AN ANALYSIS OF SOME PROBLEMS ASSOCIATED
WITH TEACHING MATHEMATICS TO SCIENCE STUDENTS

by

JON NUTTALL

Thesis submitted to the University of Surrey for the degree of Ph.D.

April 1975
SUMMARY

The analysis of some of the problems associated with teaching mathematics to science students provided in this thesis is a philosophical one, and as such claims to clarify rather than solve these problems. An important aspect of the thesis is the criticisms it offers of the philosophical and methodological assumptions underlying research within educational technology. In particular, the attempt to state the end point of educational processes in terms of precise statements of behaviour has been criticised, not by suggesting that there are other, non-observable (i.e. mental) outcomes, but by demonstrating that the intended behaviours cannot be stated in advance.

This criticism has been based on Wittgenstein's analysis of understanding, which Wittgenstein calls a 'grammatical' analysis since, crudely, it consists of looking at how words are used. The purpose of the analysis, in this thesis, has been to show that educational technologists, in looking for behaviour corresponding to understanding, have misunderstood the use of the word 'understanding'.

The context for the discussion of understanding has been provided by Kuhn's view of the nature of science and the analysis has been linked with Wittgenstein's criticism of the traditional notion of a concept since this has a bearing on assumptions made about what is learnt in the mathematics class and taken over into the science class. This analysis if accepted, demonstrates that in order to study the problems associated with mathematics for science courses, one cannot specify, in behavioural terms, what the student must transfer. Instead, and this is the main conclusion of the thesis, one must look at the criteria for understanding used in the two subjects.
I wish to thank the following for their suggestions, criticisms and support: Professor L. R. B. Elton, Institute for Educational Technology, University of Surrey; Miss P. Smart, Department of Philosophy, University of Surrey, Mr. M. Williams; and Mrs. J. Thackwray, who typed the manuscript.
# CONTENTS

## PROLOGUE

- Introduction ............................................... 1
- Review of the literature .................................... 7
- Discussion .................................................... 13
- Conclusion .................................................... 16

## Chapter 1: Mathematics and Science

1.1 A View of Science .......................................... 20
1.2 The nature of Learning for Science .................... 24
1.3 The Learning of Mathematics for Science ............. 29

## Chapter 2: The Systems Approach

2.1 Introduction ................................................ 33
2.2 Logical Empiricism ......................................... 36
2.3 Logical Empiricism in Science ............................ 39
2.4 Empiricism in the Behavioural Sciences ............... 43
2.5 Limitations of the Use of Objectives .................. 48
2.6 Implications for a Technology of Education .......... 54

## Chapter 3: Concepts in Science and Mathematics

3.1 Wittgenstein's Analyses of Concepts .................... 57
3.2 Concepts in Science ....................................... 67
3.3 Mathematical Concepts used in Science ............... 72

## Chapter 4: Criteria for Understanding

4.1 Introduction ................................................ 75
4.2 Examples of Particular Circumstances ................. 81
4.3 Bloom's Taxonomy of Educational Objectives .......... 85
4.4 Assumptions about Learning in the Taxonomy .......... 90
4.5 Terms Defined by the Taxonomy ......................... 95

## Chapter 5: Gagne's "Conditions of Learning"

5.1 Types of Learning .......................................... 102
5.2 Relevance to Teaching Problems ......................... 106
5.3 Language Learning ......................................... 109
5.4 Criticisms of the Behavioural Framework .............. 118

## EPILOGUE

................................................................. 123

## REFERENCES

................................................................. 138
Introduction

The aim of my research has been to investigate the problems associated with teaching mathematics to science students. This thesis is the result of this investigation. At first, my investigations were empirical: I was closely associated with various educational innovations in the teaching of mathematics. The purpose of my concern with these innovations was largely two-fold: first, I hoped to gain experience in ways of assessing the effects of innovations and thus gain deeper insight into the specific problems associated with teaching mathematics to science students. Second, it seemed that it might be possible to recommend a way of teaching mathematics to science students based on one or other of these innovations.

It was through my involvement with such projects, and through my attempts to find ways of assessing their effects on other parts of the students' course, that I came to realise that a more general investigation, not related to a specific course or institution, was necessary. Further thought convinced me that before empirical solutions to the problems associated with teaching mathematics to science students could be obtained, various philosophical problems would need to be carefully examined. Therefore, this thesis has become a discussion of these philosophical issues. Nonetheless, a brief account of early work on the practical projects will serve as a starting point for the discussions that follow:
A Calculus course for ex-arts students

Several science and engineering departments of the University of Surrey used to offer four year honours courses, called B courses, for students with a predominantly arts background. (These B courses were discontinued in June 1973, due to falling student numbers.) The first year was an introductory year to prepare students for the normal three year honours course, and covered large parts of an "A" level physics and mathematics course. While one or two of the students had done "A" level mathematics, the majority had only done "O" level, often some time previously. Prior to 1971 the mathematics had been taught by an orthodox lecture system. However, in an attempt to cope with the differing backgrounds and speed of working of the various students, this was replaced, in October 1971, by a Keller plan system.

A Keller plan course is one in which the course material is divided into units, each of about a week's work, which the students are expected to work through by themselves. Each unit contains a list of objectives which the students are expected to achieve, references to a text book (in this case, the main text was 'Quick Calculus', a programme text by Kleppner and Ramsey), additional notes, examples and problems, and also a short test based on the objectives. The student, after working through the material in the unit, has to present himself for a test when he feels he has mastered this material. He is only allowed onto the next unit when he has passed the test for the previous unit, equivalent tests being available if the student fails at the first attempt.
As well as acting as a tutor on this course, where I was able to gain first hand experience of the sorts of problems encountered by students of low mathematical ability, I was also engaged in research aimed at comparing a Keller type course with an ordinary lecture course.

Various aspects of my research were: to identify particular difficulties students had with the material in the units, to record students progress through the units and to assess students' reactions to the course. I was also able to assess longer term effects, of insisting on mastery of objectives for each unit, by means of an end of course examination. This examination consisted of two parts: part A contained short questions similar to those in the unit tests, part B contained four longer questions, from which the student had to answer one. The questions in part B were all based on the material taught in the course, but required some application. It was found that the number of units covered in the course proved a good predictor of how well students performed in part A, thus suggesting that the achievement of the unit objectives ensured a certain minimum standard.

However, there was no similar relation between units passed in the course and performance on part B of the examination, and this might suggest that achieving course objectives does not necessarily establish, what might be thought to be, a higher level of understanding. The question remains as to whether it is possible to produce objectives which would ensure this understanding.

(b) Kings College - Calculus Crash Course

The calculus crash course at Kings College, London, was started in the academic year 1970-1971. Prior to this there had been a "Maths for Biologists" course. Following a decline in the mathematical standards of entrants, the chemistry and engineering departments of Kings College decided to start a remedial programme, and this was
amalgamated with the Maths for Biologists course. I became involved with the course in its second year. The arrangements, which differed from those in the first year, were that the course was held all day on each of the first six Wednesdays of term. Students for the course from the chemistry and engineering departments, were selected on the basis of their performance in a shortened version of the PIP Pre-knowledge test (see later, for further details), that was given to all first year students in these departments. Those students scoring less than 60%, and all biology students, attended the course.

The text used was 'Quick Calculus' by Kleppner and Ramsey. This was divided into six, approximately equal sections, one section to be covered in each of the morning sessions. The morning sessions lasted from about 10.00 to 13.00 with free coffee at about 11.00. The atmosphere was very informal: tables were arranged in groups, staff and tutors were introduced by their first names and discussions with tutors or friends was encouraged. The afternoon sessions had no set text and consisted of films or demonstrations followed by discussion. For the afternoon sessions the attendance was optional and none of the work was examined.

In the seventh week the students were given a test, similar to the test they had taken at the beginning of the term, but with one important difference from the first test: namely that students were allowed as much time as they liked, whereas in the first test students were only given an hour. (Many students had complained about the lack of time in the previous test, especially those that had arrived late.) The extra time allowed was certainly one factor in the improved performance in the second test, where all the chemistry and engineering students achieved over 60%. Even so, I think that the results still
demonstrate an improvement in the students' performance.

Another aim of the course was to change students' attitudes to mathematics, and part of my evaluation was to measure the extent to which this had been achieved. I therefore designed and gave out a questionnaire which I hoped would measure attitudes to calculus and to mathematics in general. This questionnaire produced some predictable results, such as: that engineering students considered mathematics more important and more relevant to their main subject than did chemistry students, who in their turn considered it more important and relevant than did biology students. The questionnaire also confirmed the general impression I had that students had enjoyed the course, and that this was because of the easy and informal atmosphere, the approachability of the staff and because students were able to work at their own speed. However, it is difficult to ascertain whether this positive attitude to the course had any long-term effect on the students' attitude towards mathematics, and whether it in any way changed students' behaviour.

(c) Physics Interface Project

The Physics Interface Project (PIP) was set up by the six universities of Cardiff, Chelsea, Keele, Birmingham, Surrey and York to investigate the problems arising from the transition between school and university. One of its tasks was to measure the mathematical abilities of the student intake into each of the physics departments. It was a generally held opinion that physics students were experiencing difficulties with their mathematics and also that this might be in part due to a decline in the mathematical standards of student entrants. Therefore a test was needed to verify that the mathematical ability of science students was poor and to discover the extent of the deficiencies in students. It took the form of a multiple choice test covering all the mathematical
skills considered to be necessary pre-knowledge for a physics degree course and it was given to all first year entrants in each of the physics departments involved in the scheme.

The results showed that there were several topics, supposedly covered at "A" level, in which the performance of a large number of students was inadequate. Having demonstrated this, the next step was to remedy these deficiencies. One suggestion was that I should prepare a number of programmed packages covering these topics, and that these packages would be given to those students failing corresponding questions on the pre-knowledge test.

When preparing these packages, the first problem I had was to find a suitable way of presenting the topics. The information obtainable from the test answers was limited and merely indicated that a certain number of students had failed to answer certain questions but not why. The questions tested mathematical skills and had been designed specifically with a physics degree course in mind. I therefore thought it inappropriate to treat the topics in the same way that they would have been treated in an "A" level mathematics course, but instead tried to teach just the mathematical skill. Therefore I first produced an analysis of how I thought a person might go about answering a question of the type given in the pre-knowledge test. The analysis was then the basis for a programme that tried to teach students how to answer such a question.

A small pilot study was carried out on a group of sixth form pupils of mixed ability so as to identify any difficulties students might have, due to ambiguities and poor explanations in the text. Several students commented that the programmes had been useful in clarifying
several points with which they had previously found difficult. It was not possible to further validate the packages prior to their use since I was unable to obtain a large group of representative students. To compensate for this I prepared pre- and post-tests which I had intended students taking the packages to attempt in order to obtain a measure of the successfulness of the programmes. However, due to various factors outside of my control, this was also not possible and so I was unable to evaluate the programmes.

Review of the Literature

It is so generally accepted that there are problems associated with the teaching of mathematics to science students, that no references are required to substantiate this. I carried out a review of the literature with the purpose of finding out what research had been done on this problem, but was unable to find an account of any systematic research. The bulk of the literature consisted of descriptions of new approaches to teaching particular topics or descriptions of new syllabuses. To the best of my knowledge, these innovations were not the result of a systematic analysis of the problem nor were they followed by an evaluation of their effects. This finding that reflects the lack of systematic work in the area suggests, I think, that innovations are normally based on a teacher's new idea or desire to teach a topic in a particular way, that there is little concern for evaluation and that there is a real difficulty in knowing what and how to evaluate. A more useful type of article that also appeared in the literature was written by the person, experienced in the teaching of mathematics to science students, giving his own personal opinion, in the light of reflection on these experiences, as to the
A report, that does not fall into any of the above categories, entitled 'University Training in Mathematics for the Future Physicist', prepared for the International Congress of Mathematicians (International Congress of Mathematicians 1966) was useful for describing the state of mathematics teaching in the majority of physics departments, in the U.K., just prior to 1966. The report was prepared by analysing the results of a questionnaire circulated to physics departments of Universities and Colleges of Advanced Technology. According to the report, the mathematics that is taught to physics students is "sometimes regarded as a separate subsidiary subject and sometimes as an integral part of the physics course, but the distinction between these two is mainly administrative" (p. 2). There were no institutions where the mathematics teaching was done entirely by physicists, and in those cases where physicists taught some of the mathematics, it was generally courses such as classical mechanics and methods of mathematical physics. Therefore in the majority of institutions, according to this report, all or some of the mathematics teaching was "service teaching". The report goes on to draw attention to the difference in outlook between the mathematicians and the physicists. The writers of the report suggest that for "the physicist, mathematics is a tool, and it will be more attractive to the student if he is clear from the outset what kind of purpose each tool has been designed to serve".

It must be pointed out that this report concerns only mathematics for physics students, and not scientists in general. Also in the eight years since it was written there seems to have developed a greater appreciation of the role mathematics plays in science. For
example, Elton suggests that "mathematics is more than a mere tool to the scientist. Furthermore, as a language it is different from that used by the mathematician, when dealing with mathematics."

(Elton 1971, p.77) He goes on to quote Bondi, who unfortunately uses the term tool both in the way in which Elton uses it, and to refer to the role of mathematics as a language. Nonetheless, it is still possible to see that Elton is suggesting the same as Bondi:

"I use mathematics as a tool in two ways. First in the specialised way that the particular manipulative skills and ability to handle certain types of data are greatly helped by the particular kind of mathematical education I have received, and second, in the way that the abstract ways of thinking which arise naturally in mathematics, are very often of use in the less familiar fields of science where we deal with knowledge gained from conditions far removed from those of everyday life."

(quoted from Bondi 1966)

As Bruckheimer and Gowar point out: "There is an all too prevalent view, both within and without many educational establishments, that mathematics is merely part of an engineer's tool-kit" (Bruckheimer and Gowar, 1968). Flegg also points out that, in his opinion "the users have done themselves and their subject a disservice whenever they have approached mathematics purely from a conventional user point of view - the 'mathematics as a tool' approach" (Flegg, 1974 p. 66). Flegg goes on to suggest, a little later in the same article, that to "assume that a facility with mathematical techniques is all that is required as the end-product of a 'mathematics for science' course, is to presume at the same time that the mathematical models used in science and technology have nothing to offer to the understanding of the physical situations which they are used to represent. This is to make a presupposition which is entirely unjustified." (p. 68)
Skellam, in an interesting article on teaching mathematics to biologists, suggests three features of a healthy relationship between mathematics and science: "1. Language is the vehicle by means of which knowledge finds expression and through which it is communicated. 2. Mathematical systems can be regarded, if we so wish, as rational languages in skeletal outline. 3. Empirical science aims at developing a coherent body of reliable knowledge and rational understanding through the systematisation of sense-experience." (Skellam 1972, p. 147) Thus we can see that there is a body of opinion, of which the above are examples, to the effect that mathematics is more than a tool for manipulating data in science, but also is used as a language to model the physical situations with which the scientist is dealing.

There has also been some discussion as to how to solve the problems associated with teaching mathematics to science students. The most common suggestion is for the mathematics to be taught by a joint team of mathematicians and scientists. Elton points out that:

".....until recently the standard pattern in the teaching of mathematics to others has been that it has been conducted by mathematicians who sometimes had and sometimes had not found out beforehand what the non-mathematicians required them to teach. The unsatisfactory nature of this has long been recognised and has led in some instances to the mathematics teaching being taken over by mathematicians who had transferred their activities and their allegiance to the non-mathematician's field - theoretical physicists, mathematical economists, etc. This too is unsatisfactory, since it tends to result in ad hoc syllabuses that lack generality. At last, the realisation is dawning.....that what is needed is a collaboration on equal terms between the two sides.....Ideally, it should be done through team teaching and, failing that, the mathematician who has transferred his allegiance to the non-mathematician's field, may be a suitable go-between."

(Elton 1971, p. 78)

Mathews and Seed, when discussing the relationship between mathematics and science in schools, conclude by advocating that:

"(i) Mathematicians and scientists should meet together more often to discuss common problems
and possible areas where an integrated approach would be fruitful. (ii) Some timetable provisions should be made at all levels for such interdisciplinary work, which might well take the form of modules or projects and involve some degree of team teaching."

(Mathews and Seed 1970, p.2)

As Flegg points out, this is the sort of approach that has been adopted by the Open University, where a course in 'Elementary Mathematics for Science and Technology' has been produced by a course team consisting of pure and applied mathematicians, scientists, technologists and an educationalist. Sellars (Sellars 1972) has also spoken of the need for liaison and co-operation between mathematics and science departments.

However, although there are various demands in the literature for team teaching, there appears to have been little detailed discussion on how these teams are going to collaborate. It is implied that the content of a mathematics course can be decided by a discussion between mathematics and scientists, but as Malvern points out:

".....to restrict discussion of teaching order or teaching approach solely to a consideration of content seems short-sighted. Some consideration of the learner, and of the kind of processes involved in learning two closely related subjects is necessary. It is generally agreed that there is usually little transfer between these subjects without some elaborate prompting...... It is not sufficient, for example, to draw the students' attention to the sameness of a given topic when it re-appears in a physics lesson after being taught in the mathematics course. Even where an 'efficient' liaison exists between departments there may still be problems."

(Malvern 1971, pp 1-2)

Nonetheless, despite having said this, Malvern goes on to reduce these problems to organisational ones. Also, he does not appear to appreciate the role that mathematics plays as a language, and argues
solely in terms of responses to stimuli:

"...it seems likely that a high degree of transfer can be achieved when both subject teachers expect the same kind of answers to similar questions."

(Malvern 1971, p. 3)

As I shall discuss in greater detail later, this may well be true when mathematics is used as a tool, but it does not apply when mathematics is being used as a language.

