds and say .. why have we got pure ionic bonding in this case .. or why does our model of pure ionic bonding .. give a good explanation .. for those compounds and give a poor explanation for the other compounds ..
I 377 I see .. and that's how you . as it were . get a clue as to where to direct your research is that right?

Dr.O you get a better under .. you get a better understanding of how the . of bonding theory yes ....

I 382 I see ... could you . roughly speaking . er there's a number of terms . hypothesis . law . theory ... do you think you could roughly tell me .. just roughly because I don't want to keep you too much longer now ... because its getting on for time .. um ........

Dr.O 386 [laughs] I know it sounds ridiculous .. I used to enjoy that question

I yes?!

Dr.O 388 um ... the problem with school teaching is that you get so remote from real science and I'm not at all sure that I could define those quite so clearly now

I 390 uuhh

Dr.O uhm .. in fact I'd be frightened to because I'm not completely sure

I yyes ...

Dr.O 392 a law is usually an experimentally ... determined observation which holds in most cases ...

I uuhh

Dr.O 393 I think almost all laws have got exceptions but ......

I yyes

Dr.O its an experimentally observed .. pattern or general rule ...

I uuhh

Dr.O 395 a hypothesis is . a theory which hasn't been put to any form of test or which it isn't possible to test ....

I uuhh

Dr.O 397 um . a theory ... is um .. again an explanation or an idea which has been put forward and to which .. some level of testing or validity has been given to it ...

I I see .. now
Dr. O 402 I think I'm not... I'm not... you look a bit surprised perhaps I'm slightly wrong there certainly a hypothesis is one that in theory... that you can't really put a test to... its been suggested... people will probably work on it but... you can't put an easy test to... a theory... most obvious one is atomic theory... and some people would argue that its been so many ideas in science are based on it that in a sense that it has almost been proved by induction but um.....

I 413 um... when you say that you feel that you might be slightly wrong in your definitions... do you feel that there are... as it were... right definitions... somewhere?

Dr. O 416 no but its... I'm just sort of out of touch with that sort of philosophical approach and... I mean that... that is in fact the question you've asked is in fact an A level exam question...

I 418 Is it... yes

Dr. O and I... I can remember when I saw it on the exam paper... thinking oh super... I remember having had lots of discussions and having... discussed it a lot and... enjoyed the discussions about it in the past... now... I feel I'm a bit out of touch with that sort of thing... and I wouldn't trust my judgement any more...

I 423 O.K... um... could I ask er... a slightly different question... and that is those three... three terms... um... do they er... have any place in your sequence of observation deduction... testing?

Dr. O 430 ... I suppose one could develop... one could mesh them in...

I uhuh

Dr. O um... I mean most of the time we're playing with words

I 432 yes...

Dr. O and therefore different people will use different words in different ways...

I mm... mm...

Dr. O and even people I would appear to disagree with probably largely a case of words and there would obviously be an awful lot we agree on even... even... if we apparently disagree on words...

I certainly... certainly...

Dr. O um... yes... I suppose... yes I suppose one can fit them in... I don't think they're necessary..... [laughs]
I 439 I see so ....

Dr.O I mean there is a tendency laws tended to be declared at a
time when people were'nt looking for explanations ....

I uhuh uhuh

Dr.O because if you go through historically the first sequence ..
the first .. great coming in of science was to sort of .. first
of all discover how things worked .. so if they discovered
that something always seemed to happen they declared a law ....
whereas nowadays that doesn't .. that isn't happen in the same
sort of way because .. people are much more .. looking at ..
for explanations as to how things work ....

I 447 I see so so so would it be fair to say that .. that
initially people were rather er .. even .. that they used to
tend to say O.K. we've tested you know our deduction .. and
we've got what we thought should happen .... and we declare it a
law .... but you think people are less likely to do that ....
nowadays ?

Dr.O 455 well if you look at most of the stated laws of science they're
all well over 100 . mostly .. near .. a lot of them nearer to
200 years old

I 456 uhuh

Dr.O and I think it reflects .. the .. different state of the art ..
at that time there was .. not even a belief that things were
logic .. things did not follow logical sequences and therefore
if somebody .. found something that appeared to always happen
.. it was a great event and it was .. declared as a law ....

I 463 I see .. what er .. would it be declared as now do you think?

Dr.O 464 well it would just be published as a .. in scientific paper
somewhere and get lost in the morass of facts [laughs]

I 466 ... yeah .. would they actually call it a law or would they
call it something different do you think?

Dr.O 467 just depends on the egotism of the chap who writes the paper
[laughs]

I yeah .. so its an individual decision .. do you think? .. is is
it an individual decision er .. to call .. ? something a law ..

Dr.O 471 I don't think many people call things laws nowadays but .. I
mean you get thing .. jokingly somebody will refer to something
as a law on the other hand if somebody puts forward an idea
which is used an awful lot ..
Dr. O even today people will use that chap's name as a shorthand ...

I I mean in the calculations that I was using we used to refer to
the different sort of refinements and assumptions we used to
refer to by the names of the chaps who'd put them forward even
if it was only over the last four or five years

Dr. O somebody had put forward a suggestion at a conference then if
it was a good suggestion and people started to use it they
would refer to it as his approximation ... or doing ... um ....
a something calculation .. well depending what it was or doing
I and you call it an approximation? Dr. O or doing a such and
such a calculation then would still refer to it by somebody's
name ....

Dr. O well nowadays yes ...

I nowadays ..

Dr. O um ..

Dr. O would there be .. what .. would they under any circumstances
call that thing um .. a theory or a hypothesis instead of a law
do you think nowadays? or do you think that --

Dr. O no because laws ... laws are to do with observed fact --

Dr. O -- uhuh --

Dr. O -- theories are to do .. theories are much more .. sort of
explanations ... things that are curr .. current immediately ..
well for example the gas laws ... I mean you can actually
measure how a gas behaves under certain conditions ... laws
tend to be to do with physically observable things whereas
theories .... its the application of the theory which is um ...
tests .. which gives you an experimental value which can be
measured ....

Dr. O no no no

I well I just wondered is there a different approach for
getting a law than there is for getting a theory or whatever
... because you said that a law is a sort of observed fact ...
... and . and at one point you said its sort of it could be
fitted into sort of the end of your sequence I think?

Dr. O no no no
I no? .. sorry
Dr. O no I certainly didn't say that ... if anything the law would fit in at
in at the beginning .. that is .. that's your observation ..
... your experimental .... thing
I yep . I see so .. in your sequence of observation deduction ..
testing ... law would tend to come at the very beginning do you
think? is that what you said?
Dr. O 515 I would have thought so yes
I uhuh ... and ... the law would be ... how would that relate to
observation?
Dr. O 521 .............. don't really see the point of the question ... I
mean if we take the gas laws for example ...
I mm ..
Dr. O somebody notices that the gas expands if you heat it ..
I 523 yes ...
Dr. O 525 so they test to see if that's always true
I uhuh uhuh
Dr. O 526 and if all the gases that can be tested or that they can find
data about ...
I yyes ...
Dr. O expand when heated then that can be put down as a law
I ...
Dr. O umm
I well I think I've got a bit confused over the difference
between a prediction and an explanation because in a way you
could say that the gas law um ... is an explanation ..... but
you could also say its a prediction .. couldn't you .. ? because
it explains why . it explains observations
Dr. O 535 yes ..
I but its also a prediction?
Dr. O its no expl .. its no explanation at all
I no?
Dr. O um ... it's just an observable fact that gases expand when heated ....

I yes ... so ..?

Dr. O there's no explanation in that as to why they expand ...

I yes ...

Dr. O you've got to go on to kinetic theory ...

I uhuh ...

Dr. O in order to give a possible explanation as to why gases expand when heated .. and your theory then is to do with the way the moving particles behave and if you ...

I ah now I see the difference yes

Dr. O -- put the um . if you .. if you give them more energy you .. if you've put more heat energy into the system .. the theory states that you then increase the kinetic energy of the particles ...

I sure

Dr. O and therefore that they will be pushing back and will need more space ...

I I see ... now .. now I understand what you mean ... it was just the relation .. as it were?

Dr. O mm

I mmm ... and that comes at the beginning of the sequence ... or ... even before observations or .. just after observations ... or is it actually part of the observation

Dr. O its all part of the observation ..

I the , the gas law?

Dr. O well the gas law is surely the observation

I I see .. that's the observation . from which you get .. in your sequence er .. deductions .. and then you . test those deductions

Dr. O yyes ...

I yeah ... um ..... if you get .. um ... a prediction ... if you ... sorry . were getting a bit .. must you be off?
I'm sorry... well I've got to get to Lower School and back before a quarter past.

O.K. ... O.K. ... could I just ask you this last few minutes... if you get an experimental result that agrees with um... with your prediction do you... what do you end up with then?

why you've to some extent justified the assumptions and that that you've put into your calculation.

uhuh ...

they were in that particular situation... the assumptions and exp... the explanation that you put in were correct.

uhuh

or appear to be correct and you the test a more demanding situation....

right... um... when you say something's correct do you mean that um... following this sequence you can never end up by saying that anything is absolutely correct... absolutely true or... do you feel its just something else?

...... its a working... you know its a working theory... or a working hypothesis...: I mean heaven knows... you know somebody can always come up... can discover something later which doesn't quite explain it and then you have to... build in some other assumptions or........

O.K. could I ask if you think that scientific knowledge is different from other forms of knowledge?

well I'm more aware of what you mean by scientific knowledge... I'm not quite so sure... um historians and musicians would call knowledge... I mean I think in any subject there is an element of... knowledge... which is the same as scientific knowledge.