I think it is fair to summarise the literature by saying that it is generally recognised that there are problems associated with teaching mathematics to science students, but there has been little systematic research into the causes of the problems, and, as far as I know, none at university level. Research that has been done, for example by Bajpai and his colleagues at Camet (see, for example, Bajpai et al. 1970 and Bajpai 1972), has been concerned with developing alternative ways of teaching certain topics, often using multi-media approaches. The consensus that emerges from general articles is that problems have been caused by treating mathematics as a tool that is used by the scientist and ignoring its role as a language. There is also agreement on the need for co-operation between mathematicians and scientists. However, no-one has appeared to consider the way in which team teaching will bring about a solution of the problems arising from mathematics being used as a language, rather than simply as a tool, in science.

In addition to the specific literature on the teaching of mathematics there is also a large body of more general educational literature that is more or less relevant to the issues of this thesis. Much recent work, on learning problems, has been carried out in educational
technology. However, unfortunately, for reasons we shall consider later, such work has as its basis the formulation of precise statements of behaviour. This naturally favours the teaching of skills and so, if applied to the problems associated with teaching mathematics to science students, would lay emphasis on the use of mathematics as a tool rather than as a language. Later in the thesis we shall come across, what I take to be, fairly representative samples of this literature, and I will analyse the contribution of such literature to our problems. At this stage there is, I feel, little to be gained by a prior description of the literature.

Discussion

The experience of both Kings College and the PIP universities provide evidence to support the claim that the mathematical abilities of students are unsatisfactory. One possible reason for this is the general shift away from science, resulting in university departments having to accept students with poorer mathematics "A" levels. If, in fact, a significant proportion of the student intake is of low mathematical ability, then remedial programmes at the beginning of a university course may well be giving more work to those students who are already having difficulty in coping with their work load. Hence it may be that a major re-structuring of teaching, and a cut-back in the material a student is expected to cover, is required. However, the consideration of such a proposal is beyond the scope of this thesis.
Although the two remedial programmes I studied presented their material in different ways, there was nonetheless a similarity in the sort of material they presented. Both the Kings College course and the PIP programmes were intended to teach students to do something, such as integrate, differentiate, sketch graphs, etc. The Kings College course did try to go beyond this by also considering students' attitudes, but on the whole, skills were being taught in both cases, as is shown by the use of similar tests. Although the skills tested by the PIP pre-knowledge test would appear to be necessary prerequisites for the use of mathematics as a tool, to the best of my knowledge there has been no test developed to measure whether a student is able to use mathematics as a language.

Now, it is often thought that students experience difficulty with their mathematics in science courses because they have failed to transfer what they learnt in their mathematics class to their science class. However before we can talk about a 'transference problem', we must be clear about what, if anything is being 'transferred'. The answer we give will, I suggest, depend on whether the mathematics is being used simply as a tool or whether it is also being used as a language. If only the first of these roles is fulfilled, then, the transference problems may be studied by isolating the particular skills that are necessary and devising tests to measure these skills.

However, I do not think problems arise just through the failure to transfer skill in the use of mathematical tools, from the mathematics to the science class. It is often the case, for example, that students can follow each stage of a mathematical argument in science, thus showing that they can use the tools, yet are still left with a feeling of uncertainty. This is, I think, because the student lacks
an understanding of mathematics as a language. When mathematics is used as a language the transference problem becomes more complex. It is obvious what is meant when we talk of transferring a skill from one subject to another, we can readily see that the same skill is being used in two different sets of circumstances. But, what is meant by 'transferring understanding'? This, as we shall see, is not simply a matter of defining 'understanding'.

When mathematics is used as a language, as well as a tool, the suggestion to teach the mathematics as part of the science course, rather than as a separate subject, has major drawbacks. If the mathematics is used simply as a tool, then the transference problem may be solved by integrating it into the science course, since the student will immediately see the relevance of the mathematics, will know why he is learning it and how it is used. Such a situation arises, for example, in psychology and sociology where statistical theories do not, in general, model the phenomena, but are used as tools to enable the mass of data to be handled. Statistics could therefore be taught as an integral part of the main course and this would avoid students having to learn statistics in abstract situations that bear no relation to the real situations in which it is normally used. If however, the mathematics is used as a language to model the phenomena being studied, then the above approach is in many ways analogous to teaching language from a phrase book. As Elton suggests (see above) this leads to ad hoc syllabuses that lack generality; the student can manage in those situations for which he has learnt appropriate phrases, but without grammatical rules, he cannot generalise to other situations. This is probably even more serious for mathematics than for a natural language, since the mathematics will lack any of the overall structure which is its most significant
An alternative way of considering the transference problem, which at first sight appears to avoid some of the problems associated with understanding, is in terms of concepts. One understands a language when one has grasped the concepts of the language; similarly we suggest that in order to understand the language of mathematics students need to learn the concepts of mathematics. In this way we can reformulate the problem of transference by saying that, in his mathematics classes, a science student learns both skills and concepts, and that these then have to be applied in the science class. This suggests that the transference of concepts is similar to the transference of skills, for, just as the same skill can be isolated from different situations, we are suggesting that a concept can likewise be recognised and isolated in different situations. A similarity between concepts and skills is implicit in the way we talk about 'grasping' and 'applying' concepts. If our reformulation is valid, then it suggests that the transference problem, whether it involves skills or concepts, can be tackled by evaluating the learning of skills or concepts in the mathematics class, and measuring the extent to which transference occurs.

Conclusion

As can be seen, my early work into the problems associated with teaching mathematics to science students was empirical. I was concerned, directly or indirectly, with devising new methods of teaching,
with specifying precise objectives to describe what a student needed to learn, with devising tests to measure these objectives, and with producing attitude questionnaires to assess what science students felt about mathematics. It might be thought that such an empirical approach would eventually yield solutions, or at least partial solutions to the problems, and indeed, this is one of the assumptions underlying educational technology. In spite of this, I became increasingly dissatisfied with an empirical approach and abandoned it in favour of what might be called a philosophical investigation. It is clearly necessary to give some justification for this. However, no short explanation can be entirely satisfactory, and in a sense, my entire thesis is an attempt at such a justification. At this stage, I can only outline some of the reasons for my dissatisfaction with my early work.

First, I was concerned that mathematics is, in general, considered in isolation from the science in which it is used. Even where a statement of aims of a mathematics for science course is based on an analysis of the science course, the mathematics is subsequently taught in isolation from the science and it is difficult to assess the effect that the mathematics course has on students' work in the science course. Second, although there were several suggestions, in the literature, that mathematics is used as a language in science, I could find no further analysis of this role, nor any discussion of how this insight into the role of mathematics might affect the teaching of mathematics for science. Third, I was unhappy about the almost exclusive concern with behavioural objectives within educational technology which I have briefly mentioned, and the
assumption, without any attempt, to my mind, to justify it, that all talk of understanding could be given a more precise expression in terms of specific behaviour. Although I appreciated the usefulness of behavioural objectives, I could not accept these extravagant claims that were being made.

What I felt was, not that a philosophical investigation could solve the problems associated with teaching mathematics to science students, but that such an investigation was necessary, prior to an empirical solution. This is because, I think that there are problems of a philosophical nature, that invariably arise when we try to attempt an empirical solution. For example, supposing we assume that the problems, or at least some of them, associated with teaching mathematics to science students, arise because the students fail to transfer their learning from the mathematics class. The empirical approach that appears to be called for, is to measure what has been learnt in the mathematics class, measure what is known in the science class and thus find out how much has been transferred. However, sooner or later, we shall come to the question: how do we measure understanding? Further, if it is understanding that is being transferred, is it meaningful to speak of 'something' being transferred at all? These questions are not empirical ones, they are philosophical ones.

I am well aware that the above argument will, in all probability, fail to convince someone committed to an empirical solution to educational problems. Such a person will probably accept that there are difficulties associated with his empirical approach, but that these are practical difficulties, which it is his job to remove. Unfortunately, he will not easily be dissuaded from this view, since
no empirical investigation will convince him that his difficulties necessarily arise out of the nature of his empirical approach. What is needed is a philosophical argument; but it is just this sort of argument whose necessity the person denies! Consequently, my above comments are not intended to, nor could they be expected to, force such a person to revise his opinion. Rather, it is the purpose of the thesis as a whole to do this. Therefore, I suggest that the thesis as a whole is read before any judgement is passed as to whether I was justified in undertaking a philosophical investigation, bearing in mind, at the same time, that the results of a philosophical investigation cannot be the same as, nor a substitute for, the results of an empirical investigation.
1.1 A View of Science

There is, as we have seen, a growing realisation that mathematics is not simply used as a tool in science, but also as a language. One consequence of this, as regards the teaching of mathematics, is that the mathematics should not be treated in isolation from the science. Although it is, to some extent, possible to isolate the mathematics teaching, where the mathematics is used as a tool, this is not possible where it is used as a language. Instead we must explore the relationship between mathematics and science.

One question that must be considered is whether the function of mathematics as a language differs according to the particular science we are concerned with, that is, we must consider whether there are any fundamental differences between the various sciences, or whether one particular science is a model for all. Thus our investigation is concerned with the philosophy of science. Kuhn, in his book 'The Structure of Scientific Revolutions' (Kuhn, 1970) approaches the philosophy of science from the history and sociology of science, and suggests that there are fundamental differences between different sciences. Each science is based on its own particular paradigm and a science such as physics, for example, cannot be considered as the example of what science ought to be like. Although Kuhn's work is still the subject of much debate in the philosophy of science, I think that this does not affect its significance for science education. This claim will, I hope, be borne out in this chapter.
One of Kuhn's main theses is that the popular view of science as an evergrowing stockpile of knowledge cannot be supported by historical evidence. Although much of science is directed towards cumulative development, and this research Kuhn calls 'Normal Science', in contrast to this there are times when research is directed towards overthrowing previously accepted ways of viewing the world. Thus Kuhn replaces the picture of science as a cumulative progression, with a picture of science progressing through a series of revolutions each followed by periods of normal science. The significance of this is that, although old scientific theories are discarded, they cannot be considered as being, in any way, unscientific. Therefore, Kuhn suggests, historians of science must accept the fact that science has included bodies of belief that are incompatible with the ones held today, and that there must, consequently, be an element of arbitrariness in the beliefs that are held today.

When working within a community of social scientists, Kuhn noted that there was considerable discussion about, and disagreement over, the nature of the scientific problems and the methods of social science. This does not occur in the physical sciences, where there is, in general, widespread agreement over fundamentals. This agreement has not, however, always been a feature of the physical sciences, and Kuhn therefore set himself the problems of discovering what it is that transforms several competing schools into a unified field, in which the practitioners accept the same underlying view of the world.

Central to Kuhn's solution to this problem, is the concept of a 'paradigm', which Kuhn, at one stage, describes as: "universally recognised scientific achievements that for a time provide model
problems and solutions to a community of practitioners." (Kuhn 1970, p. viii). Thus the classics of science such as Aristotle's Physica, Newton's Principia and Opticks, etc., "served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners." (p. 10). Since the nineteenth century textbooks, which "expound the body of accepted theory, illustrate many or all of its successful applications, and compare these applications with exemplary observations and experiments" (p. 10), have served the same purpose. Therefore, normal science is research that is firmly based on a paradigm. Although scientific achievements become paradigms because they are successful in solving a particular problem or group of problems, they are never completely successful, nor do they solve all problems. Rather, the paradigm is a promise of future success. Thus normal science "consists in the actualization of that promise, an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself." (p. 24) This 'mop-up work', as Kuhn calls it, does not aim at novelties (p. 35).

Nonetheless, occasionally problems arise which appear to be capable of being solved by the paradigm, but which resist all attempts at solution. In time several such anomalies can lead to a period of what Kuhn calls 'Revolutionary Science', in which the old paradigm becomes increasingly unacceptable to a small group of scientists within a field. A new paradigm is looked for, and if one is found which is successful in removing the anomalies, and also promises to be as useful as the old paradigm in explaining other phenomena, it is eventually accepted by the majority of scientists within the
field. Thus, according to Kuhn, revolutions occur in science when a new paradigm replaces an older one, and this is accompanied by a basic shift in beliefs and commitments.

The notion of a paradigm is clearly fundamental to Kuhn's work, and therefore it is necessary to clear up the confusions that resulted from Kuhn's use of this term. In the postscript to the second edition of his book, Kuhn discusses these difficulties. One of Kuhn's students, Masterman (Masterman 1970) isolated twenty-two different ways in which the term paradigm was used. These differences were largely due to stylistic inconsistencies, but nonetheless, three distinct meanings can be isolated. The first of these is what Masterman calls the 'sociological' meaning where a shared paradigm is used to define a group of scientists. As Kuhn states in the postscript, this use is unnecessary since it is possible to isolate the various groups of scientists without recourse to shared paradigms. Thus this use of 'paradigm' can be eliminated fairly easily.

This leaves us with two other uses, which Masterman calls the 'metaphysical paradigm' and the 'artefact or construct paradigm'. Having isolated a group of scientists we can ask what it is that they share, and the answer Kuhn gives in the body of the text is 'a paradigm', where this is taken to mean a body of beliefs and commitments, or more generally, a world-view. In the postscript, Kuhn acknowledges that the use of the term 'paradigm' is inappropriate here, and suggests the term 'disciplinary matrix'. The main sorts of components in such a matrix are, Kuhn suggests, common symbolisms, commitment to beliefs in particular models, and values. There is also another element, which is a commitment to the problem-solutions that serve
as shared examples, and for which the term 'paradigm' was originally introduced. Thus Kuhn comments that:

"...in much of the book the term 'paradigm' is used in two different senses. On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science."

(Kuhn 1970, p. 175)

The term 'paradigm' is appropriate for these puzzle-solutions, since they serve as a pattern, or paradigm in the normal sense of the word, for further research.

1.2 The nature of learning for science

It appears to be commonly accepted that a science student spends more time engaged in 'cognitive' rather than 'affective' learning. According to Bloom et al, the term 'cognitive' covers "activities such as remembering and recalling knowledge, thinking, problem solving, creating" (Bloom 1956 p.2), and these activities are distinct from such things as "interests, attitudes and values, and the development of appreciations" (p.7) which are covered by the term 'affective'. Pring suggests that this distinction "rests upon the belief that the cognitive capacities - the ability to know and to think and to understand - can be conceptually isolated from the feeling side of mental life and analysed without reference to it" (Pring 1971, p.85/6, original italics). He continues as follows:
"It does not make sense to have knowledge as one's objective......without caring about those standards of truth and correctness which are built into what it means to know and to understand and appreciate. To think scientifically entails a concern - a feeling, if you like - for the standards of scientific truth."

(Pring 1971, p.86)

This criticism is accepted by the authors of Volume 2 of the Handbook, on the Affective Domain, who point out that the division into cognitive and affective is an arbitrary one, that reflects the way in which educators have traditionally classified objectives, rather than a basic distinction between behaviours. (See Krathwohl, Bloom and Masia 1964, p.47)

A similar point can be made, based on the arguments so far developed in this chapter. Kuhn suggests that part of a student's learning involves coming to share the same disciplinary matrix as shared by practitioners in the field; that is, the student becomes committed, not only to the same symbolic generalisations and theories, but also to the same beliefs in certain types of models and explanations, and to the same sets of values. If we ask how the student comes to share this disciplinary matrix, the answer Kuhn gives is that he does so by studying the paradigm, that is, by studying the concrete puzzle-solutions that are found in text books, by applying theories to problems, by doing examples and gaining practice in the laboratory. Thus, it is through what might be thought of as cognitive learning that the student also learns things that might be thought of as coming under the heading of affective. Moreover, as Pring suggested, this affective learning is not just something which happens to accompany cognitive learning, it is an essential part of it.
If the supposedly cognitive learning of scientific concepts did not also involve an affective part, normal science would not be possible since, normal science requires, as Kuhn suggests, a commitment to a paradigm. That this commitment exists is shown by the incommensurability between paradigms. In choosing between two rival paradigms, Kuhn insists that "each party must try, by persuasion, to convert the other." (Kuhn 1970, p.198) Therefore, Kuhn suggests that, to:

"...understand why science develops as it does.... one must understand....the manner in which a particular set of shared values interacts with the particular experiences shared by a community of specialists to ensure that most members of the group will ultimately find one set of arguments rather than another decisive."

(Kuhn 1970, p.200)

In criticising the use of the terms 'cognitive' and 'affective', I am not suggesting that we should simply be more precise in the way we apply these labels, nor that we must alter the way in which we apply them, but that learning is far more complex than is implied by the use of these labels. Thus, although it may be possible in theory to separate the learning of scientific concepts and theories, which might be thought to be cognitive learning, from the learning of beliefs and values, which is thought of as affective, we must not think that there is a corresponding distinction in practice, nor that the cognitive and the affective are two components of learning. Kuhn points out that it is often thought that "scientific knowledge is embedded in theory and rules; (and that) problems are supplied to gain facility in their application". However, he continues by saying:
"I have tried to argue that this localisation of the cognitive content of science is wrong. After the student has done many problems, he may gain only added facility by solving more. But at the start and for some time after, doing problems is learning consequential things about nature."

(Kuhn 1970, p.188/9)

It is also, I suggest, learning consequential things about how scientists view nature. Therefore, at the start, doing problems may well involve learning that might be classed as affective as well as learning classed as cognitive. This implies that it is not simply what the student is doing that determines whether learning is cognitive or affective, but also the extent to which the student already shares the disciplinary matrix that forms the context for the paradigm.

Thus, it seems to me that not only is the division of learning into cognitive or affective domains based on a naive theory of knowledge, but it also has various attendant dangers. Although the distinction may be a convenient one, traditionally made by educators, and although in some instances learning may well consist of a purely cognitive component, problems are likely to arise due to the fact that the classification of learning into cognitive and affective is normally done by a member of the community, sharing the particular disciplinary matrix. Such a person, while able to identify cognitive learning, will probably be unaware of the affective learning that exists for the student who does not yet share the same values. Thus, there is a tendency to perceive the difficulties experienced by weaker students as being less than they are.

It is reasonable to suggest that for any piece of learning that a subject-expert classifies as 'cognitive', the degree of affective
learning also involved will differ from student to student, and
this will not depend simply on their previous immediate learning
experiences, but on, what we might call, the character and attitudes
of the students themselves. Once we recognise that so-called cog­
nitive learning is inextricably connected with affective learning,
we can begin to see attitude problems in a different and, I suggest,
more useful way. Instead of trying to measure likes and dislikes,
which can reflect so many different factors, we become more concerned
with the extent to which a student is able to share various commit­
ments and values. It is clear that if affective learning 'contam­
inates' the cognitive learning of a science for some students, this
'contamination' is likely to be considerably increased for the
majority of science students studying mathematics.

The values expected by the mathematics teacher, concerning such things
as accuracy, consistency, simplicity, elegance, clarity, degree of
generality, etc., will be different from those values that the science
student is in the process of acquiring from the study of the paradigms
in his science subjects. We can illustrate the different values that
will be expected by the mathematician and the scientist, by consider­
ing one of these: generality. If we consider the solution of a second
order differential equation, then the mathematician will be interested
in the general result that there will be two arbitrary constants.
The scientist, on the other hand, will be more interested in how to
find these constants in particular situations, and thus, in a sense
he is interested in a more specific result than the mathematician.
However, the scientist is also interested in the fact that a second
order differential equation has general application to a variety of
different sorts of physical phenomena. Thus, on the one hand, the mathematics teacher will expect, what might be termed 'mathematical' generality, and on the other hand, the science teacher will expect, what might be called, physical generality.

1.3 Learning of Mathematics for Science

Now consider the following quotation, bearing in mind that we are not concerned with learning scientific concepts, but with mathematical concepts that occur in science:

"Scientists......never learn concepts, laws and theories in the abstract and by themselves. Instead, these intellectual tools are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications.... (which) accompany the theory into the textbooks from which the future practitioner will learn his trade...... The process of learning a theory depends upon the study of applications, including practice problem-solving both with a pencil and paper and with instruments in the laboratory."

(Kuhn 1970, p.46/7)

The implications that we can draw from this are that the problems that science students encounter with mathematics are, in part due to the fact that mathematical concepts and theories are learnt "in the abstract and by themselves". It is only later that they are fitted into a paradigm that shows their application. However, we cannot, I feel, simply suggest that the mathematics should therefore be taught within and as a part of the science course. Apart from leading to a phrase book approach to language learning, such a suggestion also ignores an important role that mathematics may
play in science learning. Where mathematics is used as a language, one of its functions is a descriptive one. Mathematics is not simply an abstraction from various physical situations, it is also a language that describes those situations. For the student, such a language, if he can use it competently, may serve to demonstrate important similarities between apparently different phenomena. If the student simply used mathematics as a tool, then he could be provided with a pocket calculator. The fact that this would not be an adequate substitute for mathematics learning, points to the role that mathematics also plays as a language.

For example, electrical vibration is given by the formula $E=E_0 \sin \frac{2\pi t}{T}$, mechanical vibration by $x=x_0 \sin \frac{2\pi t}{T}$, and air pressure (sound) vibrations by $p=p_0 \sin \frac{2\pi t}{T}$. One way of expressing this is to say that a formula of the general form $y=y_0 \sin \frac{2\pi t}{T}$ represents a vibration. However this ignores the fact that it is not self-evident that an electrical field is in any way similar to a rigid piece of material or a volume of air. Consequently, it is not at all obvious that there can be a phenomenon, such as an 'oscillation', which is common to each, nor that these oscillations can be described by the same mathematical formula, or the same type of mathematical formula. To learn this is, therefore, to learn something significant about the phenomena being studied. Thus it does not do justice to the use of mathematics as a language to say that formula $X$ is an abstraction from the physical situations $A$, $B$, $C$,..... If we were to view the subject from the student's point of view, we might find that initially he sees the formula $X_A$ as modelling $A$, $X_B$ as modelling $B$, etc., and that only later does he see that $X_A$, $X_B$, $X_C$,..... are all forms of $X$. Once he sees the mathematical similarity between $X_A$, $X_B$, etc., this will help him to
Now, in the above example, the similarity between the three forms of
the equation \( y = y_0 \sin \frac{2\pi t}{T} \) is very obvious, but this need not always
be the case, e.g. Heisenberg's and Schrödinger's formulation of
quantum mechanics. Nor is it obvious that the formulae \( mg = \frac{d^2s}{dt^2} \)
and \( mg \sin \theta = -ml^2 \frac{d^2\theta}{dt^2} \) are both forms of the formula \( f = ma \). However,
without being aware of this similarity the student will not fully
appreciate the similarities between free-fall and the motion of a
pendulum. Further, it is through recognising the appropriate formula
as versions of the formula \( f = ma \), that a student is able to develop
his concepts of 'force', 'mass' and 'acceleration'. (For a similar
point see Kuhn 1970, p.188/9) Thus unless the mathematical similarity
between the formulae is understood, the student will not fully under­
stand the meaning of these formulae nor will he appreciate their
descriptive role.

Before concluding this chapter, it should be noted that Kuhn continues
the passage quoted at the beginning of this section as follows:

"If, for example, the student of Newtonian dynamics
ever discovers the meaning of terms like 'force',
'mass', 'space' and 'time', he does so less from
the incomplete though sometimes helpful definitions
in his text than by observing and participating in
the application of these concepts to problem-solution."

(Kuhn 1970, p.47)

While agreeing with what I think Kuhn is saying, I must disagree
with a possible implication, which Kuhn may or may not have intended.
What this passage seems to imply is that a concept may be defined and
then applied, and that it is this application of a concept that gives
the terms their meaning. However, I think it makes more sense (and
this will I hope be brought out more clearly in the following chapter),
and does not significantly alter Kuhn's account, to suggest that the student learns the concepts 'force', 'mass', etc., (that is, as Kuhn says, the meaning of these terms) through the applications of these terms to problem-solution.

Thus, I am suggesting that although scientific terms are carefully defined, a definition, such as, for example, that 'force is the external agency which is capable of imparting motion to a body', is totally inadequate at conveying the concept. A concept cannot be given by a definition, for even when the student has learnt the definition, he will not be able to apply it. He can only learn the concept by learning the use of the terms of science. This use, as Kuhn implies, is learnt through examples and practice. We shall see later that this must significantly alter our ideas about the transference of learning from mathematics to science.
2.1 Introduction

Over the past decade or so a more systematic approach to education has developed within the new discipline of educational technology. Davies (Davies 1971) distinguishes two separate sources of educational technology, which he feels should be brought together to produce a 'systems approach' to education. I am not here concerned with what he calls educational technology 1, which is based on the use of hardware such as tapes, T.V., etc. However, I shall be concerned with what he calls educational technology 2, which involves:

".....the application of behavioural science to the problems of learning and motivation..... This view of educational technology is closely associated with the modern principles of programmed learning, and is characterised by task analysis, writing precise objectives, selection of appropriate learning strategies, reinforcement of correct responses, and constant evaluation."

(Davies 1971, p.7)

Thus the basic approach to evaluation that Davies puts forward is a system in which one formulates aims, derives precise objectives from these and then tests these objectives. Other evaluation strategies have been developed, for example, Eraut points out the need to assess whether course aims are worthwhile and to take into account such things as practicality, cost, etc. when evaluating a course; (Eraut 1973) Parlett and Hamilton suggest the importance of an anthropological approach, which they call 'illuminative evaluation' (Parlett and Hamilton, 1972). These alternatives stress the fact that a course is more than just its aims and objectives, and that when evaluating
a course one must consider all the factors that interact to produce a particular teaching and learning situation. However, in this thesis, I am not concerned with the evaluation of particular courses, where one has to consider all relevant factors, but with the general problems associated with teaching mathematics to science students. Therefore I wish to consider whether this general problem can be tackled by the sort of process Davies suggests, even though it may be argued that in particular circumstances there may be other factors that are important.

As we shall see, it is a basic directive of this evaluation strategy that the intended outcomes must be stated in a behavioural form in order to evaluate the observed outcomes of a course. A distinction is normally made between an educational aim, which, according to Goodlad (Goodlad 1966, see Popham et al. 1969, p.35) is a 'remote end for the guidance of educational activity" and an educational objective, which is a "statement of what students ought to know, be able to do, prefer or believe as a consequence of instruction". Therefore, the first question I would like to consider is, why it is thought necessary or desirable to specify precise descriptions of behaviour to replace more vague educational aims.

I think that two reasons can be given for this, and these both involve a criticism of the use of words such as 'understanding', 'appreciate', etc. The first criticism is that words such as 'understand', 'appreciate', 'enjoy' have vague meanings in everyday speech, so that aims expressed in terms of these words do not convey very clearly what it is one wants the course to achieve. If educational technology is to be at
all 'scientific' in its approach to educational problems, then it must carefully define and restrict the meanings of the words that it uses. Unless the meaning of words such as 'understanding' are clearly defined, it will be impossible for those involved in education to communicate unambiguously with each other.

The second reason for formulating precise behavioural objectives is based on a more fundamental criticism of the use of words such as 'understanding'. In order to appreciate this criticism we must look at the history of Behaviourism, which, in reacting against a psychology based on introspection, committed itself to dealing entirely with observables and hence aligned itself with the philosophy of logical positivism, later to be known as logical empiricism. As Sockett writes:

"The 'behavioural objectives' model of curriculum planning rests on a methodological behaviourism with connections to both operational definitions and a verification theory of meaning."

(Sockett 1973, p.38)

If this is so then it cannot be claimed that the systems approach to educational problems, adopted by educational technologists, is simply a naive empirical approach that requires no empirical justification. As soon as it is recognised that the methods of educational technology are based on certain philosophical assumptions, then it must also be accepted that a philosophical criticism, as well as criticism at a more practical level, is valid. This is particularly so under circumstances where either the underlying philosophy itself has been severely criticised, or where methods based on this philosophy appear in principle, as well as in practice, to have little chance of success. I hope to show in this chapter that both these conditions apply. The philosophical arguments that I shall put
forward to criticise the methods of educational technology will rely on the later philosophy of Wittgenstein, in which he is critical of his earlier contributions (albeit unintentional) to logical positivism.

2.2 Logical empiricism

The fundamental tenet of any form of empiricism is that knowledge is based on experience of the world and does not exist prior to experience. To this logical empiricism added the necessity for a logical analysis of the language in which the knowledge was expressed. It is possible, suggested the logical empiricists, for sentences to appear to be statements of knowledge when they are not. Such sentences include all or most of those of meta-physics and religion. Sentences formally true (such as those of mathematics, which according to Russell, and the Wittgenstein of the Tractatus, could be reduced to tautologies), nor were capable of verification or falsification by experience, were thought to be meaningless. Thus, in order to distinguish what they thought to be sense from what they thought to be non-sense, the logical empiricists proposed the following criterion of meaning: any meaningful statement should either be capable, at least in principle, of being tested experimentally, or should be capable of being translated into statements which are themselves capable, at least in principle, of being tested experimentally. As an example, Schlick says that the proposition 'there is a mountain of height 3000 m on the other side of the moon' makes sense since it is capable of being shown to be either true or false (although at the time he said this there was no technical means available for testing the proposition). (Schlick 1959) Thus
all sentences are true, false or meaningless.

The legitimate method of analysis of sentences is by means of the propositional logic developed by Frege and Russell. Thus language was seen as approximating, to a greater or lesser extent, according to how 'scientific' it was, to a sort of calculus governed by precise rules. This enabled the logical empiricists to suggest that, although a sentence may not itself be directly testable, and clearly many sentences of science were not, it should nonetheless be built up from sentences which were testable. Thus according to Ayer:

"The underlying assumption is that there are statements which are elementary in the sense that, if they are true, they correspond to absolutely simple facts."

(Ayer 1959, p.11)

Even though we may never use such elementary statements in the ordinary course of language use, the statements we do use "are significant only in so far as they say what would be said by affirming certain elementary statements and denying certain others, that is, only in so far as they give a true or false picture of the ultimate 'atomic' facts" (Ayer 1959, p.11). Therefore, in order to determine the truth or falsity of a meaningful complex sentence, one first ascertains the truth or falsity of the constituent atomic sentences and then ascertains the truth or falsity of the complex sentences by means of an appropriate truth-table into which the truth values of the atomic sentences have been inserted.

These elementary sentences corresponding to atomic facts, were assumed to be reports of single observations. According to Hempel, an observation sentence is "any sentence which - correctly or incorrectly - asserts of one or more specifically named objects that they have, or that they lack, some specified observable characteristic." (Hempel -37-
Thus we can express the criterion for a sentence to be meaningful in terms of observation sentences by saying that a meaningful sentence is either an observation sentence or can be translated into an observation sentence. All sentences of science were thought to be ultimately reducible to observation sentences; the sentences as they stand are merely shorthand forms of this set of observation sentences. Similarly Carnap argues that "every sentence of psychology may be formulated in physical language", that is, "all sentences of psychology describe physical occurrences, namely, the physical behaviour of humans and other animals", and so physical language is "a language into which every sentence may be translated". (Carnap 1959, p.165)

The proper method of science was thought to be a process of induction starting from single observations and gradually building up more general statements. In order to show how this ideal picture of scientific progress could be reconciled to the actual practice of science, Carnap introduced the idea of an "abbreviated method" which is possible when the scientist is in possession of universal sentences. Thus, "it sometimes seems to be the case that a general law is established on the basis of some single event. For instance, if a physicist can determine a certain physical constant, say, the heat of conductivity of a sample of some pure metal, in a single experiment, he will be convinced that, on other occasions, not only the sample examined but any similar sample of the same substance will, very probably be characterisable by the same constant." (Carnap 1959, p.169) This generalisation from a single event is only possible because, "as a result of many previous observations the physicist is in possession of a universal sentence of a higher order". (p.169)
The universal sentence which allows him to take this short-cut can be roughly stated: "All (or: the following) physical constants of metals vary only slightly in time and from sample to sample." (p.169)

This process of induction as the correct method of science applies not only to the physical sciences but also to the human sciences. Thus psychology should start from reports of simple facts and only later arrive at more general statements. Since man is an animal it is not only legitimate but also proper to investigate animal behaviour as a prelude to investigating man's behaviour, since animal behaviour is simpler. True statements about human behaviour can then be obtained by induction from statements about animal behaviour.

2.3 Logical empiricism in science

Logical empiricism as a basis of science has been criticised by many philosophers of science, from Popper onwards. As Ayer points out, there is a serious difficulty in the case of such universal statements as the one given above by Carnap.

"For while the truth of such a statement may be confirmed by the accumulation of favourable instances, it is not formally entailed by them; the possibility that a further instance will refute it must always remain open: and this means that statements of this sort are not conclusively verifiable. On the other hand, they are conclusively falsified in the sense that a negative instance formally contradicts them."

(Ayer 1959, p.13)

This problem of induction, which has been recognised since Hume, was not solved by the logical empiricists. Russell, who was closely associated with the Vienna circle of philosophers who originated logical positivism, suggested that the principle of induction should
be accepted as a fundamental logical principle, for "without this principle science is impossible". (Russell 1961, p.647)

According to Magee, "Popper's seminal achievement has been to offer an acceptable solution to the problem of induction. In doing this he has rejected the whole orthodox view of scientific method...and replaced it with another". (Magee 1973, p.22) Popper recognised the logical asymmetry between verification and falsification, and incorporated it by suggesting it is the possibility of falsification that distinguishes a science from non-science. Thus, scientific knowledge is based on experience, but in the sense that theories are falsifiable and not verifiable by experience; scientific theories can be, and should be tested by systematic attempts to refute them. Although new theories may be arrived at by a process of induction, this is not relevant to whether or not they are 'scientific'; theories are only scientific by virtue of being testable. As Magee points out, Popper was not, like the logical empiricists, interested in distinguishing sense from non-sense, but science from non-science. Thus Popper accepts neither that induction is the fundamental method of science, nor that statements which are not testable by experience are meaningless. Statements which are not testable are not statements of science, but they may well be meaningful.

Magee suggests that Popper also criticises the logical empiricists for their suggestion that it is possible to start simply from observation, and from these pure observations arrive at more general statements or theories. For Popper, "...the belief that we can start with pure observations alone, without anything in the nature of a theory, is absurd". (Quoted from Conjectures and Refutations,
Popper 1963, p.46, by Magee 1973, p.33) This means, as Popper wrote earlier "...that observations, and even more so observation statements and statements of experimental results, are always interpretations of the facts observed; they are interpretations in the light of theories." (Quoted from the Logic of Scientific Discovery, Popper, 1959, p.107, by Magee 1973, pp.33, 34)

As we have seen, Kuhn, like Popper, rejects that picture, implied by logical empiricism, of science approaching absolute truth. The revolutions which overthrow existing paradigms, and establish new ones, necessitate the re-evaluation of prior facts. Thus sentences of science are only meaningful assertions within particular paradigms, and are not universally meaningful simply by appeal to experience. (see Kuhn 1970)

Toulmin also rejects the arguments put forward by the logical empiricists and claims that their views are too limiting since they reduce the ways in which knowledge can be tested by experience to one sort only: that of matching particular propositions against empirical facts. Toulmin points out that science does not consist of just individual propositions, but also concepts and theories, and there can be no question of matching concepts against facts, nor of choosing a new theoretical structure just by seeing how it matches with particular facts. However, this does not imply that conceptual problems are not also empirical, nor that scientific theories are chosen on the personal whims of scientists. Concepts and theories are subjected to objective external constraints by being required to produce hypotheses which can be tested. (see Toulmin 1972)
Ziman has suggested that the attempt to arrive at general principles of induction for science has not succeeded. It is often the case that complex theories, such as in elementary particle physics, depend on very few observations. He suggests that the distinctive feature of science is that it is "Public Knowledge":

"Its facts and theories must survive a period of critical study and testing by other competent and disinterested individuals, and must be found so persuasive that they are almost universally accepted. The objective of Science is not just to acquire information nor to utter all non-contradictory notions; its goal is a consensus of rational opinion over the widest possible field."