... uhuh

but whether... English and music would refer to other things as knowledge which wouldn't be considered as knowledge in science I'm not sure.

uhuh... what er... what sort of attributes... do you think scientific knowledge has...?... because some people make a distinction don't they between sort of general knowledge... religious knowledge... scientific knowledge... and what is it... roughly?
Dr. O: well sorry ... I don't think that there is a distinction ...  
I: uhuh ...  
Dr. O: I mean general knowledge is simply the knowledge which you would expect somebody most people to have a background of knowledge  
I: uhuh  
Dr. O: um ... I don't see that that is intrinsically different from other forms of knowledge ....  
I: I see so could I ask um in your everyday life um do you feel that the way in which you acquire and justify knowledge in your everyday life um has a relationship to how scientists acquire and justify scientific knowledge?  
Dr. O: it isn't tested as thoroughly as scientific knowledge ought to be but that um I suppose there's more hearsay knowledge but then there's lots of that in science anyway it's something that I haven't really thought about .....  
I: so it's not tested so thoroughly but er the could be some link in your view?  
Dr. O: um  
I: yeah ... O.K. thanks .
Appendix 19. Example Transcript of Focussed Interview with Lecturer:

Dr. T, FI(L)2
What I'd like to actually ask you about today is in the general area of the conduct of science and of undergraduate science teaching. So .... the first thing I'd like to ask you is, just very briefly, do you think you could tell me a bit about your personal academic background and occupations up to and including your present academic duties and research duties, just very briefly.

mm .... well between school and university I spent two years doing National Service which I suppose was an education in some ways and I think probably its enabled me to reflect much more on what I wanted to do .. um then I spent three years at Durham um reading for a degree eventually in Zoology, on Zoology. I then did a years postgraduate course at Cambridge and I suppose the main reason for going there rather than going on to do research was that I was very interested in doing running at that time (laughs).

A years postgraduate course in?

In education.

At Cambridge, mainly running but I did do some education, I did quite a lot of teaching and I quite enjoyed it. I think I had actually reflected sufficiently, that I didn't just sort of drift into teaching because that happened to be the easy way out, um basically I suppose I'm a person who enjoys contact with people and um, therefore a job either teaching or in some sort of relationship with people, doesn't matter what their age is but it happens to be now university students in the main. But in fact after Cambridge I then got a job as Head of Biology at Forest School which is in the outer part of London at 7 and I was there for four years, enjoyed it enormously but I felt it was a bit limiting and at that point in time I used to go quite often up to University College in London and the University of London Library and I got involved in a little bit of research at school ... and I must admit that some of the things that had put to me .. I'd been actually asked to do research when I graduated . but there was nothing particularly at that stage that motivated me strongly enough to want to do research, because I think to take on three years research you need to want to do it. I mean its not just the degree, frankly, you've got to be interested and basically enjoy what you are doing for those three years otherwise it could be purgatory .. um but I got interested in some problems ...... I thought about at one stage applying for a Royal Society Grant which was given to school teachers ... but in the end I decided to make an absolutely clean break and
go back as a mature research student on very little money and subjected my wife and family at that time too ... um low income but um I don't think we really regretted it ... er eventually because of the costings and one thing and another I went back to Durham, I thought about London. But the trouble was London was very expensive to live and, amusingly enough, some of the people I'd been talking with in London had actually moved up to Durham again. A total coincidence, so I did a PhD at Durham into the sort of physiology and biochemistry of ageing which is something that still continues to interest me and something which, as a sort of outside the university, I'm very keen on aspects of nutrition and ageing. I'm a great believer in preventative rather than curative medicine, we spend ridiculous sums on curative medicine and by the very nature of medical training we seem to focus on that rather than preventative medicine that would stop a lot of people getting there in the first place. We even set about this in the right way and I believe in the right form of education, because diet and smoking habits and this sort of thing are all quite an exercise, are all quite fundamental to good health and unfortunately in this present society that we have, which is instant junk food, watching television and getting fat, possibly in some instances smoking too much and possibly drinking too much, not that I'm anti-drinking or anything. But again I think people do and so ... so it's an issue that I have actually interested myself in, outside the university as well as partly inside it and it stems as I say from an interest in physiology and biochemistry of ageing and now I find myself within the School of Biological Sciences and I was appointed here as what was then called an E Tutor. This was an experimental thing that Sussex did in the early stages, Sussex was very full of experiments in the early stages, which I think was rather good, and I regret to say that as time has gone on it has been less innovative. But at that time we make a lot of mistakes but I think it was an innovative place, perhaps there was money also available at that time that allowed more innovation unfortunately in my view. So I was appointed as an E Tutor, and that meant that I spent two thirds of my time approximately in the School of Biological Sciences... and about a third of my time in education, at that time it was called the School of Education and Social Studies and then changed its name to Cultural and Community Studies and Education actually detached off from Cultural and Community at a later stage. My prime involvement in Education at that time or Science Education was the PGCE course, and although I had a spell away from that during a time that I was directing one or two undergraduate teaching projects in the last two or three years I've come back into the PGCE work, which I enjoy because the um... its quite nice to be part of the formative stages of um... potential teachers, so um... in other respects I've not had a great deal to do with the education side, I have actually been involved in supervising one or two MPhil theses and I was involved with in-service BEd for a few years in the early 70s which meant a
certain quantity of science education teaching. But er most of the time I actually spend in undergraduate courses and from about 1969/70 to about 1979/78 ... first of all was the Directorate at Sussex of the inter-university biology teaching project here and during that period as well I also took on a writing role with one of my colleagues, Sir Robert Whittle, in the joint universities genetic project which culminated in I think S299 genetics at the Open University. Again that was an interesting experience, partly for the political wranglings as well as the design and writing of courses, it was quite an eye opener I think, but er . primarily I think that takes us up to date.

Could I just ask you what your current research speciality is?

Well I've been doing one or two things for um .. having spent something like eight years on developing undergraduate teaching materials. I must admit it was a fair relief to doing more research on the purely biological side and I had a research student and recently I had a visiting Professor from the States .. and we've primarily been looking at what's it called ... Biogenesis Microchondrio, that's how Michondrian is made, but er I have recently been well over the last few years I have been involved with the Institute of Biology particularly with the Journal Biological Education, which I sit on the Board there . and the Education Division of the Institute um .. I'm also as it happens ... sitting for the Education Division on one of the Nutritional Committees which does interest me. Um .......... I am currently thinking as I recently talked to a man called Dr Roger Hammond, who with another colleague at Cardiff have developed a self-learning course for the first year, rather like we have in the Department of Zoology and er because I've always thought that this has been wrong . that we've each and everyone of us beaver away in our own little institutions primarily producing materials designed specifically for our institutions ... and . unfortunately despite our good intentions . that was the intentions of the Inter-university Biology Teaching Project, many of the materials produced did not get excessive use, in some cases didn't get any use at all in other institutions, despite all the efforts and time and money that had gone into it. Now, as it happens our materials, the basic biology course was used rather more extensively not only in the university but in some sixth forms um . so that pleased us a little bit, but it wasn't taken up in quite the same way that perhaps we had originally anticipated.

Are you referring to that set of books?

Yes, published by Cambridge. But the thing that I'm moving onto is that Roger Hammond and um . his colleagues also did a similar sort of thing. That was a first years Zoology course, but we've been talking about interactive video as a means of self-learning and teaching in the first year.
That has the advantage that it is far more flexible plus the fact that you can write the materials on the word processor and change it. Indeed so much so that you could actually put a framework out to any first year course and any first year group of people could slightly modify that to their specific needs whereas if you write a book it unfortunately requires a number of years before the mistakes and other changes are made in it, and this is very attractive because interactive video is in its early stages of education, but I think it has quite a lot of potential and it may well be that in the next year or so we shall be looking in that direction. And the other thing that interests me is um a scheme that’s been going in the States for some time, I met the sort of co-founder of the idea, a man called um Professor Wales at the Engineering Institute at the University of er... sorry I think they like to call it West Virginia University at Morgantown, and he's been looking into the problems of decision making. That decision making is not a thing that is taught in many schools or universities, and they have a scheme that they have been developing over the last, I should think, seven or eight years, called Guided Design Programme and essentially it involves groups of students in making decisions about a specific problem. In other words to come up with solutions not on a totally ad hoc basis, but actually to now be offered the refinement that they've recently made is to um offer guidance as to how to come up with suitable suggestions, given, of course that in a real situation, you've got to look at how practical your suggestions are, and what the cost is going to be and whose going to implement them um how much flexibility are you going to allow and some idea of how effective it is going to be. And so they may come up with a number of alternative solutions and look at them more deeply, of course it demands in each case a um some knowledge, but it's not essential that everyone has this knowledge locked up inside their brain, I mean one of the other things I'm very keen to promote and indeed do so in the first year is what I call a literature search, I have a number of these little...... well they're almost like treasure hunts or little games that I present to students in which they have to go away and find out information from journals and other things in the library and it makes them use just about all the facilities of the library as well including the audio-visual, the inter-library loans um reserves section, they are of course there on computer search. Actually its the computer search, I think is complete from 1979 up to the present day, when it is incomplete prior to that, you have to go back to the card indexing. There's also a Microfiche that they can search and scan in on things so I try and get them involved in just about everything and yet at the same time to the questions relate to a specific theme. the two themes, that I've developed with them so far it takes a bit of time... one is called um Haemoglobin and er
... studying the structure and the programme, genetic programme of haemoglobin and of course the various anaemias that are sickle cell and ... and so on, associated with a very well research area of biochemistry and physiology um ... the other area, the other little literature research is called Nutrition and the Third World, and that actually. both of them start from very simple beginnings, basically let's find out some fairly straightforward facts and then they get them more involved in specific papers and um in the Nutrition and Third World actually it leads them into a paper which they have to look at somewhat more deeply than with some of the Haemoglobin, with Haemoglobin they go through more papers with more questions with the Nutrition they do some scanning and go to one, one specific paper. um ... which I've doctored slightly so that they have to come up with some ideas of their own ... and they probably have to go to consult some of the references given in that paper of their own accord, it uses a bit of initiative so those are some of the things that we've been interested in. The other things, two year ago I had a couple of visitors from Mexico, and er they were mathematical biologists but we did at that time was to look at some simulation exercises in mathematical ecology and er these are very simple well er ... perhaps not quite as simple that they might appear, they basically are simply, they use either a programmable calculator or some simple models and little bit later on, on a simple computer. um. they could be quite appropriate to slot in to a number of courses but that was a sort of a joint effort because that was their interests. I think that pretty well brings us up to date.