(Ziman 1968, p.9)

Thus science is empirical not because it produces logical inferences, by means of well-defined inductive processes from observation statements, but because it is subjected to an ongoing process of experiential testing. The ways in which experience is brought to bear are varied and do not just consist of matching statements with observation. Ravetz makes a similar point when he says that the success of science is not due to any magic formula, but rather "the scientific knowledge we possess is the result of a social endeavour, which over the centuries has developed an approach appropriate to its limited goals.....". (Ravetz 1973, p.181)

I think it is fair to conclude that the consensus of opinion among philosophers is that logical empiricism has not provided a suitable philosophy of the natural sciences. The important area of debate now centres around the different theories put forward by Kuhn and Popper (see Lakatos and Musgrave 1970). This, however, is not the case in the behavioural sciences.
In order to appreciate the influence that logical empiricism has had in psychology it is necessary to look at the development of the behavioural sciences, and the philosophical framework that was replaced. When Watson introduced Behaviourism in psychology between 1910 and 1920, he deposed the Structural Psychology expounded by Wundt. Mischel has suggested (Mischel 1969) that Wundt's New Psychology embraced the Cartesian philosophy of Mind/Body dualism, and so mental and physical processes were seen as running parallel to one another. Since the mind was private, the most important technique of this psychology was introspection. Watson's Behaviourism (and the Neobehaviourism of Hull and Tolmin that succeeded it) was a reaction against the concern with 'unobservables' such as a mental 'inner record'. If the mind was private, and not susceptible to observation, then psychology should have nothing to do with it, but should concern itself only with observables. Consequently introspection and all reference to mental states were rejected in favour of the Stimulus-Response approach.

From Watson's classical Behaviourism emerged Neobehaviourism, in which some of the inadequacies of Behaviourism were removed. For example, 'intervening variables' were introduced between Stimulus and Response. According to Weimer and Palermo (Weimer and Palermo 1973, p.225), Neobehaviourism may be said to date from Hull's introduction of Mediation Theory in 1933. They go on to suggest that there are several distinctive features of Neobehaviourism, and among these are certain 'metaphysical directives',

"These directives were largely constituted by an explicit philosophy of science and/or methodology of research, and by the implicit effects and presuppositions of that philosophy-methodology concerning the 'scientific' nature of psychology."

(Weimer and Palermo 1973, p.225)
The following directives, taken from their list, are I feel, relevant to our particular inquiry:

"(2) The explicit philosophy is empiricism (in 'modern' dress: logical positivism, and later, logical empiricism...)

(3) But observation must be objective. Only the objectively observable is scientific. As Tolman put it in describing his own behaviourist position:....'an organism's private mind, if he have any, can never be got at'.

(5) Since there is a legitimate scientific method (the sophisticated inductivism of logical empiricism), which specifies that science collects facts and then induces theoretical generalisations from them, the 'science' of psychology must study simple phenomena exhaustively and then (and only then) tackle the more complex phenomena. Psychology should be a building block endeavour that moves from simple-to-complex phenomena. Psychological atomism is the result.

(6) A resultant atomistic directive states that the complexity of the 'higher' forms of behaviour is one of degree only....

(8) Behaviour is to be understood as a function of the interaction of variables intervening between objective stimuli in the environment. The laws of behaviour are laws showing the relationships between variations in stimulus input and response output.

(10) The study of all behaviour of all organisms is equally legitimate, and the use of animals and non-adults as subjects is legitimated. Further, due to the belief in directives (5) and (6), in combination with the tacit assumption that animal behaviour is 'simple' in comparison to human behaviour, the exhaustive study of 'simple' behaviours in 'lower' animals is to be preferred."

(Weimer and Palermo 1973 pp.225-7)

It is useful to see how these directives are applied in practice, and this can be done by considering the 'systems approach' in more detail. Some limitations of this approach, embodying the above directives, can be shown up by seeing how it might be used to solve some of the problems associated with teaching mathematics to science students.

The first stage in the systems approach is to formulate a list of aims or general, long-term intentions. The next stage, which is
crucial, is to translate these vague aims into specific objectives that communicate a detailed account of the intended behavioural outcomes of the course. There are several reasons for obtaining behavioural objectives from aims, and Mager suggests the following advantages:

"When clearly defined goals are lacking, it is impossible to evaluate a course or programme efficiently, and there is no sound basis for selecting appropriate materials, content, or instructional methods."

Second, objectives enable a clear assessment of:

"...the degree to which the learner is able to perform in the manner desired....unless goals are clearly and firmly fixed.....tests are at best misleading; at worst, they are irrelevant, unfair, or useless."

Third:

"...the student is provided the means to evaluate his own progress at any place along the route of instruction and is able to organise his efforts into relevant activities."

(Mager 1962, pp.3-4)

Once objectives, defining the terminal behaviour that is intended, have been prepared, these can be used to devise tests to measure the extent to which the objectives have been achieved. The derivation of tests from objectives should be straightforward, since the test questions are designed to elicit the behaviour specified by the objectives. Although in practice students are normally tested on completion of a course, in some situations, for example where students' backgrounds are unknown, it may be preferable to also give pre-tests before the course. The use of both pre- and post-tests enable direct comparisons to be made, and also ensure that students who can pass the pre-test do not take the course. The post-test is derived directly from the course objectives, therefore, provided the course objectives are complete and accurate descriptions of the
intended outcomes, post-test scores should provide criteria for evaluation of the course. This evaluation can then provide the basis for future improvements and innovations. By looking at those objectives that are poorly achieved and isolating the appropriate parts of the course aimed at achieving these objectives, the teacher is able to discover those parts of the course that need improving. Subsequent post-test scores monitor the extent to which the course has been improved. Thus there may be considerable advantages in adopting the systems approach, both in helping the teacher clarify for himself what he is trying to teach and in evaluating how successful he has been.

It is generally recognised that the statement of precise objectives is difficult within the higher education sector, and that educators still lack sufficient expertise. Elton, for example, in the paper mentioned earlier, suggests that the problems arising in the teaching of mathematics to science students should be tackled by first translating aims (and he gives some examples) into precise and detailed objectives. However, he then goes on to suggest that this is "not an easy task, but it is clear that it is most easily done for those parts of the course in which the tool aspects of mathematics is stressed. Even there it is rarely done and ordinary examinations are certainly inadequate for the purpose." (Elton 1971) An implication of this might be that, although difficult to prepare objectives, it is nonetheless possible in principle, even where mathematics is used as a language in science.

As Elton suggests, it is easier to obtain objectives for those parts of a course in which the tool aspects of mathematics are stressed
since it is possible to isolate the mathematical skills a student will require when the mathematics is used as a tool. Thus, the intended outcomes of the mathematics course can be specified in terms of the behaviour one would expect of a proficient tool-user. Where such behaviour can be identified, it is desirable to specify it as precisely as possible. For example, instead of expecting a student to 'be able to differentiate', one would obtain a more precise description based on the sorts of functions the student has to differentiate in the science course. This would clearly be an improvement on the use of ordinary examinations which, as Elton suggests, are inadequate, even where we are considering the tool aspects of mathematics for science. The ways in which they are inadequate are clear when we realise that a student is able to pass a course with a percentage mark of between 40 to 50, by answering perhaps eight out of twelve questions, which may well cover as little as half the total course. The purpose of ordinary examinations is, generally, to distinguish between students, rather than to simply ascertain the student's total knowledge. However, this means that a student can pass a mathematics for science course, knowing a very small fraction of what has been taught, yet he is expected to be able to use all the mathematics in the science course. In a situation in which the outcomes of one course are pre-requisites for another course, a criterion-referenced test, rather than the usual norm-referenced test, would be more suitable. Therefore, in those situations in which it is possible to formulate the behavioural outcomes of a mathematics for science course, it is reasonable to expect a high level of mastery of the objectives.

Thus we can see that where it is possible to specify the intended outcomes of a mathematics for science course in terms of student
behaviour, it is possible to use the systems approach to solve some of the associated problems. I have suggested that this is possible where mathematics is used as a tool in science, since specific behaviours can be isolated.

2.5 Limitations of the use of objectives

Before we consider the serious limitations of the systems approach, it must be recognised that Mager undoubtedly represents an extreme position, implying, as he does, that education can be described in the same terms as are used to describe training or instruction. Therefore, in criticising Mager, later in the chapter, I do not consider that I am criticising a view that is commonly held, since I am sure that the majority of those involved in education would not agree with all that Mager says. Nonetheless, I think that less committed forms of Mager's position are commonly held and these positions are still based on many of the underlying assumptions about the nature of learning and understanding that underpin Mager's position. Thus I consider that it is necessary to criticise this extreme so as to establish the limitations that must be imposed on the use of objectives. Clear statements of the stance that I am criticising are given in the following quotations from Mager:

"Though it is all right to include such words as 'understand' and 'appreciate' in a statement of an objective, the statement is not explicit enough to be useful until it indicates how you intend to sample the 'understanding' and 'appreciating'. Until you describe what the learner will be DOING when demonstrating that he 'understands' or 'appreciates', you have described very little at all."

(Mager 1962, p.11)
"Some verbs that may be employed in writing objectives for specific subject-matter areas have meanings that are so precise that they require no further explication. Such words as spell, subtract, read and alphabetize should be used in preparing statements of objectives whenever they are appropriate. The above list (which gives six performance terms, viz Identify, Name, Describe, Construct, Order, Demonstrate, a definition for each and examples of objectives*) however, names and defines behaviours common to many curriculum areas, and it provides a framework for constructing precise objectives for most instructional tasks."

*Sullivan 1969, p.78*

The suggestion that it is possible to replace all 'vague' words by their precise equivalents is an assumption that Mager does not question. It appears to be blind faith that leads Mager to dismiss the use of the word 'appreciate' in the objective 'To develop an appreciation of music', in the following way:

"Let's ask the key question of this objective. What is the learner DOING when he is demonstrating that he has achieved this objective? What is he doing when he is 'appreciating' music? You can surely see that, as now stated, the objective does not give the answer. Since the objective neither precludes nor defines any behaviour, it would be necessary to accept any of the following behaviour as evidence that the learner appreciates music:

1. The learner sighs in ecstasy when listening to Bach.
2. The learner buys a hi-fi system and $500 worth of records.
3. The learner correctly answers 95 multiple choice questions on the history of music.
4. The learner writes an eloquent essay on the meaning of 37 operas.
5. The learner says, 'Oh, man, this is the most. It's just too much.'"

*Mager 1962, p.15*

One is inclined to take this example as a reductio ad absurdum of his approach in this and similar educational situations.
This example shows up the major flaw in Mager's position. We can infer, from Mager's argument that he considers there is a 'something' called 'appreciating music', that can be detected and measured. In the same way, he implies that there is something, called 'understanding', which can be detected. What I wish to criticise is this implication that all cases where we would say that someone appreciates, or all cases where we would say someone understands, have something in common, namely, something called 'appreciation', in the first case, and 'understanding' in the second. For Mager, this something is, or is represented by, the person's behaviour.

However, my disagreement with Mager is not that I consider that what these different cases have in common is, for example, a particular mental state or feeling rather than particular behaviour, although this may well be the unstated objection of many to Mager's extreme behaviourism. Therefore, I wish to make it clear that my criticism of Mager does not consist in asserting that understanding is something that occurs in the student's mind and so cannot be described in terms of behaviour. This is the sort of objection that is put forward, for example, by Pring when he says:

"The more the description of objectives approximates to a description of overt behaviour, the greater the need to examine difficulties in translating propositions about the mind into those that describe what can be observed."

(Pring 1971, p.84)

Instead, I am putting forward an argument developed by Wittgenstein, based on the observation that the different situations in which we would say that someone has understood do not have some essence which is common.
I think that the unease that, for example, teachers feel when reading Mager, arises because he seems to reduce the value of education and suggests that teachers should be concerned only with what the students can do. Many teachers would say that there are some things that cannot be expressed in terms of student behaviour. For example, Ryder (Ryder 1970) in an introduction to the use of behavioural objectives in physics teaching, quotes the objections of Atkins that by concentrating on behavioural objectives, other important outcomes that cannot be expressed in behavioural terms will tend to be ignored. I think that in reacting against Mager's reduction of a course to behavioural outcomes, there is a natural tendency to feel that the outcomes that are ignored are things that go on in the student's mind. Thus a modified form of Mager's position might hold that only some aims can be expressed in terms of behavioural objectives, but others are to do with things that go on inside the student and so cannot be measured. However, I am as critical of many aspects of this sort of position as I am of Mager's position, in as much as it implies that all situations in which we would say that understanding occurs are similar in that there is something, called 'understanding' that is present in the person's mind; that is, that whenever someone understands he has a mental state of understanding. What I am suggesting is that understanding is not, nor is it represented by, either a particular sort of behaviour, or a particular mental state.

In order to substantiate what I have said, I wish to come back to the quotation in which Mager claims that a statement containing the word 'understand' is of little use unless it also states what the student should be doing. In the Philosophical Investigations, Wittgenstein
draws our attention to the meaning of the word 'games', and the following quotation is very relevant to our present discussion:

"Consider for example the proceedings that we call 'games'. I mean board-games, card-games, ball-games Olympic games, and so on. What is common to them all? - Don't say: 'There must be something common, or they would not be called 'games', but look and see whether there is anything common to all. - For if you look at them you will not see something that is common to all, but similarities, relationships, and a whole series of them at that. To repeat: don't think, but look! - Look for example at board-games, with their multifarious relationships. Now pass to card-games, here you find many correspondences with the first group, but many common features drop out, and others appear. When we pass next to ball-games, much that is common is retained, but much is lost. - Are they all 'amusing'? Compare chess with noughts and crosses. Or is there always winning and losing, or competition between players? Think of patience. In ball-games there is winning and losing; but when a child throws his ball at the wall and catches it again, this feature has disappeared. Look at the parts played by skill and luck; and at the difference between skill in chess and skill in tennis. Think now of games like ring-a-ring-a-roses; here is the element of amusement, but how many other characteristic features have disappeared! And we can go through the many, many other groups of games in the same way; can see how similarities crop up and disappear.

"And the result of this examination is: we see a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities sometimes similarities of detail.

"I can think of no better expression to characterise these similarities than 'family resemblances'; for the various resemblances between members of a family: build, features, colour of eyes, gait, temperament, etc., etc., overlap and criss-cross in the same way. - And I shall say 'games' form a family."

(Wittgenstein 1953, 66,67)

If we look we see that all games do not have something in common, yet even though there are no specific criteria for an activity to be a game, this does not mean that in any given context the word 'game' is ambiguous and of little use. It is a fact of our language that in a given context the word 'game' has a clear meaning which does not depend on precise behavioural criteria, and that, moreover
it is not possible to produce a behavioural definition of the word 'game'. This does not mean, however, that we would accept an analogous argument to the one put forward by Mager, that a sentence containing the word 'game' conveys little unless one states what a person is doing when he is playing a game; just because a game cannot be defined in terms of behaviour does not mean that 'game' is a vague word. Nor is it the case that we can make the word more precise by means of a definition:

"If someone were to draw a sharp boundary (for the meaning of the word 'game') I could not acknowledge it as the one that I too always wanted to draw, or had drawn in my mind. For I did not want to draw one at all. His concept can then be said to be not the same as mine, but akin to it. This kinship is that of two pictures, one of which consists of colour patches with vague contours, and the other of patches similarly shaped and distributed, but with clear contours. The kinship is just as undeniable as the difference.

"And if we carry this comparison still further it is clear that the degree to which the sharp picture can resemble the blurred one depends on the latter's degree of vagueness. For imagine having to sketch a defined picture 'corresponding' to a blurred one. In the latter there is a blurred red rectangle; for it you put down a sharply defined one. Of course - several such sharply defined rectangles can be drawn to correspond to the indefinite one. - But if the colours in the original merge without a hint of any outline won't it become a hopeless task to draw a sharp picture corresponding to the blurred one? Won't you then have to say: 'Here I might just as well draw a circle or a heart as a rectangle, for all the colours merge.' Anything - and nothing - is right?"

(Wittgenstein, 1953, 76,77)

Wittgenstein suggests that the word 'understanding' is like this; the different situations in which we would use 'understanding' do not have any single thing that is common, but instead there is a complicated network of inter-relations. If therefore, there is a family resemblance between different situations in which we would say someone understands, rather than a common thread running through
all situations, then we cannot specify particular behaviour correspond-
ing to, or representing, understanding. Thus to produce a set
of objectives corresponding to an aim beginning 'the student should
understand....', is, to use Wittgenstein's analogy, to try to produce
a sharp picture that corresponds to a blurred one. The extent to
which this can be done depends on how blurred the original picture
is. Sometimes the sharper picture will be more useful than the
blurred one, but at other times it is the indistinct picture that is
needed. In the same way, the similarity between an educational aim
and a behavioural objective will depend on the aim. In some cases
the objective may be useful, in others the objective may be so remote
from the aim as to be of no use at all. These points will, I hope,
be made clearer in later chapters in which Wittgenstein's philosophy
is considered in more detail.

2.6 Implications for a technology of education

To conclude this chapter I would like to try to reassure an educational
technologist who might feel that I am, as it were, cutting the ground
from underneath his feet, by rejecting the scientific basis for a
technology of education. To do this, I shall draw his attention to
an argument put forward by Travers, that a technology need not be
science based. Travers begins his argument by pointing out that:

"The concept that a revolution could be brought about in education through the development of an educational
technology appears to have originated in the writing of Pressey (1932) nearly fifty years ago.....The new
technology of education was to be a science based technology and the scientific foundation was to be provided by the emerging behavioural sciences."