Well thank you very much for that resume. It's quite interesting for me actually um I get the impression that your two themes of Nutrition in the Third World and Haemoglobin in related diseases, um you've developed that over the last couple of years as specific themes, but I can see a sort of precursors to those. I remember Haemoglobin figuring quite prominently, I suppose inevitably in the first year of undergraduate tutorials and I certainly remember in week two that it was go and have a look at this, find your own references. er not quite so formalised as specific enzymes and similarly I remember there was a seminar paper on photosynthesis that I think they'd even doctored and we were asked to

( no I haven't actually doctored that paper)
Right.

And so its a reflective exercise as well as a comprehension exercise. But all that I've done I think is to set up either ten or twelve questions which tested their comprehension, which made them probably look up in other journals or other books. Relatively short, three page paper with, with some fairly interesting graphs and tables, ... its a very good paper, I constantly look for papers like that ... and even the original Crick and Watson paper on DNA is quite an interesting one to get students to look at, and of course even going further back in the genetics course here we get them to look at the original Mendel paper as translated, it was originally written in German, it was translated by um ...... by I think it was Bateson, yes Bateson or Bateson and um .. again. I think what it does is to . when students come to us with an 'A' level background in genetics, very often the typical textbook approach, quite inevitably has to be to condense it and just take out perhaps two or possibly three experiments nearly always round or wrinkled peas, or yellow and green peas and then they can sling them together and . you get a sort of three page synopsis of the whole of Mendels life and basic experiments. What I think doesn't emerge from a lot of textbooks is um ... that Mendel, alot of people say that Mendel was very fortunate to get such good results, I don't think he was actually, I think he was a very clever man, I think he realised that when to stop and er I think that from a statistical point of view, there's been lots of arguments as whether he doctored the results or not. That I think is immaterial, if you go back to the original papers you see that in the number of the basic crosses he go extraordinary ratios. I mean in one sort of cross one might have got to about twenty nine greens to thirty one yellows, which is not exactly a three to one ration on the other hand he could also have got and did in fact get something like thirty two yellows to one green which is not exactly a three to one ratio. Its when you scan through the whole set of data that you see he see he started to see this ...

So what was the point you were making about the ...

The paper ..... The textbook, you started off by saying there's a problem with this school text.

Well I think inevitably because they had to cover a whole curriculum and they had to put that it has been traditional at least to put it within one cover. they can't make it too big or too bulky because it is not easy to carry around and if they make it too big anyway its frightfully expensive um ... either the students or the school can't afford it so of
necessity I think they have to um, be fairly brief, but it tends at times, to take away some of the um. the really hard spade work which quite clearly emerges from Mendel's writings of his experiments.

Right.

I mean it was jolly hard work, sheer hard slog for many years because peas don't grow like bacteria or flys, I mean you don't knock up ten generations in a year, you knock up one, then you've got to analyse them and replant them in the next year and so on, so it was a very painstaking piece of work and that often is not appreciated I think in my students.

Do you feel this is an aspect of the conduct of science that you would like to bring over to undergraduates in your teaching?

Do you mean the history or the hard graph?

The hard graph at this stage um..

I think elements of both, I quite like the historical aspect, I'm not quite sure whether the students appreciate it or not because I think you've got to be a bit reflective and put yourself in the students' shoes at times, there's a marvelous saying, I don't know if I can actually quote it directly but, "never judge someone until you've walked in their moccasins" its an old Indian saying.

That's a lovely saying, I might even use it myself if I may (laughs).

I've only got part of it exactly right, I think it's always a good thing from time to time particularly if things or if the responses or marks, marking of paper, marking exam, or marking an essay. If the marks are uniformly rather low, the tendency I think is to put the blame on students and say well they're a thick lot they just didn't understand and that may be true, on the other hand I think its not a bad thing to put yourself in their shoes and think, did the ideas that I was trying to convey were they put across quite as well as I could have done, were the basic principles brought out and so um. it may be that. I personally enjoy the historical background now from a retrospective view from the science, one enjoys it and sees that pleasure of those experience and in fact the hard work.

The pleasure of those experiments?

I don't think you can really ever quite portray to a student unless they've done some sort of research of their own or unless they've and MSc or PhD as to the amount of material that you tend to accumulate and have to analysis or even the routiness.

Could we put it a little more strongly, would you feel that er
a knowledge of history of science is actually a necessary component of say an undergraduate degree in science.

.... well necessary is probably quite a strong work.

Only you've mentioned .........

Um I, I mean I take a personal view on this, not necessarily accepted by all my colleagues, or whether it would be accepted by the students, I think its not bad, and some students I'm sure would appreciate it. To look back on how a particular subject area has grown because I think if you're not careful if you take it without the history, you tend to see the subject as being frightfully logical, this happened and then that happened and it developed beautifully, frankly most science doesn't develop that way at all as far as I can see. Its a little happening that occurred in 1908 and then there was another piece of the jigsaw puzzle that happened in 1915, and at that time 1908 and 1915 seem poles apart, and then gradually you find bits more of information being brought up and then suddenly someone will come along and fit the pieces of the jigsaw puzzle together um, I think for example a classic case of this is in certain aspects of biochemical metabolism, certain biochemical pathways, um, in which little snippets of information from experimentation had been built up since the turn of the century, round about the 1900s and eventually culminated for example in quite a good knowledge of fermentation and also a pretty sound knowledge of the triocolic acid cycle and the Creb cycle but if you as you said look back into history it didn't all develop beautifully, logically, we got from this step to that step ..

Could I ask have you developed any personal policy towards considering controversies in science, past and present.

Oh yes! plenty of times, I think that .... I've done it a few times, I suppose ....

I'm remembering from my observations ..... Yes we did ......

Structure of cell membrane and michondria and symbiosis.

Yes that's right, I think its not bad to sometimes shake the students a little bit, not too much because then they start to feel that nothing is sound.

Perhaps can I come in there with a question that I really meant to ask a little bit earlier. er are the any sort of patters that you've discerned in first year undergraduate students' understandings of the conduct of science which you would regard as unfortunate
... well .... I think they. I think there is a tendency again by virtue of the fact that 'A' level is what it is, you know there's a lot of stuff crammed into a comparatively short space of time and it may also be true of university as well to some extent. What I slightly sad about is perhaps the students at times sufficiently reflective on the evidence, experimental evidence that went to make up a particular fact um the facts are only as good as the experiments behind them and sometimes the students will quite willingly the textbook because it has to be fairly precise and simplistic DNA makes RNA makes Protein basic central dogma.

And it probably start ..... That's right, and in fact that is probably true now, but at one point I think there could have been some doubt um over the past few years the evidence is overwhelmingly in favour of that central dogma. But if you challenge the students to substantiate that dogma even in outline through experiments many of them can't do so, they just say well that's in the textbook so it must be right and there is this feeling that not best amongst children er students but amongst adults as well that the printed word is somehow gospel, the printed word contains thousands of mistakes, I mean with the best will in the world most Newspapers which people as the sort of gospel, not only have quite often a very biased view but because of a way that information is gleaned and because it's a very personal thing it can be very very inaccurate, I mean I've known many examples both in and out of science reported in Newspapers which bear no relation to, it's the man's personal or woman's personal view.

Could I just ask you on that choice of the Central Dogma on biology, um Asez was the first thing that you tackled week one.

Yes I think I did that ...

Now that. er there are many other things that you could have tackled and I just wondered whether you were influenced at all by um, this specimen question, which had come out in 1982 from the Oxford board called the Central Dogma of biology I just wondered whether ......

No, not at all its..

Pure coincidence?

Pure coincidence, absolute pure coincidence I had no connections with the Oxford board, no. none whatsoever, I have seen the question, I'd be interested to have a look at the question
Well, I'll give you a copy.

Yeh, great, um

.... Could I just ask you what sort of qualities do you look for in prospective undergraduate biologist?

......... What when they come here ...

.. mm you mentioned you've a few problems er ... 

Yes, I'm not sure if its always the students fault you see, I think it may well be the very nature of teaching, and I'm not always sure that its necessarily always the teachers fault, both are under a certain amount of time pressure, time is a commodity that no one has more of in a week than anyone else you can only have seven days and twenty-four hours in a day.

Nevertheless you have to make a decision.

yes ........ let me just reflect on this a little bit ...
I think ........ its an exercise ........ and I know that I've been involved in it for quite a number of years now and recently has biology school selector .... we like to know first of all why our student chose Sussex I suppose, and whether they're the sort of person who's done their homework about the place which might also imply that they will actually do their homework on other problems, now some students come to us and quite clearly they really haven't read or certainly not studied the prospectus in biology, they glibly put us down probably amongst other people and its quite clear that they haven't really much idea why they wanted to come to Sussex, now some students clearly do and they've either been advised to come here maybe other teachers or other students who had come here and enjoyed it and advised them to come here and that's generated an interest about the place. Others of course are interested in the type of course that we run, now I think that most universities in the last ten years have probably quite seriously relooked at their courses in the light of new universities who have I suppose the advantage of when they started up they didn't necessarily have to conform with any previous tradition and probably when you've been brought up in tradition its not easy to break that um .. but I think what the new universities probably did was by moving away from rather artificial boundaries of zoology and botany and biochemistry rather narrow in my veiw subjects . which are very difficult to define anyway without moving over into another area but when the new universities came along and set up, Sussex did a sort of schools of study and .... it is that sort of ... I mean this sort of um more broadly based course or course that's not divided into little compartments, certainly appeals to quite a number of the students that we eventually accept.

So what is it that you're actually looking for?
I think we're looking for students who are interested in contemporary biology.

Contemporary biology?

Yes ... um but um ...

When you say Contemporary biology ..... 

Well I mean they're interested in the way in which not just science but particularly biological science is progressing. Every university has a certain research expertise and accordingly I suppose some students although relatively few choose a place because of its research record. If that were the case Sussex would be extremely high placed on the list because it has in terms of its biological research ..... 

I saw some things on the wall.

That's right, it's been in the first two or three in the last ten years or so. But in fact students don't go. It's interesting.

May I ask you at that point would you value that as a criterion in a prospective undergraduate, they say I want to come here because the research level is good.

Yes .... mm I think we would be quite pleased, yes, I'm not sure if its the absolute criterion for selection and I don't normally base it on that, I think we would be pleased because what we would I think we would have seen would be a student who had taken quite a lot of trouble to find out about this place and who showed quite a strong ....

Had done their homework.

That's right, who showed quite a strong interest it not just well a strong interest in his or her subject and in certain areas of that subject to which we perhaps had some research expertise ... but the things that tend to influence them are probably personalities, so that for example um John Maynard Smith who's on the television, they may know about Maynard Smith and they may know that Sussex is good from the point of view of evolution in biology, well that's what they see though nothing actually is quite correct but um ... likewise a number of people have heard of David Streeter because he's been on naturalist programmes and quite a well know figure in the South East of England, because of his knowledge on ecology and geology and this area. Those are the things that tend us.

The sort of things you've mentioned so far are ....

That's right see if I can just try and list them to make it easier, were the people that had done their homework and
decided that they really want to come to Sussex because of all the things that Sussex offers the ... the type of course I guess the type of university that it is obviously the situation helps, in fact that can even be a negative thing towards some students who I think if you really delve deeply all they want is a sort of three years holiday camp by the sea um . if that's their only reason then I think well some are very honest and it may be an additional attraction and I don't mind that at all. What I like to do is in the very short interviews that we can offer them ... um I look at the reports that the headmasters or whoever it is makes and I try and judge whether that's a fair report on them, picking certain things out of that report and seeing whether the students in a very short space of time that we have them and we've got to bear in mind that some of them are very nervous whether they seem to be, whether they seem to live up to this sort of reference I may show them picture on an electro micrograph, I may show them a file of ..... I may show them a ... um plate with yeast cultures on or something, I may show them something quite simple and ask them some very sort of basic questions and then I might ask them how would you find out type question. 

How would you find out question .......

Yes in other words, let's take a little culture of yeast and at some time I might show them a yeast plate which shows normal colonies and what is described as petite colonies and I might just simply say to them well you can see that those colonies are smaller than others and .... and why do you think .. you know how would you find some sort of cause for one set of colonies being somewhat smaller than the other and what initial punches would you play, hypothesis would you go for and then perhaps very briefly I might ask them well, what sort of experiments might you set up to try and find out whether was it just the fact that they were genetically .......

Well what sort of thing is it that you're trying to get at ...

Well they may suggest oh, there's a genetic difference, OK well how are you going to sort of ....

But what is it that you're actually trying to, is it their thinking?

Well I think its you know that they have . they're not totally set in to a routine of facts, this is this, this is that and when you actually ask them to actually think about a problem and even though it doesn't worry me
that the approach that they use is either right or wrong, that doesn't matter, but as long as they have some idea of the way they might approach the solution of the problem because basically although the whole of the course may not be problem solving there will be quite a lot of factual things .... we certainly will be presenting them with problems of one sort or the other.

Could I ask you a sort of global question then, in general terms roughly are you trying to achieve through your teaching of undergraduate science?

In general terms not in a sort of detail.

very very concisely I'd like to make, I'd like to help people become self sufficient ... again I think there's a marvellous quote somewhere and I think its actually Hearse in one of his books and I can't remember which one, in that a good teacher, and it doesn't make much difference to a teacher of science or something else is one who eventually moves the students on so that he makes himself or herself redundant in other words they don't need you because you've guided them and .. helped them to learn effectively for themselves .... um they no longer need your ... well they need less and less of it as times goes on. Their dependence, I mean what one should see in the three years that they're here is that they move on and that they become more mature in their outlook and thinking um that they .. consider scientific problems from perhaps a number of different view points, we're all very guilty at some point of this, even professional scientists and people that ought to know better, the are occasions, where I think we become very entrenched in our views and don't necessarily look at and accept too readily um that may be a good thing not to accept it too readily but to actually look at . I mean there have been some excellent examples over the last twenty or thirty years of um people coming up with really quite novel or new thoughts on a particular problem and for the established fraternity this has really brought them out with quite a shock and they don't want to accept and they don't want to believe that its right.

You mentioned just now ....

Yes, I was talking about students you see ... now ... er ... its all part of the ongoing process of their development in education, I want to try and wean them away from purely a regurgitator of factual information .. what has gone on in the past is important because that's the basis for
What goes in the future, so in other words you're building current research in science upon what other people have done. So you're can't ignore what other people have done ...

Can you enlarge a bit on what you mean by building on?

Yes, if you start in any field of research you always look at the background literature to that subject, to see what other people have done ... er, in relation to the problem that you actually have. It would be very foolish if you didn't do that because sometimes you might formulate a question to which there is already an answer or at least a lot of work has been done and you just might sort of rather naively repeat someone else's sort of work and frankly it wasn't worth your time or effort to do so. However, what we're really looking at is how science continues to advance and we're looking I suppose for students who are ... who show some sort of creativeness towards the progress of science ...

Creativeness?

Yes, they ... a lot of students won't have it, I'm not sure if research workers necessarily have it but some people have this facility to totally re ... or take a slant on something that is very fresh and new and that's what I think I mean by creative in this sense and that um ..... in one of the tutorials where Chan actually talked about a Amoeba er thinking its something (laugh) of.

Yes, Yes.

You know you really taken with that idea because he's so bongy, bongy like ducks.

I mean sometimes, Chan is a marvellous example of a chap who at times can be very sidetracked and perhaps on the face of it come up with ideas that are really nonsensical but such people can also come up with ideas that are very fresh and occasionally some of these ideas are very novel and actually would be worth testing and I think you've got a case in point with that particular student, I'm not sure how well he will do at this university er ... he has some disadvantages as English is not his native language and therefore he might find it difficult at times to express himself as well as he'd like to ... but he's an enthusiast and I think we're looking for those sorts of people, not all our hundred odd students are, but yes we
would like ideally to see people who were enthusiastic about their subject they have chosen, we're thinking about the subject all the time which I think is what Chan does even if his ideas are a little bit way out that doesn't matter. I mean I'd sooner have someone who was thinking even if at times in a rather obscure sort of way or clearly at times would appear a nonsensical way than somebody was really just a bit like porridge and just sort of soaked up information handed to them but really didn't make too many constructive... didn't take too many risks as to make up good ideas.

Do you think that taking risks is....

Yes, I think that's part of life, I think well, risks perhaps is a quite strong word, to take an opportunity or to take a chance, to do something slightly different or to come up with an idea slightly different the risk is that you could be wrong, but lets face it the chance is that you could also be right... um if you take no chance, never seek to break out of the crowd and take a chance, well its unlikely that he's going to be chopped off but its unlikely that he's ever going to make much progress either because you're conforming and I think there are occasions when I'm sure university life should at times encourage the non-conformist, I mean if you can't be somewhat non-conformist as an undergraduate then doesn't mean to say you've got to be an anarchist or something and totally knock down the system um but... it should provide three years in which you can be reflective you can... and I think Sussex quite rightly has got quite a bad name at times because its done things and we've been equally fed up with some of the students antics but in sense I think that's part of the price you've got to pay if you're going to encourage radical thinking about problems and that doesn't mean radical in a political sense I mean it maybe, it may be radical in that its a very fresh approach even though its not a viable approach as mainly it turns out to be.

If we could bring it down in what you're trying to achieve in more global terms than now to the biology. May I ask you if you have a personal theory of scientific methods as part 1 and part 2 to what extent and do you try and put that over to your undergraduate students?

Yes this is a question asked of me quite a number of times.

May I ask by whom?

Oh, in a number of different contexts, yes I, I once on more than once I think on a couple of occasions I
when I was interviewed for a Chair of Scientific Education and someone had asked me this question, and er I think you've got to be a weany bit careful about it. It's the sort of question that instinctively you can rush into and say yes we do this, this this and this, um but I'm.. I've become less sure about the so-called scientific method. I think there are a number of ways of solving a problem although there is a um...... er..... a pattern that has been established to some extent in science through the way in which science is reported in papers and in literature even though we hold an introduction and then we have er... which we generate perhaps an hypothesis and then we have a method and then we have the results and then we analyse the results and come to some sort of conclusions, we have a reference list and acknowledgements and God know what else and that's normal way our papers are presented and if you know careful one could say that's well scientific methodology, but in a sense it is but it also doesn't totally reflect what really goes on in science.....

I think that's in inverted commas the so-called scientific method as you get it presented in a paper...

Yes, I think that's some peoples concept of science, the scientific method is, I think it actually is... I not only think it is but I think it should be somewhat more flexible, I don't think that's the way in which science is generally done or as you commented earlier that I think allot of it is er... somewhat randomized in the way that information pops up.

I think you mentioned bits of...

Yeh, that's right, um... I mean clearly it wouldn't... I mean in setting about a research task you have to narrow down what the problem usually is and then probably within your resources and one thing and another decide how you're going to tackle it um... you probably do set up a hypothesis although probably you modify and refine that hypothesis in the course of doing your work but I think sometimes play hunches in science, I think they do.

So what is the difference... is there a difference between hunches and an hypothesis?

.............. If we really looked at it there would probably be not allot actually, probably not allot, I mean there are probably people who strongly disagree with what I'm saying, but I think that the more you look into the way in which science has developed um often I feel that the directions in which a particular subject have um... changed partly
 depended on what I would call these rather creative individuals who take a totally ... I mean let me give you a .. what I think is one of the very good examples in recent years er because it happens to be in a subject area that perhaps I know something about but um .... up an till about 1960's people were interested in the problem of how ATP was made and um most people's views had been channelled for so long on a chemical hypothesis if you like but that's the way that most people were thinking then along comes a man called Peter Mitchell who says er no I don't think this is how .. we haven't got any evidence at all, you're basing it on the fact that this is what you will expect, lets totally rethink that and let's base it on electrochemical grading across a membrane um .. rather than you know a reaction that only reacts with B to produce C and there's a interaction with another substance D A ATP is formed and er the amount of ATP that's formed is to some extent independent of certain ideas .. and when ... I mean that was a very . at the time it was a very novel and totally different slant to the problem and people weren't prepared for it, weren't ready for it, they couldn't except if very easily and they said well what's the evidence on which you base this, you see, well Mitchell at that time didn't have that much evidence but quite rightly he said what's the evidence on which you base your thing, you see, and they said well there's been parallels in such and such situations X is the way in which ATP is made in glycolosis so um .. what eventually happened was that Mitchell started to produce some experimental evidence and the people who were rather against this sort of radical idea decided that they would also do some experiments and prove that they were right and he was wrong there were others who I think rather admired him and then thought I think I'll go along and see whether he's right or not, and there were others who were probably the fairest of all who said he could be right or he could be wrong we're not going to go and sort of immediately say because its such a radical idea you're wrong. What happened over the next ten year period I think that was the weight of evidence favoured Mitchell much more than the other alternatives, so much so that when another seven years lapsed the evidence, I think quite strongly no overwhelmingly it couldn't be conclusively said that this was it but the evidence was certainly very strongly weighted in Mitchell's favour and because of this very novel approach, totally different, I mean what Mitchell did was really to see what other people and seen before in a number of different contexts and put it into this context, I think basically you know 80% or 90% he was right and I suppose the Nobel Committee also thought so as well because they awarded him the Nobel prize for Chemistry in 1978 so you know, which was quite a remarkable achievement for one person to achieve that.
I 221 So you say its the weight of evidence gradually wins ..