(Travers 1973, pp.979/980)
In fact this technology was not developed and Skinner "took the position that a technology had not emerged, as Pressey had predicted it would, because the science of learning had not, until near mid-century, provided the foundation on which a technology of education could be built." (p.980) However, by the 1950's, "Skinner believed that a science of learning had, at last, advanced to the point where it could be used as a guide in the development of a technology of education" (p.980).

This optimism was not justified, and:

"Skinner's view that a body of psychological knowledge is there, awaiting application, is not shared by many other scientists who investigate learning phenomena."

(Travers 1973, p.980)

Scientific theories of learning were based on controlled laboratory experiments and as Travers points out, translations from the laboratory to the real situation have not been marked by a history of success. In fact, Travers criticises the view of technology that is implied:

"The basic idea propounded by both Pressey and Skinner was that technology was a result of scientific development and that technology would not develop until the necessary underlying sciences had progressed to the point where related technologies could be based upon them. This is a modern, and parochial, view of technology, for the fact is that technology has had a history of development at least back to the time of cave men, and probably before.....although the development of scientific knowledge has had enormous impact on some aspects of technology in recent times, history shows that technology has had growth independent of scientific knowledge."

(Travers 1973, p.980)

Kuhn (Kuhn 1970) makes a similar point to Travers when he suggests that, far from being based on science, technologies of the past have often played an important part in the emergence of a science. (See
They have played this role by (1) providing the science with facts that would not have been discovered in a casual investigation, (2) presenting science with particular problems, thus stimulating research along specific lines, (3) testing scientific theories in ways in which they probably would otherwise not have been tested. On the other hand, technologies have undoubtedly benefitted from advances made in science. Thus we can suggest that there can be a two-way relationship between science and technology, rather than a one-way exchange of findings from science to technology.

So far, educational technology has simply tried to apply the findings of the behavioural sciences without trying to influence them. Within certain areas this has proved successful, for example, programme learning has found a number of applications. However, this does not mean that as soon as a technology of education encounters serious difficulties it must await further developments within the science on which it is based. Such an approach denies a technology its traditional role of bringing about developments in science. Clearly it cannot be the aim of this thesis to fulfill this role since, although I have started from what might be thought of as technological problems, I have not attempted to arrive at technological solutions. However, my criticisms of attempts to find solutions within the framework set by the behavioural sciences will serve, I hope, to free educational technology from their present all-pervasive influence.
3.1 Wittgenstein's analyses of concepts

In the Prologue I put forward the suggestion that instead of talking about transference of understanding from the mathematics to the science class, we might be able to reformulate this in terms of concepts, and that we could then talk of transferring concepts in the same way as we talk of transferring skills. Thus, problems arising through students failing to transfer mathematical concepts could be investigated in the same way as we investigate problems arising through a failure to transfer skills.

In this chapter I wish to show that such a hope is illusory and that a reformulation of the transference of understanding in terms of concepts is faced with exactly the same difficulties. The idea that acquiring a concept might be similar to acquiring a skill is based on a traditional notion of a concept which, I believe, Wittgenstein has demonstrated convincingly to be inappropriate. This traditional idea, which arose in philosophy, has also been taken over by psychology, and can be found in various psychological dictionaries: according to the Dictionary of General Psychology, a concept is a "set of abstracted principles or properties" (Heidenreich 1970). The Dictionary of Psychology puts it slightly differently and defines conception as a "process which is characterised by the thinking of qualities, aspects, and relations of objects......of which language is the great instrument, and the product the concept - normally represented by a word" (Drever 1964). The learning of
concepts is referred to as 'concept formation' and, according to
the Dictionary of General Psychology, a concept is formed by "the
observation of characteristics common to a set of objects or situations.
The abstract idea is derived from the grouping of objects by their
distinguishing characteristics, common property or common relation­ship" (Heidenreich 1970).

This view of a concept as a class or an abstraction from a class,
can, for example, be traced back to Kant, where, according to the
Dictionary of Philosophy, a concept is "any generic or class term,
exclusive of relational terms or categories" (Runes 1950), and
presumably, back to Aristotle, who formed Categories by abstracting
the Universal from the particular. The idea of a concept is closely
tied up with the meaning of words. The traditional view in philo­sophy is that a word acquires a meaning by referring to something.
In some cases this is a particular, in others it is a 'universal'
or a concept. Thus psychology has taken over this idea, so that
the learning of meaning is seen as concept formation. There is,
therefore, according to this view, a specific process by which
meanings of words are learnt, and this process of concept formation
is one of abstraction from classes of objects or situations.

However, according to Wittgenstein, the "idea of a general concept
being a common property of its particular instances connects up with
other primitive, too simple, ideas of the structure of language."
(Wittgenstein 1958, p.17) In order to appreciate Wittgenstein's
analysis of concepts, and later, of understanding, it is, I think,
necessary to discuss Wittgenstein's style, and by this I do not
mean simply his literary style, but his style of philosophising.
Wittgenstein's style is closely connected with his conception of philosophy itself, and it is only by appreciating this style that we shall avoid drawing false conclusions. The first thing to notice is the construction of the Investigations, which Wittgenstein draws our attention to in the preface. The book consists of "the precipitate of philosophical investigations" which are written down as remarks: "my thoughts were soon crippled if I tried to force them on in any single direction against their natural inclination. - And this was, of course, connected with the very nature of the investigation. For this compels us to travel over a wide field of thought criss-cross in every direction. - The philosophical remarks in this book are, as it were, a number of sketches of landscapes which were made in the course of these long and involved journeyings."

(Wittgenstein 1953, p.vii)

Thus Wittgenstein does not give a systematic account, and to try to translate his remarks into a systematic account is to distort his philosophy. Binkley takes Wittgenstein's claims seriously, and some of the implications of Wittgenstein's style can be seen in Binkley's discussion of the analogy Wittgenstein makes between a philosopher and a therapist. Wittgenstein suggests that a philosophical problem is like an illness; it is like a neurotic obsession with a particular picture, for which the cure is therapeutic treatment.

"Whether it be an epidemic or an isolated case, philosophic treatment is directed towards philosophic disease: it is a corrective, and sometimes a preventative, measure."

(Binkley 1973, p.113)

Wittgenstein is, therefore not interested in putting forward a theory, nor even in denying a theory, except where this is part of the treatment. He does not propose a 'correct' picture, but tries to show
the inadequacies that are inherent in the picture that we have become obsessed with.

Failure to appreciate the analogy of the philosopher as therapist can result in the corrective pictures being mistaken for pictures which are claimed to be correct. Wittgenstein's sketches are simply descriptions, not explanations or theories, and it is, as Binkley comments, "from this point of view we need to look at Wittgenstein's treatment of such topics as private language, essentialism, family resemblances, etc. - in short, all the sketches grouped under rubrics which have since become theories." (p.171)

In section 2.5, I quoted Wittgenstein's famous passage on 'games' in which he points out that all games do not have something which is common, instead there is "a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail." However, Wittgenstein is not saying this is what concepts are like, he is simply showing us that a picture of a concept as a common attribute is, at least in some cases, and perhaps in many cases, too limited. Similarly, when Wittgenstein says that: "the meaning of a word is its use in the language." (p.43) it is often forgotten that this applies for "a large class of cases - though not for all", and that the purpose of this remark is to get us away from the idea that the meaning of a word is something (for example, a class or abstraction) to which the word refers. Wittgenstein is not saying what a concept is, he is trying to show us that we have a too simple picture. It is no argument against Wittgenstein to say, for example, that we can learn concepts without being able to use words and that therefore a concept
is not given by the use of a word; in fact this is a remark that Wittgenstein himself might have made, to show that a concept is not as simple as we think it might be.

It is for the same purpose that Wittgenstein draws our attention to how a concept is taught. Thus, Wittgenstein asks:

"How should we explain to someone what a game is? I imagine that we should describe games to him, and we might add: 'This and similar things are called "games"'."

(Wittgenstein, 1953, para. 69)*

In this way, Wittgenstein draws our attention to certain aspects of language which we are inclined to overlook. Wittgenstein also employs dialogue to further the argument, the purpose of which is to show up flaws in commonly accepted views, for as Wittgenstein writes:

"My aim is: to teach you to pass from a piece of disguised nonsense to something that is patent nonsense."

(Wittgenstein, 1953, 119)

The suggestion that a concept is an abstract principle or property is disguised nonsense, which Wittgenstein uncovers by means of the following dialogue which starts with Wittgenstein saying how he would explain the meaning of certain words: "...I shall explain these words to someone who, say, only speaks French by means of the corresponding French words. But if a person has not yet got the concepts, I shall teach him to use the words by means of examples and by practice. - And when I do this I do not communicate less to him than I know myself." (208) But if someone feels that a concept is an abstracted property, they are likely to be dissatisfied with this explanation, and to reply: "'But then doesn't our understanding reach beyond all the examples?'". To which Wittgenstein comments:

* Reference numbers in Wittgenstein 1953 refer to paragraph numbers
"A very queer expression, and a quite natural one!" (209) It is natural because we feel that there must be something more to under­stand than just giving examples. We feel that there must be some essence underlying all these uses and so we might ask: "'But do you really explain to the other person what you yourself understand? Don't you get him to guess the essential thing? You give him examples, but he has to guess their drift, to guess your intention.'" To which the reply is: "Every explanation which I can give myself I give to him too. - 'He guesses what I intend' would mean: various interpretations of my explanation come to his mind, and he lights on one of them. So in this case he could ask; and I could and should answer him." (210)

To show how relevant Wittgenstein's remarks are, we can usefully turn from philosophy to a psychological learning theory proposed by Brunner, Goodnow and Austin (1956), which is implicitly criticised by the above quotation. According to their theory, a concept is a categorisation that renders "discriminately different things equivalent" (p.1), and the process of concept attainment is the "process of finding predictive defining attributes that distinguish exemplars from non-exemplars of the class." (p.22) They illustrate this process with an example that is to be taken as a paradigm of concept attainment. A foreigner is introduced, by a friend, to various people and is told whether or not these people are influential. On this basis of being given the exemplars and non-exemplars, the foreigner has to abstract the attributes of the class of influential people. Brunner et al then consider the various strategies that are possible and which of these optimise the process of concept attainment under different conditions.
Although it may be interesting to study these different strategies, we are entitled to ask why the foreigner could not have been given a list of the attributes of influential people, rather than having to guess these attributes. For the above example to serve as a paradigm of concept attainment, the only answer that can be given is that, although in this particular case the attributes could have been listed, this is not always so, but nonetheless the process of concept attainment is the same in both cases. Thus the implication is that if, for example, a person has to learn the concept 'blue', he will do this by seeing what is common to blue things and thus what distinguishes these exemplars from non-exemplars; in the first case the attributes abstracted are, for example, 'rich', 'landowner', etc., and in the second the attribute is 'blueness'. However, by means of the following examples, Wittgenstein tries to show that the appeal to 'seeing what is in common' cannot be so straightforward:

"Suppose I show someone various multicoloured pictures, and say: 'The colour you see in all these is called "yellow ochre"'.....Then he can look at, and point to the common thing.

Compare with this a case in which I shew him figures of different shapes all painted the same colour, and say: 'What these have in common is called "yellow ochre"'.

And compare this case: I shew him samples of different shades of blue and say: 'The colour that is common to all these is what I call "blue"'."

(Wittgenstein 1953, 72)

Thus Wittgenstein suggests that the idea of learning, say, the concept 'blue' by seeing what blue things have in common misleads us by implying that what happens is that we acquire a sample or picture in our mind. "So if I am shewn various different leaves and told 'This is called a "leaf"', I get the idea of the shape of the leaf, a picture of it in my mind." (73) "But", Wittgenstein goes on to
ask, "what does the picture of a leaf look like when it does not shew us any particular shape, but 'what is common to all shapes of leaf'? Which shade is the 'sample in my mind' of the colour green - the sample of what is common to all shades of green?" Wittgenstein then goes on to suggest that even if there were such a sample of 'pure green', we still must know how this sample is to be used, that is, that it is used as "a sample of all that is greenish and not as a sample of pure green". (73)

So the point here is that even if, having learnt a concept, one holds a picture or abstraction in one's mind, this alone would not be sufficient since one must also understand how this abstraction is used. As Strawson notes:

"Variants on 'use' in Wittgenstein are 'purpose', 'function', 'role', 'part', 'application'",

and the aim of this central notion is to:

"get us away from our fascination with the dubious relation of naming, of meaning, and to make us look at the speaking and writing of language as one human activity among others, interacting with others; and so to make us notice the different parts that words and sentences play in this activity."

(Strawson 1970, p.25)

To someone who is still fascinated with the dubious relation of meaning, Wittgenstein says:

".....you think of the meaning as a thing of the same kind as the word, though also different from the word. Here the word, there the meaning. The money, and the cow that you can buy with it. (But contrast: money, and its use)"

(Wittgenstein, 1953,120)

It is this contrast that Wittgenstein wants to bring out, and one way of doing so is to look at, what Wittgenstein calls, 'language-games':

"Here the term 'language-game' is meant to bring into prominence the fact that the speaking of language is part of an activity, or of a form of life."

(Wittgenstein 1953, 25)
Examples of language-games include: "giving orders and obeying them", "reporting an event", forming and testing a hypothesis", "asking", "thanking", "cursing", etc. (23) The notion of a language-game is an important one in Wittgenstein's work and we can see that the concept 'game' plays a dual role: it is used both as an illustration to show that words do not necessarily have precise meanings, and also as a direct comparison with language: "games form a family, and so do the various activities, which come under the general description of 'using a language'." (Strawson 1970, p.32)

To demonstrate the way in which psychologists, as well as philosophers have ignored the functions of words, we can turn to another learning theory, this time based on various experiments, the results of which are used by Vygotsky to propose a process of concept formation (Vygotsky 1962). The concepts the subjects had to discover were denoted by four nonsense words: 'lag', 'bik', 'mur', 'cev'. The subjects were given twenty-two blocks of varying colour, weight, shape and size, and had to learn that, for example, 'lag' was tall and large, 'bik' was large and flat, etc., by looking for various common properties between blocks described by the same word. Thus like Bruner, Vygotsky investigates the ways in which subjects abstract the attributes that distinguish exemplars from non-exemplars. Vygotsky then observed and described the various stages through which the subjects passed before arriving at the final, well-defined groupings.

Again, as with the experiments of Bruner et al, we cannot consider the above situation as a paradigm of concept learning. I think we are entitled to ask why the subject could not be taught the concepts by listing the attributes, for example, that 'lag' was to be used
in place of the two adjectives 'tall' and 'large' etc. When we ask this question, I think that it becomes clear that although the subject is learning new words, he is not learning new uses of words, or rather, the use he has learnt is a very restricted one. That is, he is able to use the words 'lag' 'bik' 'mur' and 'cev' in connection with the activity of describing the twenty-two blocks, but he has not learnt how to use these words outside this activity. The words that he already knows, 'large', 'tall', 'flat', etc., he is able to use in many different language-games applied to many different types of objects, for example, sky-scrapers, dogs, matchboxes, lorries, needles, apples, etc. In the case of the four nonsense words, either the subject is unable to apply them to these various objects, or else he applies them in place of a conjunction of pairs of words he already knows. In the first case, the concept learning is hardly representative since the use of these words is far too restricted. If we look at the way a word such as 'large' is used, I suggest that we will not be able to abstract the attribute for which it stands, but only observe family resemblances between the various objects described. In the second case, where the words are used in place of pairs of known adjectives, the subject is not learning a new use, he is not learning new concepts. This is the same situation as the one in which Wittgenstein explains the meaning of words to someone who only speaks French, by means of the corresponding French words.

Thus, I think it is clear that learning a concept involves more than simply abstracting common attributes. To learn that 'lag' is used to describe tall, large blocks, does not, by itself mean that a new concept has been learnt since the word has no other use. Although a rule has been given, we do not know how to apply it outside the
limited circumstances in which it was learnt; the word has no clear application in the normal activities associated with speaking and writing.

3.2 Concepts in Science

The traditional view of a concept can also be found in the philosophy of science. In particular, Ravetz in "Scientific Knowledge and its Social Problems" (Ravetz 1973), proposes a definition of a concept that is clearly related to the traditional view. He maintains that in a scientific problem "the things discussed are not the objects and processes perceived through ordinary experience, but intellectual constructs." (p.72) "I describe these 'objects of inquiry' as 'classes of intellectually constructed things and events', rather than as 'concepts', in order to stress what, for me, is their most important feature." (p.73)

Thus, according to Ravetz, a concept is a class of things or of events and as we see in the following quotation, where he tries to distinguish scientific from everyday concepts, this applies to all concepts:

"It might be objected that I have not yet shown any distinctive feature of scientific knowledge, for even our ordinary speech and thinking are done with names, which are identifying tags for general classes of things and events. Moreover, these names, and their classes, are far from natural..... In this sense it is true that all our knowledge is 'artificial'...... But the objects of scientific knowledge are more artificial than this; to indicate the difference I have used the term 'intellectually constructed'.