M 222 Yes I think quite often it is , there are some things that are very difficult to get conclusive evidence about

I 225 Ah, there's conclusive evidence and there's the weight of evidence.

M 225 Yes, that's right, I think in these case we're looking at weight of evidence and that weight of evidence is important in making a decision eventually whether this idea is fundamentally right even though you haven't got the details necessarily and something which is quite difficult to prove and he read in the areas of evolution and as you've probably read in the past three or four years all sorts of uprisals and confusions have occurred with and neo Darwinists and God knows what else and they've all got into the melting pot, the creationists have had a great time as well because in the turmoil they picked on various things and said right support our view and..

I 236 Well that perhaps is no different from Mitchell that he ...

M 236 NO NO NO, it isn't and I'm sure its very good to stir up the scientific fraternity from time to time, I know the arguments are very heated but what it tends to do is to make people clarify their positions and to try and do the experiments if that's possible to begin to build this weight of evidence in support of or again ...

I 244 May I ask you if that's a sort of personal criterion that you would use for choosing between two rival theories.. weight of evidence?

M 246 Yes, I think in the main I would, yes I think that's the only thing I could really go on.

I 248 And when you say ....

M 249 Well when I say evidence we look at the range of quantitative data um ....... you've got to be a little bit careful because occasionally you will find on certain issues a huge volume of published materials and you might think because that weighs more that its necessarily better and therefore weighs that tips the balance in favour, now I think you've got to look at the range as well as the sheer volume ...

I 259 The range?

M 259 Yeh, in other words what could happen, I mean I could give you one or two examples, what could happen is that one
particular man really belts out the papers and you know they produce a particular view and on the face of the number of papers you might think well ... 

the weight of evidence ....

Yes, the weight of evidence, but I think along with that you've got to look at the quality if the evidence there and that can be quite difficult, that mean at times if your'e really going to judge that you might have to be fairly knowledgeable in that area of research.

Well may I take it when you say the quality of evidence, you mean how much the evidence actually does provide evidence like for that point of view.

How good was the experimental set up, how many variables did they actually sort of ... iron out um what sort of procedure did they adopt in how many instances could that procedure have been somewhat suspect um ...how far did they budge the issue, now I think there is budging in science.

Could I just zero in on this evidence bit, if you've got a point of view called an hypothesis, what is evidence?

You've got methodological problems etc etc.

Yes, that's right, evidence I think is is er ... um ... the amount of data that emerges of ........ in sufficient variety that looks, it seems to match up from different quarters in support of or not in support of the particular idea principle or hypothesis which you try to formulate.

From other areas.

Yes, I think if you've got er a number ... a different sources these slightly different types of experimental set ups and they all tend to point favourably in the direction of say the Mitchell hypothesis.

So this is the range that you were sort of referring to ...

Yes a sort of range that I'm referring to I think um perhaps ... its difficult to try and find the right set of words if a number of laboratories.

You're talking about coherence perhaps, consistency?

Yes, basically they are coherent in that they add up to a majority of pluses lets say in favour of this idea um what could you do I think is to do, I mean in any specific
place take a table and you look at items A on that problem B, C and you can look at how the evidence cummulatively from a number of sources adds up either positively or negatively, you can actually do a table very crude and say Bloggs and Smith found that um from their experiments using such and such and this sort of method that this was basically supporting Mitchell's hypothesis plus.

You said it was a crude method why crude?

No, I think actually it can be quite powerful in the sense that what you're doing is to add up the pluses and minuses from a whole range of experiments and you can say you know Bloggs and Smith yes this seems to support the idea and it maybe that Bloggs and Smith have a particular set of data which possibly I and another group of scientists think particularly powerful.

Ah, now that's what I was getting at the weighting of each plus and each minues.

But, er how powerful maybe a consensus view and it may not always, its quite a difficult area because I may think gosh that that's a .. I like this experiment you know and this looks good to me and I may take it along to my colleagues and they say yeh, that's really good, and we all agree, you know and then we take it outside and someone says yes this is really a sound bit of work and they've not got any really loose ends they've confined it they've got beautiful technique of showing this, I like it and in other instances and I think techniques are very important you see because alot of things have developed in you know various areas of science and certainly areas of biology because more improved techniques have come to light which twenty years ago were not available and therefore it was very difficult perhaps at that time to do the sorts of experiments which could provide much cleaner evidence.

Cleaner evidence?

Yes, in other words there weren't any fuzzy side issues, now I think with any sort of experiments its very difficult not to get any sort of side issues, I mean I can't think of anything immediately off-hand, I expect there might be

I'm sure there are.

Yes um .. nothing immediately springs to mind but I'm sure that there is ... that ......
I 350 Have you any ideas how you resolve perhaps this contradiction between a table where you've got a sort of cummulative list and the weight ......

M 353 Yes, this is what I call the weighting aspect you see.
I 353 Weighting quantity perhaps we could call it.
M 354 Yes .... yes mm....
I 354 A paradox.
M 354 Yes, you see if I drew that table, it might, the weighting given to a particular sort of experiment might differ from my personal point of view as supposed to a colleagues point of view. Even though both of us might say yes this is a plus in favour of this idea I might think that's really terrific, I like this approach that's being conducted I'd give that a double plus, that's the sort of weighting I'd give it, someone else might not think like that at least they'd think yes that's a plus in favour of this particular idea ....
I 365 Could you speculate on what sort of criteria might bring that sort of difference out between you and others, what sort of fundamental ideas might be at work that make you think well, I give that a double plus but my friend only gives it a single plus?
M 371 ................. Yes I suppose ........ if I had been ... working myself on using a particular technique which I was very familiar with in terms of its sequences and what things could go wrong and I looked at this and I saw that Bloggs and Smith had really um ... you know ...... find out what I would call some of the technical challenges, the technique they present and um they'd sort of for me in reading this they pinpointed this and said we managed to do that and we managed to get rid of that and pick up in the system etc etc, um
I 386 Doing their homework.
M 386 Now again because its unlikely that a colleague would have exactly, I mean occasionally he would find someone in the school, because you've probably both actually work side by side its likely that you both formulate a very similar opinion and if you both work collectively and collaboratively on a problem then its more likely that you know ... if it were someone who was basically in the same sort of area but whose interests was slightly different who had been researching using techniques which were somewhat different from yours
they might also view it favourably but they may just feel that it wasn't perhaps, I think there is a certain amount of flexibility and freedom I can't quite point, I can't quite put it into words between different persons, I mean I said earlier on that if we asked four or five reporters to report on an event independantly inevitably a scientist has a certain amount of training as a journalist I suppose but are the way we see the problem, the same problem but we may actually see it in a slightly different way and we may also judge the importance of certain items of evidence or words in the case of the journalists certainly issues that have cropped up now sometimes we may even exaggerate. I think even at times people may even fudge issues a little.

Er, are you using the word fudge to mean sort of concious or unconcious ...

Well some people ..... as it were intellectual negligence perhaps, I think there's a spectrum here, I think you can go from, I'm sure I mean that when I say that I know from Reports of New Scientists and even in some .... because of the sorts of pressures that are point on people in science to get grants and one thing and another and to be first in bringing out a particular idea ....

Pressures put on them rather than they generating them to be first that is, what I really want to ....

Well, personalities vary enormously, if you're in a fresher situation that you're Postdoctoral Fellowship depends upon you getting money and how good the research you've done up to that point determines whether you get the money or not, I think the external pressures are put on you. Now some people don't concede to those pressures, they know that they are there but they'll not in any way prostitute their science for the sake of just getting some money but clearly there are times when there are some unscrupulous individuals or because they just push to far and they feel that they must take just a few short cuts, there have been some instances in science of that, that's the extreme at the other end of the spectrum there's the absolutely totally honest person reports everything, won't let anything go by even if he's only got er . thirty six results, thirty five out of thirty six results which all seem to point in one direction and you get one result which seems to be absolutely hay wire. I think probably alot of people presented with that perhaps rather odd sort of result and if they can't make sense of it may well sort of ignore it um .. probably one spurious result in thirty six is not going to make much difference even if you do some statistics on it um .... I think .. you know it is very difficult to
know what really happens in any laboratory um .......
I think as far as on a personal concern ........ I would say that there would be very few occasions that I have not put every piece of relevance into the melting pot, there might have been occasions at the beginning when I was trying to develop a technique and I was unhappy with whether I had got the technique right and I got some results and I probably wouldn't have included those eventually, when I was happy with the technique I would have then started from there but you could argue about and say why didn't you put in the four and five ...

Am I correct in saying that this was when you were developing the technique, prior to publication.

That's right, prior to publication, now I would imagine that most people are very honest and very straightforward I just don't know the level, but clearly since there are dramatic misleadings and fudging of data I would imagine that there is a spectrum of fudging as opposed to downright dishonesty um . at the other end total and utter honesty, you know, so scientists like everyone else are individuals and subject to individual variation and all the vagaries of pressures and other events in their environment.