(Ravetz 1973, p.113)
We see that Ravetz is not concerned with demonstrating that concepts, in general, are classes of things and events; he accepts this, and then tries to show that scientific concepts are also classes of things and events, but differ in that they are more artificial. Thus, although "chemical 'substance' and biological 'species' are less artificial than many objects of scientific knowledge, for their samples are things which have many properties accessible to fairly direct inspection" (p. 112), nonetheless, a concept such as 'substance' is still an intellectually constructed class, since it "is not a formalised description of a unique collection of material; rather, it is a class of things, the members of the class being defined by their possession of certain properties." (p. 111)

Consequently, since he takes it for granted that concepts are classes of things and events, Ravetz is not worried by the fact that in science "many conceptual classes have samples whose properties or even whose character as a 'thing' or 'event', are incapable of easy translation into ordinary experience." (pp. 112/3). He goes on to state:

"....it is beside my present purpose to determine whether the scientific concept is best considered as a class of 'things' or of 'events'......For to the extent that their ontological status becomes obscure their character as intellectual constructs becomes apparent." (my italics)  
(Ravetz 1973, p. 113)

It must be clear from the preceding section that my criticism of Ravetz does not consist in suggesting that scientific concepts are not intellectually constructed, but in suggesting that concept words are not the names of classes. Thus I do not wish to counter Ravetz's
main arguments that scientific concepts are artificial or intellectually constructed, but to question his basic assumption as to the nature of concepts in general, and hence scientific concepts in particular. I therefore consider that it is for just those concepts, "whose character as intellectual constructs becomes apparent" that a demonstration is necessary in order to show that they are in fact classes at all.

Wittgenstein points out:

"One thinks that learning language consists in giving names to objects. Viz, to human beings, to shapes, to colours, to pains, to moods, to numbers, etc..... To repeat - naming is something like attaching a label to a thing. One can say that this is preparatory to the use of a word. But what is it a preparation for?"

(Wittgenstein 1953, 26)

Thus we may want to say that the word 'substance' is a label attached to a class of things, but to simply attach a label to a class does not show how the label is to be used. Thus to say that 'substance' refers to a class of things does not give the meaning of the word; it does not tell us what the concept of a substance is. Nor do I think that on closer inspection it is possible to defend Ravetz's suggestion that a word such as 'substance' refers to a class "defined 'intentionally' by certain properties of its members" (p.111), the 'extension' of which is determined by testing to see whether a particular 'object' "satisfies the defining properties of the member of the class to an acceptable degree" (p.112). When we realise that the term 'substance' has included among its members not only present day compounds and elements but also the ancient elements earth, air, fire and water, and the now discredited 'substances' phlogiston and the aether, then I think that we must come to the conclusion, not that there is a
common defining property or set of properties, but rather that there
is a network of inter-relationships.

If we are able to escape from the fascination of the relationship
between naming and meaning, then I think it is even clearer in the
case of concepts such as 'field', 'mass', 'energy', for which we
cannot isolate things or events as samples, than it is for concepts
such as 'substances', where we can isolate a class of things, that
these terms are not the names of classes. Even if it were possible
to isolate a class of things or events to which the word 'energy'
referred, this class would not determine the meaning of the word
'energy' since it plays no part in the teaching of the meaning of
'energy' to someone. A student does not learn the concept 'energy'
by being shown a class to which the word refers, nor by being told
the defining properties of members of a class, but by seeing how the
word is applied in various problem-solutions. That is, he learns
the meaning of scientific concepts not by learning what the words
refer to, but by learning how they are used.

Wittgenstein's criticisms of the traditional notion of a concept are
clearly related to Kuhn's analysis of the nature of science. In
particular, Wittgenstein's references to 'forms of life' can be seen
for the scientist, as a 'paradigm', in the sense which Kuhn later
termed metaphysical paradigm or disciplinary matrix, with which the
scientist works. It is therefore useful to consider in more detail
what Wittgenstein meant by 'form of life'.

Wittgenstein uses the expression 'language-game' to "bring into
prominence the fact that the speaking of language is part of an
activity, or of a form of life." (Wittgenstein 1953, 23) The
term 'form of life' is used elsewhere in the Investigations, as in:
"...to imagine a language means to imagine a form of life."

"(human beings) agree in the language they use. That is not agreement in opinions but in form of life."

"Can only those hope who can talk? Only those who have mastered the use of a language. That is to say, the phenomena of hope are modes of this complicated form of life."

"What has to be accepted, the given, is - so one could say - forms of life."

(Wittgenstein 1953)

It is clear from these quotations, that 'forms of life' is used to refer to "the human behaviour, the activities, the natural expressions that surround the words for that concept." (Malcolm 1968, p.91)

However, it is not immediately clear how restricted or how general an interpretation of the words 'behaviour' and 'activity' is intended. This is associated in the Investigations with a similar vagueness in the term 'language-game', which at times can be taken to mean a 'game' within language as a whole, and at other times, as language as a whole.

I think that the reason for this vagueness (and vagueness, as we have noted, is not necessarily a fault) is that, on the one hand, mankind as a whole shares the ability to speak, and underlying this must be some common characteristic that distinguishes man from other animals. Yet on the other hand, there are many different languages, which are not all perfectly translatable, one into another, and within these languages there are various 'specialist languages' (such as for example the 'language' of chemistry) which are not understood by everyone who speaks the language, and underlying this must be corresponding differences in behaviour. Since, according to Wittgenstein,
speaking a language is part of an activity or form of life then, there must be a form of life common to all human beings, but equally, the fact that people speak different languages, in the sense of different tongues, such as English, French, etc., and different language-games, implies that there are different forms of life within that which is common. If to imagine a language means to imagine a form of life, what is meant by form of life will depend on how restricted is the use of the term 'language'. We can say that, to the extent that people share a form of life they can communicate with each other.

I therefore think that it is quite consistent with Wittgenstein's use of the expressions 'form of life' and 'language-game' to suggest that scientists within a community, and here we can make this as restricted or as general as we wish, can be said to share the same form of life, and to speak the same language-games. They are able to communicate with each other through sharing this form of life. The paradigms of science, that is, the concrete puzzle-solutions that guide research, constitute language-games within these forms of life. Scientists within different communities share different forms of life in as much as they are guided by different paradigms, speak different language-games, and hence have difficulty communicating with each other.

3.3 Mathematical concepts used in science

It is the fact that mathematics and science are different language-games that denies us the possibility of being able to talk of trans-
ferring concepts from the mathematics class to the science class. A student may well learn mathematical concepts such as function, integral, limit, variable, etc., in his mathematics class, and he may well need mathematical concepts such as function, integral, limit, variable, etc., in his science class, but we cannot therefore conclude that we are talking about the same concepts in these two different language-games. That is we cannot conclude that words such as 'function', 'variable' etc., have the same meaning in the different language-games.

If a concept were simply an abstraction from a class of things or events, then indeed we could talk of transferring concepts, in the same way that we talk of transferring skills. However, when we replace this simple picture of language with one in which it is recognised that a concept, that is the meaning of a word, is given by the use of a word, then this is no longer possible. A student cannot learn concepts in mathematics and then apply them in science, since the concepts that the student needs are often given by just these applications. It is true that the use of mathematical terms in science will be related to the use of the terms in mathematics, but the two uses will seldom be identical.

Part of understanding a concept will be applying the term to various types of situations, and because a student is said to understand a concept in his mathematics class, it does not follow that he will be able to apply the term to the sorts of situations that he encounters in his science class. Hence, it does not follow that he will understand what is, on the face of it, the same concept. In fact, as we have seen, it is not the same concept, but a related one.
The objection might be made that if the student had really understood the concept in his mathematics class, he would have been able to apply this in his science class. However, this begs the question of what we mean by understanding: it suggests that understanding is being able to apply a mathematical concept in a science class. It is in fact, one of the features of Wittgenstein's treatment that a consideration of the nature of understanding cannot be avoided. Wittgenstein's treatment makes clear the relationship between concepts and understanding, which is obscured when a concept is thought of as an abstracted principle or property. In order to be able to use the word 'understanding' there must be observable criteria, since otherwise there would be no difference between a right use and a wrong use. The criterion for the correct use of 'understanding', in relation to a concept, is given by the correct use of a concept word. Thus, since the uses of a term may be different in mathematics and science, there will be different criteria for understanding that concept.
4.1 Introduction

I would like to introduce this discussion of the nature of understanding by considering Vygotsky's investigations into the relationship between thought and speech. Most behavioural approaches tend to consider thought to be unobservable, and hence incapable of scientific investigation. However, Vygotsky, while still retaining a behavioural approach and scorning introspection as a valid scientific method, nonetheless embarked on a study of thought. He found a way of studying thought, or as he called it, inner speech, by observing a child's linguistic development. Like Piaget, Vygotsky distinguishes three types of speech: ordinary vocal speech, egocentric speech, in which the young child holds 'conversations' with himself, and inner speech. However he did not agree with Piaget's original suggestion as to the relation between these three types. According to Vygotsky:

"The primary function of speech, in both children and adults, is communication, social contact. The earliest speech of the child is therefore essentially social. At first it is global and multifunctional; later its function becomes differentiated. At a certain age the social speech of the child is quite sharply divided into egocentric and communicative speech."

(Vygotsky 1962, p.19)

Thus Vygotsky has this to say about the development of speech and the relation between the types:

"Egocentric speech as a separate linguistic form is the highly important genetic link in the transition from vocal to inner speech, an intermediate stage between the differentiation of the functions of vocal speech and the final transformation of one part of vocal speech into inner speech. It is this transitional role of egocentric speech that lends
it such great theoretical interest. The whole conception of speech development differs profoundly in accordance with the interpretation given to the role of egocentric speech. Thus our schema of development - first social, then egocentric, then inner speech - contrasts both with the traditional behaviourist schema - vocal speech, whisper, inner speech - and with Piaget's sequence - from non-verbal autistic thought through egocentric thought and speech to socialised speech and logical thinking. In our conception, the true direction of the development of thinking is not from the individual to the socialised, but from the social to the individual."

(Vygotsky 1962, pp.19/20)

Wittgenstein makes a similar point in the following passage: "An 'inner process' stands in need of outward criteria." (Wittgenstein 1953, (580) This, as Malcolm points out, is why:

"Wittgenstein exhorts us, over and over, to bethink ourselves of how we learned to use this or that form of words or of how we should teach it to a child. The purpose of this is.....to bring into view those features of someone's circumstances and behaviour that settle the question of whether words (e.g. 'He is calculating in his head') rightly apply to him. Those features constitute the 'criterion' of calculating in one's head."

(Malcolm 1968, pp.83/4)

Thus Wittgenstein suggests:

"Ask yourself: would it be imaginable for someone to learn to do sums in his head without ever doing written or oral ones? - 'Learning it' will mean: being made able to do it. Only the question arises, what will count as a criterion for being able to do it?"

(Wittgenstein 1953, 385)

The notion of 'criterion' is an important and difficult one in Wittgenstein's philosophy. His treatment of topics is inter-related and one way in which he considers what is meant by 'criteria for understanding' is to consider the criteria for being in pain. Thus we must consider how a child learns the meaning of the word 'pain':
"What would it be like if human beings shewed no outward signs of pain (did not groan, grimace etc)? Then it would be impossible to teach a child the use of the word 'tooth-ache'."

(Wittgenstein 1953, 257)

It is no answer to suggest that the child may be able to invent a word for the sensation of tooth-ache, since Wittgenstein points out that a private definition is no definition at all; it is the same as suggesting that the right hand should give the left hand money. Wittgenstein reminds us that "there are certain criteria in a man's behaviour for the fact that he does not understand a word". (269)

If we return for a moment to the topic of understanding, the implication is that Wittgenstein is not denying that understanding may be accompanied, or may sometimes be accompanied, by a certain feeling or sensation. He is, however, pointing out that, by itself, this feeling is not sufficient, since it stands in need of outward criteria. A child would not be able to learn the meaning of the word 'understand' simply by attending to an inner feeling. He is only able to learn the word, or perhaps more exactly, the word only has meaning within language, because there are typical behaviours of understanding. Thus, as Vygotsky suggested, a child first learns to use words in a social, communicative context, and is only later able to apply these words to his own feelings.

Now it might be thought that by demanding outward criteria for understanding, I am reverting to a behavioural stance, that I have explicitly denied, and am implying that it is possible to produce objectives for understanding. However, I think that Wittgenstein's behaviourism is significantly different from the psychological theories of Behaviourism and Neobehaviourism. If we propose an objective specifying
the behaviour that is to count as understanding, then the implication is that if a person satisfied this objective, then he has understood.

That Wittgenstein would not accept this implication can be demonstrated by looking at what Wittgenstein has to say about the relationship between pain behaviour and being in pain: we can consider whether Wittgenstein would accept that satisfying the criteria for being in paid necessarily implies that a person is in pain. Malcolm suggests that Wittgenstein does not accept this: "A criterion is satisfied only in certain circumstances." (Malcolm 1968, p.85) That is, although a person may exhibit pain behaviour, which in certain circumstances we would accept as a criterion for his being in pain, in other circumstances, for example, where he had been hypnotised, or was in a play, etc., the same behaviour would not be accepted as a criterion. It might then be suggested that if we listed all these circumstances in which pain behaviour did not count as being in pain, then, in all other circumstances we could be sure that pain behaviour implied being in pain.

"But here we must be on our guard against thinking that there is some totality of conditions corresponding to the nature of each case (eg. for a person's walking) so that, as it were, he could not but walk if they were all fulfilled."

(Wittgenstein 1953, 183)

As Malcolm adds to this:

"The list of circumstances......is not infinite, but indefinite. Therefore, entailment conditions cannot be formulated; there are none."

(Malcolm 1968, p.86)

However, as Malcolm goes on to point out, we are faced with the following problem:

"....if it does not follow from his behaviour and circumstances that he is in pain, then how can it ever be certain that he is in pain? ....It looks
as if the conclusion ought to be that we cannot 'completely verify' that he is in pain. This conclusion is wrong, but it is not easy to see why."

(Malcolm 1968, p.87)

He interprets Wittgenstein as suggesting that:

".....there can be situations of real life in which a question as to whether someone who groans is pretending, or rehearsing, or hypnotised, or...... simply does not exist. 'Just try - in a real case - to doubt someone else's fear or pain' (Wittgenstein 1953, 305) A doubt, a question, would be rejected as absurd by anyone who knew the actual surroundings."

(Malcolm 1968, pp.87/88)

Thus, the fact that it does not logically follow from a person's behaviour in particular circumstances that he is in pain, does not mean that, in an actual situation we need doubt whether he really is in pain (although, clearly, in some situations we might). Conversely, the fact that in a real case we can be certain from a person's behaviour that he is in pain, does not mean that we should be able to specify behavioural criteria in advance. A process of inference, from observing pain behaviour to knowing the person is in pain, is not an appropriate description here, since such a process implies uncertainty and chance where there need be none.

We can draw an analogy between pain and understanding, and suggest that it is not possible to specify precise conditions which, if fulfilled, must imply understanding. That is, we must guard against thinking that there is a totality of conditions such that, if they were fulfilled, a person must understand. Yet, this does not mean that, in a real situation there need be any doubt that a person has understood. Nor, does it mean that there are no circumstances in which we doubt whether a person has understood, but only that we could not doubt it in all circumstances. It should be clear now
how Wittgenstein's account differs from the Behavioural accounts I considered earlier; although the 'inner process' of understanding stands in need of outward, that is, behavioural criteria, this does not imply that it is possible to list these criteria.

If we recall the previous chapter on concept learning, then we remember that it is often the use of a word in a language-game that supplies the outward criteria as to whether a person has understood a concept. Although the learning of a concept 'green', say, may involve forming a picture of pure green in the mind, to be used as a sample, this is an inner process that stands in need of outward criteria. The outward criteria of understanding the concept 'green' are the correct uses of the word 'green'. If, for example, a person were to say that an object was both red and green all over, we would not understand him; we would say he was not using the word 'green' according to the normal rules; that he did not understand the concept. Although we have said that the use of a word is governed by rules, these are not the rules of a mathematical calculus. The use of a word is not everywhere bounded by rules and so the rules cannot specify in advance all those uses which are to be accepted as correct ones, just as the rules of a game do not specify in advance which 'moves' are correct. Whether or not a use is correct will depend on the circumstances in which it is used, and the circumstances in which a word might be used are obviously indefinite.
4.2 Examples of particular circumstances

The above discussion of understanding can perhaps be made clearer by looking at a particular example and seeing what is meant by understanding in this case. An important example that Wittgenstein uses quite extensively is the following:

"A writes a series of numbers down; B watches him and tries to find a law for the sequence of numbers. If he succeeds he exclaims: 'Now I can go on!' - So this capacity, this understanding, is something that makes its appearance in a moment. So let us try and see what it is that makes its appearance here. - A has written down the numbers 1, 5, 11, 19, 29; at this point B says he knows how to go on. What happened here? Various things may have happened; for example, while A was slowly putting one number after another, B was occupied with trying various algebraic formulae on the numbers which had been written down. After A had written the number 19, B tried the formula:

\[ a_n = n^2 + n - 1 \]

and the next number confirmed his hypothesis. Or again, B does not think of formulae. He watches A writing his numbers down with a certain feeling of tension, and all sorts of vague thoughts go through his head. Finally he asks himself: 'What is the series of differences?' He finds the series 4, 6, 8, 10, and says: 'Now I can go on'. Or he watches and says: 'Yes, I know that series' - and continues it, just as he would have done if A had written down the series 1, 3, 5, 7, 9. - Or he says nothing at all and simply continues the series. Perhaps he had what might be called the sensation 'that's easy!'. (Such a sensation is, for example, that of a light quick intake of breath, as when one is slightly startled.)

Wittgenstein then goes on to ask:

"But are the processes which I have described here understanding? 'B understands the principle of the series' surely doesn't mean simply: the formula \( a_n = \ldots \) occurs to B. For it is perfectly imaginable that \( a_n \) the formula should occur to him and that he should nevertheless not understand. 'He understands' must have more in it than: the formula occurs to him. And equally, more than any of those more or less characteristic accompaniments or manifestations of understanding."