I think actually on the whole um science in this country is .. somewhat less pressurized financially than perhaps the States I mean we're under pressure because there ain't much money but I think there's not quite the um ............ the same types of pressures which exist from what I understand

Can you elaborate a bit of difference and quality of the pressure?

............... well ........ that's very difficult

I mean I have my own idea.

Yes that's right ............... I think most people would like if they're in a sort of leaders in a field of research they like to be first and its pretty disappointing if somebody beats them to it. There is alot of work going on in the States and its funded from a whole range of people and what appear to be very odd sort of funding agencies, certain things that goes on, I mean even the military can fund certain sort of biological things, not related to biological warfare at all. Um .... I have seen the papers you know sort of the metabolism of moths sort of thing and you see the funding agency is the USA Army or something and you know I can't quite fathom this out but still. The ..... but the way in which I think ... but I think rather that money is somewhat more generously given in the States but the competition is fiercer and I think there is a much
firmer time constraint put on them and I think this is where the differences lie um and at times may lead to people being quite competitive and desperate to be there first um .. and it may tempt them at times anyone, in any walk of life to try and cut corners. I don't think I can be more positive, you know more succinct than that. 

Well I think, Its difficult actually.

A very difficult area.

If you could just ...... well you can't get any more succinct than that but if you could just draw out perhaps some evidence of things you are saying like there's hunches and hypotheses, there's weighing up of evidence in terms of both quantity and range or weight, sorry weight, and then there's a range of new view which you seemed to understand in terms of how sort of related to areas which it wasn't specifically desired to talk about was my impression so perhaps you could say that that sort of compatibility er .. or coherence with or consistency with, the surrounding matrix or that in terms of actually informing the rest of the matrix.

Yes.

At which both.

I think both possibly might be true actually .. you see I think ........ I mean science is a creation of man and I mean man is a very complex ..... person I mean man and woman, I don't just mean male, um therefore to think coming back to by earlier point, I mean the tendancy perhaps initially is to think of the various, the scientific method as reflected by published papers which tend to adopt a particular sort of format um .. but I'm sure the development of ideas and ... which is one thing, because I think you see there are people who very much ideas people and ....

When you talk about that chap Lawrence, a great experiment in science and it sounds like he was a great theorist perhaps ..... 

I suppose the socalled great experiments are a retrospetive judgement.

Ah, experimentalists, someones seems to be good at handling experimental situations, then there are other people who perhaps Eistein who perhaps wasn't a very good experimentalist but he was a great thinker, was that the sort of thing ...
Yes that's right, I think there are both sorts of people in science and I think there's a need for both sorts of people in science um ...

Do you think you could perhaps just pinpoint if you can what you think is perhaps wrong with the journal type expedition .... they're saying that alot of people are tending to think that is the way things are written up in journals is their scientific method. You said that you think its unfortunate or words to that effect.

Well I don't think it reflects really what's gone on, and it certainly doesn't reflect the way in which science is actually done.

It certainly doesn't, in what way does it certainly not?

Because I think that when he writes the paper you are much more in the picture from when you started, certainly in a sense when you started on a problem, you probably didn't know exactly where it was going to take you, and you didn't know what the results were going to be or one shouldn't have done or what was the point in doing it. You had done some previous reading, so that way would be the introduction. The likelihood is however in the course of doing your experiments you would come across some other things that you wouldn't of read and that would also colour later on the sort of introduction that you're going to make to your paper. er. so I think that the end result of the paper has been your views sort of modified and retrospective in the light of that and you can see perhaps more clearly I mean you're trying to make the thing clear to readers but from the point of view from the way its written up it should be clear and I mean it shouldn't have all the dilemmas and doubts and so on that probably were originally in your mind when you started on that piece of work, so in that sense in reports eventually what took place but what it didn't do was to show how the product really emerged from its origins... it does so in some degree but it does it in what appears to be a very logical format but quite often its not always like that. Now I'm saying this, and I'm saying it really in the context of biological science. now ... I'm not sure if this would also be true of chemistry and physics or some other science, if human nature's the same then probably there are some parallels to be drawn on but ....

...... no I don't think so but the human element comes into it so, it may make it easier, I gather, I'm not an expert
is the area but just looking at some of the papers that
are published in chemistry for example and talking to my
chemistry colleagues who are sometimes a little bit
cynical about the volumes of chemical papers that come
out even by standards of current publication in science
that, um alot of these papers ... well I've even heard one
of my colleagues saying not really worth the paper they're
printed on um ... in which they perhaps start with a
molecule and modify it and there's you know a new sort of
molecule emerges, I suppose it could advance scientific
knowledge, I suppose it does and it may be that it will
have more important repercussions, not immediately but
later on and that has often been the case with scientific
publications that seemingly on the face of it the
publication isn't quite adequately and competently done
that doesn't appear to advance the scientific world in
any way.

Have you got any personal criteria for judging or
weighting whether or not or to what extent advancement
in scientific knowledge has been made?

that's a very difficult question
actually ........ there are times I'm sure that certain
papers come out which um appear to make really quite a
major contribution to our understanding of the problem ..
and yet there have been instances of papers which have
seemingly been rather obscure or published in rather
obscure journals which twenty, thirty years down the road
have er really been very significant but perhaps people
did not really appreciate them at the time um ..... for
example I suppose as we were talking about Mendel's paper
earlier on, I mean that was quite a remarkable ...

And a good case in point of it not really being ....

That's right.

That's right, it hadn't changed the paper but in a sense
I suppose Mendel was rather ahead of his time.
He was a modest man, and the fact that he published in a
rather an obscure journal didn't exactly .. and at that
time communications were not as they are today um but if
the um ...... if we can sort of believe the way in which
the scientific establishment reacted at the time and
certainly when Mendel was doing his exams and um .. I think
he failed his biology.

Did you see that recent documentary on him, very good?
Yes, well if one can accept that as a reasonable um historical representation of events that took place then I think that you can begin to see what I mean by the scientific establishment not liking to suddenly see someone as a apparent student coming up with a novel idea and it ... that's why I come back to someone like dear old Chan, who's a lovely chap and comes out with some total nonsense, what appears to be total nonsense but also its refreshing and one shouldn't sort of dismiss him and say Chan you know you're talking a load of nonsense why don't you stick to the facts man and don't rock the boat. I think it is at times good to rock the boat.

Are you saying really that there isn't, you haven't criterion judging whether somethings an advancement or not or is the a difference between an absolute decision or a decision between well there are difficulties and I might be wrong? Yes I do have criterion myself as to whether I think there's an advancement....

Yes I think there are, there are times when certain experiments have been done that have really cracked a thorny problem, um oh yes, I'm sure of that.

So its cracking a thorny problem ...

Yes, it may not always be an experiment, sometimes its a series of experiments or experiment and sometimes its merely the putting together of some ideas in a totally fresh way, I've already sited Peter Mitchell, but you see basically what Crick and Watson did was to put together some ideas in a very different way. Now I know they had some experiment but primarily that paper was more a thinking ..... Right, it was exercise in model building.

It was an exercise in model building and theory but at that time they had some experimental data actually I think that they personally hadn't done, yes it was the King's College group that had really done most of the spade work experimentally, to xray um but bless them, they came up with the ideas and then had advanced but I think anybody even then certainly can conceive that that was a major advancement in science. I think you can if you've been involved in science for a number of years you can begin to see papers that you know do

It sounds rather mysterious.

Yes I know I think there is alot of mystery to it to
some extent, and this is a very personal view and er some people feel very much, and probably if I'd have been asked this question twenty years a may have come up with a much more boom, boom boom boom answer yes I can see it, the more time goes on up not saying the more cynical I become, I'm not a cynical person but the more that I feel that it's not quite as cut and dry as one would like to think .. um . yes I do look at the clarity with which people have formulate their idea on that hypotheses and do look at the cleanliness they have conducted their experiment, remember that I have already indicated that I think that they've ironed out the technical problems that they've done sufficient experimentation which um produces results which you can quite clearly subject to statistical analysis and they pretty clearly show that more or less unequivocal support for the hypotheses which was set up. There are some papers like that and I think its not difficult to find them when they come I think you can say that's a nice piece of science um ... doesn't always necessarily at that moment in time advance science that much.

I was just going to say that couldn't all those criterion that you sited be implied to the chemistry paper that's not worth the paper its written on.

Well that's not my quote.

That's your anonymous colleagues. Nevertheless that's a justification ....

Yes, I'm sure that's right, and that's why they're accepted for publication presumably because most papers nowadays um .. are not usually just accepted without some sort of referees comments um there are risky journals and there those regarded as more prestigious than others.

Very much so yes.

And probably there are some good cause for that to happen but if its going to be published usually there are people judging because quite alot of papers are totally rejected or are so much rejected that are asked to be rewritten so ... you could send it to a different journal, but a really bad paper is going to be clobbered by almost every journal and there are limits that even the worst journals will accept or not accept . um I ... yeh I think that um ... by the very nature of science and the way that its going and developing, its going in leaps and bounds

An actually assessing the weight of a paper is what you're saying is something that perhaps is intuitive really born of experience and hard to formulise in any kind of
you know, simple way at least, seems to be what you're saying.

I personally would not like to formulise it in a very simple way, perhaps other people, I might have done possibly twenty years ago.

Getting to the point about perhaps other people. um I'd like to ask you a question that is actually related which is related absolutely to what we've been saying about your views on advancement of scientific method and advancement of scientific knowledge I should say and scientific method and er I mean you're something of a God-send to me because you're a professional scientist er you're a teachers of teachers on the PGCE to some extent, er what you're other occupation, you're a teacher of undergraduate scientists, so, on all these areas that I'm quite interested in you've done something or quite alot.

And yet come up with no hard and fast set of rules for you to er ...

Ah, that's an interesting finding in itself perhaps, um you mentioned that you were interviewed in a Chair Science Education and they asked ..... 

I think that there was something on scientific methodology then and I can't remember the answer I gave them, It probably wasn't the right one (laughs) that they were looking for anyway.

To some extent if you're a professional scientist its a nonimpeachable answer isn't it, perhaps maybe not if you're within education and you're applying ...

Yes, that's probably the reason why I'm somewhat more sceptical to er, er a hard set of rules, you know I guess that's probably that.