(Wittgenstein 1953,151, 152)

So Wittgenstein is suggesting that we are not satisfied that certain behaviour, such as writing down the formula, continuing the series,
having a 'light quick intake of breath', or certain sensations, such as the sensation 'that's easy!' are the same thing as understanding. Instead they seem to be merely accompaniments. It is as if we were "trying to get hold of the mental process of understanding which seems to be hidden behind those coarser and therefore more readily visible accompaniments" (p.153). But if this is so, then we are looking for the wrong thing:

"If there has to be anything 'behind the utterance of the formula' it is particular circumstances, which justify me in saying I can go on - when the formula occurs to me.

"Try not to think of understanding as a 'mental process' at all. - For that is the expression which confuses you. But ask yourself: in what sort of case, in what kind of circumstances, do we say, 'now I know how to go on', when, that is, the formula has occurred to me?"

(Wittgenstein 1953, 154)

We can relate this to the previous discussion on the use of objectives since, under some circumstances (but not others) achieving the following objectives for example:

1. Given the first four terms of a simple series the student should be able to write out the following four terms;
2. Given the first four terms of a simple series the student should be able to write out the general term, $a_n = \ldots$,

may indicate that the student has succeeded in the aim of understanding the principle behind the series. But in general, the aim and the objectives will not have the same sense, and we can imagine circumstances where the student achieves the objectives yet does not understand the principle behind the series. (Notice however, that this lack of understanding would not be discovered by the teacher through some mysterious process, but, for example, by his observing that the student in other situations was not able to do certain things. Thus understanding depends on behaviour, it is not something 'mental' and
unobservable. However, it is the particular circumstances that give meaning and significance to the behaviour.) On the other hand it is possible that the student has understood but fails to achieve the objectives due to factors within the particular situation, for example he was tired, etc. Thus, understanding will be manifested in different behaviour according to the circumstances. The teacher will often be able to judge when a student has understood something, and he can only do this by observing the student's behaviour, not by some mysterious insight into the student's mind. However this does not imply that it is possible to establish specific criteria of understanding nor state the general behaviour patterns to which the student will conform prior to the particular circumstances in which the understanding occurs.

I want to try and enlarge on this by, as it were, 'filling out' Wittgenstein's example in two different ways. In both cases, B's behaviour will be the same, but I think it will be clear that in the first case the particular circumstances do not justify us saying that B understands, whereas in the second they do.

Situation 1: A student, S, has just completed a course on mathematical series and has been warned in advance that he will be given a test on what was taught in the course. In this test he is asked to write down the formula for the following series of numbers: 1, 5, 9, 13, .... S writes down the (correct) formula $a_n = 4(n-1) + 1$.

Situation 2: Two people, X and Y, are discussing Wittgenstein's example and X suggests that B could show that he had understood what was meant by a series by giving another example. Y comments "That is
all very well, but what would you accept as another example?" "What do you mean?" replies X. "Well, you would probably accept 1, 2, 4, 7, ... as another example. But would you accept....". Y thinks a moment before carrying on. "Would you accept, say, 1, ... 39, .... ah, .... \(\frac{1}{2}\), would you think he had understood what a series was?"

They continue this discussion further. Meanwhile, Z, who has overheard this, starts scribbling on a piece of paper. A little while later he hands a slip of paper across to X and Y, on which is written

\[ a_n = \ldots, \] the formula for a series starting 1, 39, \(\frac{1}{2}\), ... in which the first differences form an arithmetic series, just as in 1, 2, 4, 7, ....

Let us now consider in a little more detail the particular features that distinguish the two situations:

(a) S had just attended a course and had had an opportunity to revise. He was sitting in an examination in which he knew he might well have to produce a formula of the form \(a_n = \ldots\). On the other hand, Z was in an informal situation for which he had had no specific preparation.

(b) Since examination questions are specially prepared, S had every reason to suppose that it was possible to produce a quite simple formula for the series. However, in the second situation, Y was inventing the numbers as he went along, to try to demonstrate that a series was not any string of numbers.

(c) The first series looks like a series that has a formula, that is, it has a similar form of appearance to series that we commonly see. The numbers 1, 39, \(\frac{1}{2}\), ... do not have this familiar appearance.

Although the behaviour is the same in both cases, I think it is clear that in the second case it would have been absurd for X and Y to have
doubted that Z had understood the principle behind the series. Doubt would not even have arisen. However, we might also suggest various intermediate examples in which judgement is not so clear, that is, where we may think that a person has understood, but where we also have a nagging doubt that he has not. Yet we should also bear in mind that it would not be possible to doubt that a person had understood on every occasion. If someone were to do so we should assume that he did not know how we used the word 'understand'. It is not the case that: "we are in doubt because it is possible for us to imagine a doubt." (Wittgenstein 1953, 84)

4.3 Bloom's Taxonomy of Educational Objectives

To see how this approach to understanding differs from a behavioural approach (where, incidently, the temptation to think of understanding as a 'mental' process is not allowed) we can consider the Taxonomy of Educational Objectives which sets out to: "provide for classification of the goals of our educational system. It is expected to be of general help to all teachers, administrators, professional specialists, and research workers who deal with curricular and evaluation problems. It is especially intended to help them discuss these problems with greater precision." (Bloom 1956, p.1) The Taxonomy was planned in three parts, of which the first two, covering the cognitive and affective domains are the most important. I have already commented on the distinction between cognitive and affective learning, and the discussion in this chapter will be confined to the first Handbook dealing with the cognitive domain. It is undoubtedly true that since
The Taxonomy was first published it has had a major influence on educational thought. As Eggleston comments:

"The aim of the authors of the taxonomy was that it should be communicable, comprehensive, stimulating to thought concerning educational problems, and acceptable and useful to workers in the field. There is evidence that a substantial measure of success has been achieved."

(Eggleston 1969, p.81)

The main uses to which the Taxonomy has been put are in the construction of tests, where it has often resulted in an extension of the types of questions asked, and in the classification of objectives.

However, Sockett makes the point that "empirical validation of the Taxonomy has so far been relatively scarce" and "philosophical criticism has been positively scanty". (Sockett 1971, p.16) Sockett considers that the "over-riding criticism is that the Taxonomy operates with a naive theory of knowledge which cannot be ignored however classificatory and neutral its intentions". This criticism is developed in his article (Sockett 1971) and in a subsequent article by Pring (Pring 1971). The criticisms that I would like to develop in this chapter are different from those developed by Sockett and Pring, although they are also concerned with the foundations of the Taxonomy and do draw on some points made by Sockett and Pring. These criticisms will, I hope, show more clearly what Bloom et al have achieved, by removing those claims that seem to me, to be unfounded. The main areas of criticism will concentrate on the assumptions made about learning, the terms or categories defined by the Taxonomy, and what it is that these terms are categorising. The basis of my criticisms will be a questioning of the assumptions that: (i) it is always possible to describe educational outcomes in terms of behaviour, and (ii) these descriptions can be made precise and unambiguous.
When preparing the first part of the proposed taxonomy, Bloom and his colleagues started from a long list of educational objectives gathered from various institutions and from the literature, which were thought to lie in the cognitive domain. They then established principles that would serve as guidelines in classifying these objectives.

First:
"....the major distinctions teachers make among student behaviours. These distinctions are revealed in the ways teachers state educational objectives."

Second:
"....the taxonomy should be logically developed and internally consistent."

Third:
"....the taxonomy should be consistent with our present understanding of psychological phenomena. Those distinctions which are psychologically untenable, even though regularly made by teachers, would be avoided. Further, distinctions which seem psychologically important, even though not frequently made in educational objectives, would be favourable considered for inclusion."

And finally:
"....the classification should be a purely descriptive scheme in which every type of educational goal can be represented in a relatively neutral fashion."

(Bloom 1956, pp.13/14)

Once these had been formulated, the group started classifying the educational objectives they had collected by first determining "which part of the objective stated the behaviour intended and which stated the content or object of the behaviour. We then attempted to find divisions or groups into which the behaviours could be placed."

This was done by ordering the objectives into various divisions from simple to complex. These were then subdivided, in some cases several times. Each of these subdivisions were then defined "in such a way
that all of us working with the material could communicate with each other about the specific objectives as well as the testing procedures" (Bloom 1956, p.15) In this way the group arrived at the following six major classes which the group felt represented "something of the hierachical order of the different classes of objectives", in the cognitive domain:

1.00 Knowledge
2.00 Comprehension
3.00 Application
4.00 Analysis
5.00 Synthesis
6.00 Evaluation.

A condensed version of the taxonomy is included in the appendix of the book to give an overall view of the classification system and the way the categories have been defined, along with examples of objectives in each category. Therefore I will not attempt to produce a similar, even more condensed version here, but will try to indicate with the use of a small number of examples, the sorts of subdivisions and definitions that Bloom et al. propose. First, examples of the major classes: Knowledge "involves the recall of specifics and universals, the recall of methods or processes, or the recall of a pattern, structure, or setting" (p.201); Comprehension "represents the lowest level of understanding. It refers to a type of understanding or apprehension such that the individual knows what is being communicated and can make use of the material or idea being communicated without necessarily relating it to other material or seeing its fullest implications". (p.204)
The first class contains the most subclasses. Thus Knowledge is divided into: 1.10 Knowledge of Specifics, 1.20 Knowledge of Ways and Means of Dealing with Specifics, 1.30 Knowledge of the Universals and Abstractions in a Field. Each of these is further subdivided, for example: 1.11 Knowledge of Terminology, 1.24 Knowledge of Criteria, etc. Most of the other classes are also subdivided, for example Comprehension contains three subclasses: 2.10 Translation, 2.20 Interpretation, 2.30 Extrapolation. Each of these subclasses is also given a definition, for example, Knowledge of Specifics is defined as the "recall of specific and isolable bits of information." (p.201), and the Knowledge of Specific facts as "Knowledge of dates, events, persons, places, etc." (p.201). Translation is comprehension "as phrased or rendered from one language or form of communication to another" and Interpretation is the "explanation or summarisation of a communication" (p.204/205).

One of the initial purposes of the taxonomy was to provide a framework that would allow for communication among examiners. The first draft was therefore tried out on a large number of people within education to see whether they would agree in their classification of specific objectives and test materials. The taxonomy was also used, by the group that produced it, to classify a large number of objectives. In this way it was hoped both to eliminate ambiguities and to ensure the taxonomy's comprehensiveness. This resulted in the final version in which the classes and sub-classes of educational objectives are defined in three ways:

"The first and major type of definition is represented by verbal description or definition of each class and sub-class.....A second type of definition is provided by the list of educational objectives which are included under each sub-class of the taxonomy.....The third type of definition attempts to make clear the
behaviour appropriate to each category by illustrations of the examination questions and problems which are regarded as appropriate. In a way, this represents the most detailed and precise definition of the sub-class since it indicates the tasks the student is expected to perform and the specific behaviour he is expected to exhibit."

(Bloom 1956, p.44/45)

4.4 Assumptions about learning in the taxonomy

The classification of objectives from simple to complex is based on a behavioural picture of learning: the taxonomy is not simply a classification, it implies relationships between the various classes.

"Taxonomies.....have certain structural rules which exceed in complexity the rules of a classification system."

(Bloom 1956, p.17)

Although the taxonomy is supposedly organised along lines which take account of the distinctions made by teachers, Sockett claims that in practice "the psychological considerations are over-riding."

(Sockett 1971, p.22) These psychological considerations are the behavioural directives we have already seen. The tension between the educational considerations on the one hand, and the behavioural directives on the other probably accounts for the confusion, noted by Eggleston (see below) in the description of the arrangement. Thus Bloom et al. state that the present arrangement of the major classes:

"appears to us to represent something of the hierarchial order of the different classes of objectives. As we have defined them, the objectives in one class are likely to make use of and be built on the behaviours found in the preceeding classes in the list."

(Bloom 1956, p.18)
However, lower down the same page, it is suggested that:

"Our attempt to arrange educational behaviours from simple to complex was based on the idea that a particular simple behaviour may become integrated with other equally simple behaviours to form a more complex behaviour. Thus our classification may be said to be in the form where behaviours of type A form one class, behaviours of type B form another class, while behaviours of type ABC form still another class." (That is, based on a behavioural directive)  

(Bloom 1956, p.18)

Eggleston comments that the latter method of description:

"...could possibly lead to erroneous assumptions. At one point in the description of the taxonomy this statement occurs: "...so long as the simple behaviours may be viewed as components of the more complex behaviours, we can view the educational process as one of building on the simple behaviours'. This statement comes dangerously near to the assumption that this taxonomy of intended behavioural outcomes, ranked in order of complexity, is equivalent to an order in which the objectives will be attained."

(Eggleston 1969, p.81)

In parenthesis Eggleston, generously, suspects that such an assumption is "far from Bloom's real intentions", yet, I do not feel that this suspicion is grounded. The assumption, which Eggleston suggests Bloom comes near to making, is an assumption which forms the basis of behaviourism and Bloom says, in several places, that he accepts this framework. Also, the two different descriptions appear throughout the taxonomy to such an extent that suggests that Bloom et al were unaware of the differences between them, or accepted the consequences of both of them.

The tension between educational and psychological considerations, that produces the above conflicting descriptions of the organisation of the taxonomy also produces a confusion over what is being class-
ified. According to Bloom, one of the first problems raised in the group discussions was whether or not educational objectives could be classified since they:

"...were attempting to classify phenomena which could not be observed or manipulated in the same concrete form as the phenomena of such fields as the physical and biological sciences, where taxonomies of a very high order have already been developed. Nevertheless, it was the view of the group that educational objectives stated in behavioural form have their counterparts in the behaviour of individuals. Such behaviour can be observed and described, and these descriptive statements can be classified."

(Bloom 1956, p.5)

The same point is taken up later when Bloom et al state:

"We are of the opinion that although the objectives and test materials and techniques may be specified in an almost unlimited number of ways, the student behaviours involved in these objectives can be represented by a relatively small number of classes. Therefore, this taxonomy is designed to be a classification of the student behaviours which represent the intended outcomes of the educational process."

(Bloom 1956, p.12)

This assumption that a person's behaviour can be described precisely, and that a classification of these descriptions is equivalent to a classification of behaviour is, I suggest, a fundamental error. Once Bloom et al have committed this error, the ambiguity in the ways of describing the ordering of the taxonomy automatically follow. The simple/complex ordering can have two different meanings, according to whether we are ordering objectives or behaviours. The simple/complex ordering of objectives is, as Bloom implies in the first description, on page 18, an easy/difficult ordering, with the more difficult, and hence higher order objectives building on the easier objectives. However, Bloom also suggests that there is a simple/complex ordering of behaviours, which can be more accurately des-
cribed as an elementary/composite ordering, that corresponds to the ordering of objectives.

The ordering of objectives on an easy/difficult scale is much more easily defended than the ordering of behaviours on an elementary/composite scale. Wittgenstein, in a different context, makes the following comments about simple and composite:

"But what are the simple constituent parts of which reality is composed? - What are the constituent parts of a chair? - The bits of wood of which it is made? Or the molecules, or the atoms? - 'Simple' means: not composite. And here the point is: in what sense 'composite'? It makes no sense at all to speak absolutely of the 'simple parts of a chair'.

"...We use the word 'composite' (and therefore the word 'simple') in an enormous number of different and differently related ways. (Is the colour of a square on a chessboard simple, or does it consist of pure white and pure yellow? And is white simple, or does it consist of the colours of the rainbow? - Is this length of 2 cm. simple, or does it consist of two parts, each 1 cm. long? But why not one bit 3 cm. long, and one bit 1 cm. long measured in the opposite direction?)"

(Wittgenstein 1953, 47)

The only indication Bloom et al give as to what they mean by composite is in the quotation above (see page 91) where complex behaviour is an integration of 'equally simple behaviours'. Since there is, however, no indication of what constitutes a simple behaviour, this definition is of little help. We can infer from the taxonomy that, for example, 'recall of bits of information' is 'simple' behaviour, but we are given no evidence that this is in fact simpler than, for example, 'knowing what is being communicated' or 'being able to make use of the material being communicated'. Nor are we told what 'equally simple' behaviour is integrated with, for example 'recall of information' to produce the behaviour 'knowing what is being communicated'.

Even if we were to suggest that the two behaviours were 'recall' and
'making use of what is recalled', then on what grounds can these be called 'equally simple', and in what sense are they integrated?

On the other hand, we can talk about an ordering of objectives on an easy/difficult scale, and these difficulties do not arise. The suggestion that in order to 'recognise unstated assumptions' (Analysis) one needs to 'use abstractions' (Application), 'explain or summarise communication' (Comprehension), and 'recall specific bits of information' (Knowledge), is supported, not by a theory of learning, but by common experience. Consequently, the justification required for such an ordering is a lot less than is required for the claim that there is a class of behaviours corresponding to 'recognition of unstated assumptions' which is produced by the integration of simpler behaviours corresponding to 'use of abstraction', 'summarisation of communication', etc. In the first case one is simply describing a general experience, for which there are exceptions; in the second case one is proposing a specific mechanism, for which exceptions are not allowed (or at any rate, must be explained).

The evidence that is given for the hierarchy in fact supports an ordering of objectives, according to difficulty, rather than of behaviours, according to complexity. Bloom et al claim that:

"...problems requiring knowledge of principles and concepts are correctly answered more frequently than problems requiring both knowledge of the principle and some ability to apply it in new situations. Problems requiring analysis and synthesis are more difficult than problems requiring comprehension."

(Bloom 1956, p.19)

In order to use this to support the elementary/composite ordering of behaviour, Bloom et al have to distort this finding by assuming that: "problems requiring behaviour A alone should be answered correctly
more frequently than problems requiring AB." (p.18), an assumption for which no justification is given. There is also the fact that the tests for communicability of the taxonomy asked teachers and examiners to use the taxonomy to classify objectives and test items. The tests did not ask for student behaviours to be classified. Thus the agreement that Bloom et al obtained was agreement as to how to classify objectives but not agreement as to how to classify behaviour.