Well, I was just going to ask, I've got a list here of um

Have I heard of these people?

Well, I'm sure you have, five very influential philosophers of science, now. er I am asking those as though in three capacities as a scientist, as a teacher in undergraduate scientists, and as a teacher of teachers of secondary science um ... could you just give me some general ideas er on you sort of awareness of these names.

Yes, well what do you want me to say about them?
Well, um, without actually saying what you know about each one just to say roughly how familiar with the ideas of each one that you feel that you are, for example.

Well I have some ideas about the Baconian philosophy of science which is a rather interesting one but um I think not strictly, it was more or less you know, let's strike a match, let's see what happens sort of thing. Popper I think has been a very interesting person, um ... because not only has he ventured views on um science but obviously he's looked at this within a social and political framework um ...... I think .. although he .. one of the, he's one of the clearer expandists of Marxism is that right ....

Well actually no, I would disagree with that ...

Well, he's been one of Marxisms greatest critics as well.

Well, that's right certainly.

He has presented Marxists views, not from a personal viewpoint, but he's studied Marxism.

He actually wrote two books criticising Marxism.

That's right, he was one of the arch critics of Marxism. He was very influential I suppose with many of the wellknown figures currently in science, like Peter Edinburgh and so on. Um Koon, yes I suppose I've also come across on various .. he's been more involved I suppose in education and science in the context that I've come across him.

Do you mean him personally, or the use of people who made his ideas.

The use of people who made his ideas.

I see.

As far as I'm aware. Um but I'm not expert on any of these people, but you simply asked me had I come across them. Now with Lakatos, the name 'rings' a bell but I have a feeling this is one of John Gilbert, I've seen this in John Gilbert copy.

Actually I see, I am an author of that paper.

Yes, right, (laugh) now you've said that I can't remember
M 210 exactly what I had to say about, I had never heard of it until John I think, John showed me a paper and probably you to had now shown ......

I 215 Yes the paper is actually here.

M 215 Now I thought that Feyerabend, isn't he a sort of mathematical philosopher?

I 217 Well, actually, Lakatos was the mathematical one but um Feyerabend, I suppose he's got quite a facility in mathematics but he's not a philosopher in it.

M 221 Well the last two I would not have heard of, I think um or not gone out of my way to have heard of if we had not met and John Gilbert, I'm sure John Gilbert introduced me .... I haven't actually read anymore about those two but John obviously had been much more in the area of higher education and probably science ... science education um he's a much more widely read person than I am on that particular, on those particular folks. Yes I mean, the first three I have actually read articles and books on and I think that the only claim to knowing anything about the last two names, they register

I 234 Its from the science education.

M 234 That's right, but um frankly I wouldn't know what their basic philosophies are. So that is an awful omission to make ..... 

I 238 But um the thing is you say that is an awful omission but maybe it isn't. You're a successful scientist after all.

M 240 ... Well I think possibly looking at what's happened, successful, I don't know, I'm a jack of all trades with an interest in quite alot of things inevitably by spreading one's interests rather widely one can be open to criticisms for being extremely ignorant in some areas then that probably is true of everyone, but er yes they are people that I have, at least I have actually heard of all of them but I couldn't quote a chapter or verse from any of them.

I 253 Do you think these people have anything to say about the conduct of science.

M 255 I'm sure that they have been very, I'm sure that it is important that, I mean some scientists may feel that they are a pain or something like that, no but I think alot of people must feel that they have, yes I'm sure they have made quite important contributions, not just to science
But to science in society in the broadest context, they've certainly been very influential, certainly Popper and Kuhn, have been influential with some very evident people in science um influence their way of thinking and so on, so that er and I think they've probably even influenced er as I understand it, of the limited books and so on that I've read of their work and of their thoughts, I'm sure they have influenced the way, some ways in which science education has developed .... possibly secondary education in particular, I'm not sure about tertiary education and probably to some extent even there.

Do you feel that the ideas of such people should be aired in say in an undergraduate's science degree?

Oh yes I think they should, and indeed um at this University I believe some of them are because there is of course, it is an optional course, alot of students opted for it, called the principles and perspectives of science and I guess that you probably have contact with that area.

Yes I have, but it is an optional course.

It is an optional course.

It used to be compulsory at one stage, I think, it is now optional, so when I was asking do you think the views of such people should be um discussed or examined or whatever within an undergraduate science degree course I was actually putting it rather more strongly saying

Should it be compulsory.

Should it be compulsory.

I suppose if I was speaking purely personally, I'm not speaking collectively for the school, .... I would like to see it as an integral part of an undergraduate course.

And why would that be?

Partly because it comes back again to my comments earlier about my feelings on history of how science developed, a university in my view is a place which should broaden one's knowledge on a subject, ... I understand why the situation is as it is at the moment because the very nature of the English degree course which is usually a three year course means there is an awful lot being put into three years, and I'm sure there wouldn't be unanimous agreement as to exactly what should form the three years
and whether the weighting is indeed right or not, um ... anyone who is certainly thinking of going into teaching, anyone who is interested in the way in which scientific ideas have developed as well as the history of science would do well to have studied these people ... its um, well that is my view um ........ you could ... I suppose other people could justify not putting them in or strongly feeling that they shouldn't come in. Well they might say that the students don't know enough science er ... and I may even have some sympathy with that because it reflects on what I've said earlier on, I appreciate the history of science and possibly Peter Medawar and co who are very much influenced by people like Popper, and ........ we appreciate these views of science ... um .... with more experience of science, now that is not to say that students couldn't appreciate this I don't think, but some people might argue that they don't know enough about science before they can then put these views into perspectives of science.

What you're insinuating that in every secondary school science curriculum virtually you get as one of the top objectives that students should learn not only science but also about science, something about the approach, the scientific way of thinking.

That's right.

My view seems to have replaced the view that used to be attributed to classics as a training for the mind or whatever, now arguably people like Leeds have devoted their life to talking about science.

That's right.

Um and so what I'm really asking is er, do you feel though that as a practising scientist yourself, that in actual fact there's enough about science that comes through, perhaps not formulated, perhaps not within a sort of specific system but there's enough that comes through there's enough that's learnt about science for that as an objective to have been carried through into say tertiary education.

Again from a purely personal point of view, though I think the answer to the question simply would be yes. Um but I think that you could get a different answer from a number of different people. But as I've already said that I think that um in many ways I wish that looking back my undergraduate time was doing science had had the social and philosophical aspects of science included in the education um ... rather
than just, well I mean I could be quite critical of some aspects of it but um ... there were an awful lot of factual things I think um ... and alot of it did amount to rote learning, there was almost zero philosophy er of science and scientific developments and scientific thinking, .... I'm not sure whether I would have, I mean looking back I think it would have been lovely to have had that in the course, whether I would have appreciated it at the time, I don't know. This is the thing I find it quite difficult to know, and er as I say periodically one has to try and put oneself in the student's shoes.. I think the answer will be that there will be a body of students who are fascinated by the history, philosophy and the social impact of science and indeed the way in which science has changed sort of during their education.

Well, can I put it this way, if we consider an undergraduate perhaps in quite a narrow way that is as some kind of pre-condition for becoming a professional scientist, do you feel that the history, the philosophy associated with names such as Do you feel that that's er .. actually necessary as part of becoming a professional scientist or at least that it should be a necessary part of the undergraduate degree? You said that it would have been lovely, but was it necessary?

..... In retrospect I think it probably was even necessary, I regret not perhaps having more opportunity to study these aspects than I have, I welcome it here um ..... the difficult one is should it be .. and I think the answer again, I've already given is yes, um I can see that it might, in a sense the students should have some element of choice within the course that they do here for three years, and indeed they do have that, and part of the choice comes in the first year when they choose this course or not, a large proportion of them do and in a sense when you opt to do something, probably you're more motivated to do it. If you're compelled to do something and you're one of the I don't know, 20% who really is not turned on at all it could probably be a real drag and actually may do quite the reverse of what you the organiser of the course had hoped to do and that was to stimulate thought and ideas in these areas and again a somewhat different perspective to science.

Could I just, now that we're very nearly the end, ask you a couple of more questions quite briefly, um firstly what contribution if any to the questions and ideas that your undergraduate students make with your own research interest?
M 001 I can't actually say that they've made any great impact into the um, research that I've done in biology, where they have probably had some impact, I'm not quite sure how great, but where they had had some impact I think is in looking at ways of developing teaching and learning situations for first year students. The development of sorts of materials that we had done and constantly on the look out for ways of improving themselves as self dependent persons, I've done a sort of literature thing, a fairly trivial thing, you see its where to know where to get the information not necessarily how much you've got stored up here and that's it sort of thing. Where to find information, and how to use it is very important.

I 017 So this has influenced you in the way that you influence teachers which are training to become secondary science teachers, is that what you're saying.

M 018 I think there must be some influence, I couldn't put a finger on it, and I couldn't say ....... I'm sure it must have because obviously if you're interested in teaching, learning situation you're constantly reviewing what things seem to be good and what are right and what things need improvement and of course even your own personal ideas of science change as you go on, I mean I look back on some lectures or tutorials that I gave when I first came to this place and frankly I sooner forget them, at the time one did one's best but you know you think Oh my Lord they weren't good at all, they were for example the lectures were probably too detailed um they assumed knowledge that the students didn't have and all these sort of elementary things that um you would I mean what one did was to fall into the trap having spent sometime in secondary education and then realising you've got to know what your starting point is and what you can or cannot assume, if you're not careful you'll rush in the first couple of years here thinking ah yes these are all bright students, I can assume this, that and the other. Well frankly there are times when you couldn't assume those things are assume too much of them and this may have been reflected in the essays or the exam questions that they actually did. So rather than say oh well the students are think I just continue as before and they just must get brighter, I think for anything in life if its not going exactly as you'd like it to be, the first person you look at is you, now if you can then eliminate .................

M 048 You look at yourself first.
Yes, that's right, I think you see, far too often in life people always blame someone else for their own misfortune, for their misdeeds.

And this perhaps ties up with what you were saying about self-sufficient.

Yes that's right.

So you apply it to yourself.

Um ..... could I ask something .. that perhaps we didn't touch upon with the ..... You're a professional scientist as well as a teacher of teachers, what about the average um teacher, secondary science teacher who has perhaps done a degree at most in science and then becomes a teacher of pupils in science now bearing in mind my earlier remarks about the objectives of knowing something about science um .. have you any views whether or to what extent the ideas of philosophers like Popper, Kuhn, Lakatos, Feyerabend ought to be included in a PGCE course for example.

Oh yes, I think they should and indeed I would think that Sussex, I don't know whether it's unusual, I don't know in many ways it is, Sussex very much looks at the development of self, self development and that the very nature of the course is one which includes within the scientific context not necessarily me introducing it but there are a range of people whose interests are philosophy, although they may have actually trained in science, they may have actually trained in arts.

This is within the PGCE course at Surrey, er Sussex?

I think that certainly Kuhn and Popper, um .. to a little extent Feyerabend but certainly Kuhn and Popper would have had, would have been introduced at some point or other by someone or other in the context and er advice perhaps reading some of the materials that these authors produced, as regards again the last two I don't know. But I know that Kuhn and Popper have come up many times.

They do actually as part of the .. I'm interested to hear that, I tell you why because I did my own PGCE at Kiel oh only two or three years ago 1979 and there was no mention of them whatsoever, of philosophy of science or of Popper or Kuhn, or infact no mention whatever of the conduct of science. Specific method here and specific techniques there.
Well, I um have thought, I mean I know that for example Dick West, you know because .... and Michael Brown for example, ..... both very much fascinated by the history, philosophy and development of science and that is one of their areas, and certainly many of the things that they do, .... I can't imagine that they don't mention some of the ideas of Popper and Kuhn at some stage ....

I must West and Brown but I was thinking of yourself ....... and things here at Sussex because my only acquaintance in any detail with the PGCE is with and its a very good PGCE in many other respects in its rating and so on.

I have to admit I probably don't say a great deal about those two people um ....... I suppose again .... I mean no excuses I mean one could take a different view but what um if anything I tend to look at perhaps a more sort of philosophy and look at how development of childs thought processes and the way in which they manage science um ... rather more than perhaps philosophy as such. I'm sure probably along the way I may vaguely come up in passing mention someone like Kuhn and possibly Popper but I don't make a big song and dance of it no.

Is this probably the case with your colleagues as well? Do they mention them, would it be as it were to illustrate a point rather than .......... Or they might direct it in er .... you see alot of it is is a sort of tutorial discussion, a seminar workshop situation and they have once a week with me a curriculum workshop and they have then a workshop with the general tutor ...... or might be someone like Mike Brown. Within that context you see, it may be that the general tutor for example would look at the total curriculum and ask more general questions, and its more likely that those folks could come up within that context. Um ... in the early stages I suppose one would focus, not suppose one does tend to focus on fairly mundane things of just simply coping in the first term, probably just coping with sort of the classroom situation.

Perhaps that how its got to be.

Well it has to be that way, I think because those are where the students greatest concerns are, we hope that in the second term they are more reflective, it may well be that in the second term and possibly towards the
end of the second term and third term some of them may well get into these areas of their own choice and but I mean there is no sort of philosophy of science course in the PGCE in which there's various influential figures ... take quite a major role.

Could I just ask you one final thing now, have you any views concerning the role the general public should or does play in deciding science policy?

Mm

I can give you a sort of stimulus if you like, very recently on the back page of the THES there was an article entitled Science and People and it was a discussion between James Watts and Steven Rhodes.

That's right, I happened to read that one, that's right.

Yes so they take, Steven believes that everyone should be involved .......

James Watts .... at least said that research on the should stop and now he believes it is wrong and scientists should keep quiet about it.

Well democracy, I suppose is people theoretically should have a say, um but er .... at times I think there are certain issues in which the public, actually even voting quite honestly I suppose they ought, obviously in democracy they ought to have a vote, but alot of people aren't terribly well informed, more often than not its a gut reaction, I mean the classic example I suppose is nuclear reactors, the word nuclear immediately conjures up horrors of bombs and God knows what else and ......... actually the nuclear reactors are remarkably safe, the big challenge as far as I see it are the disposal of the waste material and I suppose in the first instance even the mining of radioactive material, because someones got to mine the stuff and transport it and one thing and another. Now those are probably where the greatest hazards occur and not so much in the nuclear power station itself, I'm sure its a frightfully safe place, its probably a damn site safer than going down a coal mine or on an oil well. Now has I said, the other issues are things like um, currently of course in biology the people who don't want any sort of experimentation with animals, they confuse dissection with vivisection, and want all animal dissections banned at school because they're hurting the animals, well they're not really because all dissections at school are done ... I mean I can
understand this, passioned view um but I think its always very difficult, eventually the government decides on things whether you like it or not anyway. People, I suppose should have an opportunity to air their views, there should also be some sort of forum where at least even if ...... some people form very hard and fast view on something on pure gut reaction, they don't actually have the evidence, they don't really check something out um and as a consequence they're really quite ignorant, now we're all if we're not careful tend to talk off the top of our heads on certain issues and ...

Well you see I put it to you that there might be a problem with them to become ..... 

A thing you've got to be very careful about and where scientists have a responsible I think is to try and inform the public as best they can, bearing in mind that many of the public are not trained scientists so it has to be point across in a language that is basically understood by them.

Now should this precede or for what experimentation you see, like er Watson seems to be arguing that in fact its a mistake to have public debates and things before the research goes ahead because you just get this uninformed ....

I have some sympathy with that view actually, I really have some sympathy with that view um ...... Steve Raders well .... um ......... I have to be very careful that I'm not coloured by, I like Steven alot but he, he er he does jump in at times, again its one of these, I think there is a distinct danger in sort of separating science and saying well the scientist know what they're talking about, you know you're the ignorant public, the scientist will decide what is best for you .. um that probably is not very wise people then start to be more suspicious of science then some of them are already .... um ........ what probably needs to be done is, as I say to ...... provide with in any child's education um ... a basis of science education ...... so that at least um the majority of the public do become somewhat informed about science. Inevitably whatever they do their lives are going to be affected by science, its impossible not to be, and so ideally I suppose it would be nice if they could be informed as much as possible.

Being informed or in on the decision making policy.

I don't know eventually, there are some decisions I don't know if the public have really the knowledge to make decisions on. And I know that can be a bit dangerous and I
know, I realise that but um, you can say that someone deciding for me on the other hand the are I think probably occasions ....... um ....... well let's take something like an 'n' policy, I know most governments don't have any on term and energy policy but still um its all very well perhaps on the one hand to say well you know we should abolish all, if you took a total gut reaction, and put it in a, put the emphasis on nuclear all the time, alot of people would say oh no steer clear of that at all costs, but what needs to be done is probably to weigh, given that there are certain risks what are the alternatives, what are the alternative risk.

Well maybe that can be publicly debated.

Yes, I think that that probably could, but again you see as with politics there are alot of people who um ....... are basically disinterested in about everything, they go along and stick their vote in and they probably vote um according to what their parents voted, in alot of cases, my parents always voted Labour so I always vote Labour type thing, or my parents always voted Conservative so I always vote Conservative and ....... occasionally that way I don't get embarked on politics, that's not the point, but some people I just, whether you would really get a true cross section view because some people are just going to be basically disinterested and not take part in the debate anyway.

Well I just wonder whether there could be a thing like Jury service, where you could actually have, you go on the Jury to consider such and such aspect of science policy and you actually have like a court with scientists who if necessary would employ middle men to actually translate their ideas into sort of a language which is assessible and even to the extent of having a few, saying it was an aspect of biology you could actually have someone to say right OK we're just going to have an introductory lesson on what a cell is, we can now consider this part and then we'll have morale arguments that the scientists view and then we going to have a counter argument from this conservationist and then we want you to make a vote, and your vote will be taken as a societal ... that sort of thing.

Well, I mean something along those lines wouldn't be a bad thing, at least, it may not necessarily reflect the whole of society.

I don't think it could possibly do that.

No, but on the other hand at least the issues in reasonable terms, in reasonable terms could be put and people
And people could at least consider what the issues are, rather than just have a vote on a gut reaction, you know all nuclear power stations are that.

You could even have them televised on another channel purely for democrat, I always feel that television is very under-used medium for democracy.

That's right, that's right.

Indeed interactive video would be a whole new ball game in democracy.

That's right, that's right, it's very interesting, it's an interesting issue, they've actually taken DNA and er ....... for the majority of the public I suppose they just don't know that they fear that we could be making clones of Hitler or something and they don't want anything to do with it. Or that some awful bug sort of got out of hand and just wiped us all out.

Does this .... worry one time ......

They should be concerned I suppose, but before any decisions were made I should have thought that it was important to put the major issues to the public as well as possible, well let's put it in a different way, a thing should not necessarily be voted cut on a gut reaction if it has agree with that potential, um it should be thrown out simply because a majority lobby thought Oh God I don't really know about this, but it sounds awful, you know we'll either have a nuclear bomb or bug sort of thing. They'll say absolutely no to this er ........ because I think people's natural tendency is fear of the unknown and because we fear the unknown we must reject it rather than saying well we don't know alot about this, we have reservations about it but perhaps what we should do is to look at the facts as far as we're able to judge them and see what the issues are, see what the two opposing views, put them forward, given that there are certain facts, and those facts don't change its just the view you place, the bias or the emphasis or whatever that you place on those facts, ... its like er ... but of course the human element comes in time and time again.

Well I think that's what we're talking about.

That's right you see if you take, in the world take a stupid example, someone, the optimist can see half of beer in a pint mug and say its half full and a pesimist can say its half empty, now it doesn't alter the situation because
M 363  half a glass and half a pint of beer in a mug, likewise
in any other situation, there are what I would call the
moaners in society who see a situation and can only see
what is bad in it and there are the optimistic or say that
situation, the same situation well what could be good in it,
its not saying that they don't realise that there isn't
another point of view.

I 374  Its just that you use your own analogy to weigh up evidence
perhaps, pros and cons, pluses and minuses. Well we've
gone on very much longer they I'd intended I'm really very
grateful indeed.