4.5 Terms defined by the taxonomy

Before we can consider in greater depth the terms that are defined and used in the taxonomy, I wish to establish that the objectives on which the taxonomy is based are not precise enough to warrant the claim that it also provides for a classification of behaviour. In order to do this I shall consider examples of objectives from the various categories:

2.10 Translation: The ability to translate a lengthy part of a communication into briefer or more abstract terms.

3.00 Application: The ability to relate principles of civil liberties and civil rights to current events.

4.20 Analysis of Relationships: Skill in comprehending the inter-relationships among the ideas in a passage.

5.30 Derivation of a Set of Abstract Relations: Ability to make mathematical discoveries and generalisations.

6.20 Judgement in Terms of External Criteria: The comparison of major theories, generalisations, and facts about particular cultures.
Now I think it is clear that these objectives are behavioural in the sense that they ask the student to do something; that is, they ask for action by the student. Yet it is equally clear that they do not satisfy Mager's criteria that objectives should state unambiguously what the student should do. I think it is true to say that Bloom et al have accepted a wider definition of the word 'behaviour' than have Mager or Sullivan. For Mager, 'behaviour' means 'overt action' and this implies such observable, easily identifiable activities as reciting, writing, etc.... However, Bloom et al have accepted a definition, which may be described as 'meaningful action'. Thus, for example, they ask a student to 'translate' into 'briefer or more abstract terms', and therefore, although they are asking for behaviour such as writing, this behaviour must be meaningful. Hence the behaviour is not specified unambiguously, since it needs judgement to decide whether a particular piece of writing is a translation into more abstract terms. Inevitably the need for interpretation will introduce ambiguities since it is no longer a question of simply observing the overt actions of the student, or the products of such actions, but of judging whether what is observed is to count as translation. This argument applies equally to the other objectives given.

Furthermore, I do not see how it would be possible to make the objectives more specific, for it seems to me, as was the case for understanding, that there is no particular behaviour (that is no overt actions) that corresponds to translation; that is, there is no single thing that is common to all instances of translation, but rather a network of inter-relations between different instances. (Moreover, I think it is possible to argue, at least in theory, that even
behaviour such as 'writing' is not as unambiguous as Mager maintains, since it is implicit that this writing must be intentional and did not happen by chance, as is also implied by Mager's use of the word 'action', rather than, say, 'movement'.

Once it is accepted that the objectives used by Bloom are not unambiguous it is clear that the teacher has to use his judgement in deciding whether particular behaviour is to count as an achievement of the objective. Therefore, the particular circumstances in which this behaviour occurs become a factor that will affect the teacher's decision. It is the particular circumstances that justify the use of the word 'understanding', and I suggest that this applies equally to the words used in the above objectives. We can ask, for example, is the student translating or reciting from memory? Is he making mathematical discoveries or has he learnt these before?, etc.

Bloom et al appear to recognise this type of objection when they comment that "two boys may appear to be doing the same thing; but if we analyse the situation, we find they are not". (Bloom 1956, p.15/16) However they dismiss this objection by suggesting that the only important factor is the "student's background of experience" (p.16), which is then assumed to be the same as "the learning situations which have preceded the test." (p.51). It is then assumed that it is possible to analyse the relationship between the test problems and the preceding learning situation. In this way Bloom et al suggest that it is possible for the teacher, knowing what he has taught the class, to classify test questions. Thus Bloom et al recognise that it is not only behaviour but also the circumstances in which this behaviour occurs, that is important, but they do not realise the
profound implications of this, namely that the objectives are not
simply descriptions of behaviour but also imply a certain type of
situation in which this behaviour takes place. This reveals the
basic flaw in the assumption that the taxonomy is a hierarchical
ordering of behaviours, for if the same behaviour can be classified
differently according to the different circumstances in which it
occurs, then it cannot be the case that a test problem is classified
higher in the taxonomy because it requires behaviour that consists
of the integration of simpler behaviours. Instead it can only be
that a test problem is classified higher because, due to the parti­
cular circumstances, it is more difficult than it would be in other
circumstances.

Pring suggests how Bloom came to make this error when he examines
the precise way in which Bloom uses the terms. He asks:

"Has he developed a technical use of these terms
such that appeal to what they normally mean is
just irrelevant? This might be the case. If it
is, then one requires two things - firstly, that
the technical use should be clearly defined and,
secondly, that such a use will be preserved con­
sistently throughout the taxonomy. However, upon
examination, it is not possible to find a closely
defined technical use of these terms which is con­
sistently applied. Either Bloom does try to limit
his use of these terms in which case it is not con­
sistently applied or he does not limit his use of
these terms, in which case he trades on their
ordinary usage and thereby introduces all the
complicated epistemology that lies beneath that
usage."

(Pring 1971, p.88)

Thus the point being made here is, do the terms used in the taxonomy
have their ordinary meaning, in which case, surely it would be nec­
essary to show that achievement of the objectives in particular cate­
gories constitutes what is normally meant by the category term, or
has Bloom introduced a technical use. A technical use of these terms
would require that Bloom et al should then use these terms consistently, and not also trade on their ordinary use. The validity of Pring's criticism that Bloom et al do in fact trade on the ordinary usage of the terms can be shown, I think, by considering how Bloom et al managed to communicate the meanings of these terms.

In the previous chapter I considered Wittgenstein's remark that the meaning of a word is determined by its use and cannot be specified just by a definition since the use of a word cannot be completely bounded by a rule. We saw that Wittgenstein says:

" - I shall explain these words to someone who, say, only speaks French by means of the corresponding French words. But if a person has not yet got the concepts, I shall teach him to use the words by means of examples and by practice."

(Wittgenstein 1953, p.208)

This, I suggest, is just what Bloom et al have done: they have explained the meaning of their terms by means of examples. Contrary to what they claim (see above page 8.), that the definitions are the most important part of the taxonomy, it is the examples and test questions that in fact define the categories. As Wittgenstein puts it: "we sometimes demand definitions for the sake not of their content, but of their form" (Wittgenstein 1953, 217). The way the terms in the taxonomy are to be used, and hence their meaning, is given by the examples that illustrate each category not by the definitions of each category. But, as we saw above, these objectives contain words such as 'translate', 'relate', 'discover', etc., and since they are nowhere defined, they must be assumed to have their normal meaning. Hence Bloom et al, can be seen to be trading off the everyday meanings of the use of these words in the examples that, in fact, give meaning to the terms of the taxonomy. If Bloom et al wanted
to introduce technical definitions of the terms used, then they would have to couch the objectives, used to illustrate the different categories, in terms such as 'write', 'recite', etc. We see therefore, that the descriptions of each category of the taxonomy do not provide for any unambiguous use the terms that are supposedly defined by them. If anything, it is the examples of objectives and test questions that show how the terms are used. Therefore, since these objectives and questions depend on the ordinary meanings of words such as 'translate', 'relate', 'judge', etc., the categories, as categories of behaviour, can be no more precise than the meanings of these words.

Although we may wish to give a more precise meaning to 'understanding', we certainly cannot do this in the way that Bloom and his colleagues have tried to. The meanings of everyday words such as 'knowledge' 'understanding', etc., are learnt through seeing examples of various behaviour in various circumstances, which are related in many different ways: there is what we have called a family resemblance between these different behaviours and circumstances. If we wish to introduce new meanings for these words, this is not something that can be done by definitions.

Thus Bloom and his colleagues have succeeded in classifying objectives, but the classes of objectives do not necessarily represent different classes of behaviour, and they certainly do not represent different classes of understanding. In general, the higher order objectives will be more difficult than the lower order ones, simply because they incorporate the lower order ones. Indirectly what Bloom et al have done, is to draw attention to the different types of circumstances in which we use the word 'understand': in some cases a claim that a
person has understood is justified by his applying certain knowledge, in others by his abstracting from a text, in yet others by his translating a text, and so on. However, these different behaviours are simply the different criteria that we are prepared to accept for the correct use of the word 'understanding'; they do not stand for different types of understanding.
5.1 Types of Learning

Having seen how the behavioural directives have been applied within educational technology, I now want to look at an account of learning based on behaviourism. Gagne, in his book *The Conditions of Learning* (Gagne 1970), sets out to describe the conditions under which learning occurs. However, despite his claim that he is merely describing learning and not putting forward a theory of learning, his account, as we shall see, is based on behavioural directives. A close analysis of these directives and of the application of this account of learning to the learning of language, will, I hope, finally demonstrate the inappropriateness of an approach to educational problems (such as those associated with mathematics for science students) that is based on such directives.

Gagne suggests that the conditions required for learning are both 'internal', that is, the capabilities that the learner requires for a particular type of learning, and 'external', by which Gagne means the observable learning situation. According to Gagne:

"...there are as many varieties of learning as there are distinguishable conditions for learning. In searching for and identifying these, one must look, first, at the capabilities internal to the learner, and second, at the stimulus situation outside the learner. Each type of learning starts from a different 'point' of internal capability, and is likely also to demand a different external situation in order to take place effectively."

(Gagne 1970, p.24)
In all, Gagne isolates eight different types of learning, arranged in a hierarchical order, where, according to Gagne, the higher types build on the lower types, so that the lower order types of learning feature as internal conditions for the higher order types. These are: Signal Learning, Stimulus-Response Learning, Chaining, Verbal Association, Discrimination Learning, Concept Learning, Rule Learning, Problem Solving.

Type 1: Signal Learning. "The individual learns to make a general, diffuse response to a signal. This is the classical conditioned response of Pavlov." (p.63)

Type 2: Stimulus-Response Learning. "The learner acquires a precise response to a discriminated stimulus." (p.63) According to Gagne, this is probably the building block of all higher learning. We shall see that Stimulus-Response learning appears among the conditions Gagne gives for all higher learning. According to Gagne, Stimulus-Response learning is easily observed in the field of animal learning (where it originated), but is seldom observed in its pure form in human learning. Instead, what is observed are the more complex forms of chains of Stimulus-Response or verbal associations. Gagne accounts for this by saying that human beings rapidly acquire this background of learning which then enters into more complex learning. He suggests that this "form of learning appears to govern the acquisition of a new vocalisation habit of a young child." (p.41)

Type 3 and 4: Chaining and Verbal Association. In chaining the learner acquires "a chain of two or more stimulus-response connections" (p.63). "Verbal association is the learning of chains that are verbal"
The formation of chains, both verbal and motor, is dependent on the learner first having acquired the individual links.

Type 5: Discrimination Learning. The learning of different chains tend to interfere with each other, and discrimination learning involves acquiring the ability to recognise different stimuli and make the appropriate identifying responses. As before, this is built upon the previous types, since the learner must first have learnt the individual chains connecting each distinctive stimulus with its identifying response.

Type 6: Concept Learning. "The learner acquires a capability of making a common response to a class of stimuli that may differ from each other widely in physical appearance. He is able to make a response that identifies an entire class of objects or events". (pp.63/64) Gagne makes it clear that he is considering "....the most fundamental meaning of the term 'concept', which is exhibited in individual behaviour by responding to a class of observable objects or object qualities...." (p.172). As before, concept learning is based on previous types of learning. "Prerequisites to the learning of concepts are capabilities that have previously been established by multiple discrimination. A set of verbal (or other) chains must have previously been acquired to representative stimulus situations that exhibits the characteristics of the class that describes the concept, and that distinguish these stimuli from others not included in the class." (p.180 - original italics)

Type 7: Rule Learning. "In simplest terms, a rule is a chain of two or more concepts" (p.64). This learning is according to Gagne,
truly representative of human intellectual capacity, yet is built up from previous learning types. Included in rule learning is the learning of 'abstract concepts' (as opposed to the previous 'concrete concepts') which are defined by a rule that relates two or more simpler concepts. Gagne defines a rule as "an inferred capability that enables the individual to respond to a class of stimulus situations with a class of performances" (p.191). These rules are not normally learnt in isolation but in related sets. The rules of the set are related to each other "in the psychological sense that the learning of some are prerequisite to the learning of others....." (p.203). In this way we can arrive at a learning hierarchy in which, not only may two or more concrete concepts be subordinate to a rule, but also two or more rules may be subordinate to a further rule. This rule may then itself be combined with one or more other rules to produce another, even higher order, rule. "The entire set of rules, organised in this way, forms a learning hierarchy that describes an on the average efficient route to the attainment of an organised set of intellectual skills that represent 'understanding' of a topic." (p.204)

Type 8: Problem Solving. "Problem solving is a kind of learning that requires the internal events usually called thinking. Two or more previously acquired rules are somehow combined to produce a new capability that can be shown to depend on a 'higher order' rule" (p.64). Problem solving is thus a natural extension of rule learning. It can be classed as learning since it not only requires the use of rules to achieve a particular goal, but also in the process the learner discovers for himself new, higher-order rules which he is able to use in other situations. Obviously, this problem solving
requires that the learner has previously acquired a set of rules, which in turn depend on the prior learning of concepts, discriminations, etc. Thus problem solving can be seen as being at the top of the hierarchy of learning types.

5.2 Relevance to teaching problems

Gagne has claimed to have described the conditions necessary for all types of learning, where these conditions are both internal and external. If therefore, this claim is valid, then it should be possible to describe the types of learning that obtain when learning mathematics for science. In this way, it should be possible to produce a learning hierarchy or learning map for the students. The purpose of arriving at such a hierarchy is that according to Gagne, "the superordinate capability will be more readily learned (on the average, throughout a group of students) if the subordinate capabilities have been previously acquired and are readily available for recall." (p.239) Thus, if these claims are valid, then the problems associated with teaching mathematics to science students could be solved, or at least reduced to a minimum, by producing such hierarchies. Although the hierarchies represent the internal conditions, rather than the external conditions, they nonetheless assist the planning of a sequence of instruction.

"Thus it becomes possible to 'work backward' from any given objective of learning to determine what the prerequisite learnings must be - if necessary all the way back to chains and simple discriminations. When such an analysis is made, the result is the kind of map of what must be learned."

(Gagne 1970, p.242)
To obtain a learning hierarchy, we start from the end-point and ask: "What would the student have to know how to do in order to be instructed in this rule?" (p. 207). Thus we arrive at lower order rules or concepts and we can repeat the process with each of these. This process can again be repeated until we arrive at a complete enough description of learning. Gagne gives several examples of such hierarchies, in English, Mathematics and Science. For the learning hierarchy in science, Gagne started with the topic "solving physical work problems", and suggested that in order to solve these problems, the student has to learn the following sorts of capabilities:

"1. determining the numerical values for the variables \textit{force} and \textit{distance};
2. translating concrete verbal problems into mathematical statements; and
3. solving simple equations."

(Gagne 1970, p. 264)

Thus we see that by starting with a topic in science we arrive at the mathematical capabilities that the student requires. These capabilities can then be further analysed so as to obtain a learning hierarchy for the students' mathematical capabilities.

Notice that in this example, the mathematics is being used as a tool in solving a particular sort of problem in physics and so it can be specified in terms of particular capabilities, in this case, the capability to "solve simple equations of the form $a = b \cdot c$ to obtain value of $a$" (p. 263). Here the approach suggested by Gagne would supplement the systems approach to problems where the terminal behaviour intended can be isolated. An integration of the two approaches would enable the teacher not merely to obtain objectives against which the student's mastery of the material can be measured on completion of the course, but also would enable the teacher to devise intermediate
objectives and tests to monitor student's progress through the course. The hierarchy would also enable the teacher to pin-point particular areas of difficulty.

However, before we can accept that such a process will help with the problems associated with teaching mathematics as a tool, we must consider how a teacher would answer the question 'what would the student have to know how to do in order to be instructed in this rule?'. The question, by itself, does not suggest the sort of answer that is required, and in particular, it does not eliminate the possibility that the teacher could answer by stating what logically precedes the particular rule. Now if this were all Gagne intended, then he could not claim the resultant hierarchy as a learning hierarchy, but merely as a hierarchy that showed the logical structure of the subject. Thus the hierarchy would simply formalise the teacher's intuitive knowledge of the subject. However, it is clear that this is not what Gagne intends, and he requires that the hierarchy should not show how the various rules are logically related but how they are psychologically related in the "sense that the learning of some are prerequisite to the learning of others". Yet if this is what Gagne intends, he gives no indications as to how the psychological rules differ from the logical rules, and hence no guidelines for arriving at a learning hierarchy rather than a hierarchy showing simply the structure of the subject.

There are further problems in answering the question as to what capabilities the student requires for learning a particular rule. Gagne suggests that successive answers to the question will establish a hierarchy that not only consists of rules, but also concepts, discrimin-
atations, chains, etc. However, in order for the teacher to obtain a hierarchy that includes these various types of learning, he has to accept Gagne's account of learning; by himself, the teacher is unlikely to arrive at the hierarchy, simply by putting down what he considers to be prior capabilities. Thus, although Gagne claims that he has not produced a learning theory, it is clear that he has gone beyond simply describing the conditions under which learning occurs. In fact, Gagne's view of learning is based on the behavioural assumptions that (i) all learning is of a hierarchical nature, and builds on simpler types of learning, (ii) that these lower order types of learning are part of the necessary conditions for the higher order types of learning and (iii) that all learning is ultimately based on Stimulus-response type learning. Gagne's descriptions can only be considered within the framework of these assumptions; they cannot therefore be considered purely as descriptions of observable changes in human behaviour.

5.3 Language learning

In describing the conditions of learning in science, Gagne assumes that learning is simply the learning of skills, albeit intellectual ones. However, while it is undoubtedly true that science students must learn intellectual skills, this is clearly not all they learn. Toulmin, for example, suggests that the recognition of a common explanatory goal is crucial to the making of a scientific discipline, and that one of the things that divides science from technology is that in the former case the common goal is an explanatory one, whereas
in the latter it is the development of skills and procedures, etc., to fulfill human needs." (Toulmin 1972, pp.364/365) Consequently, since explanation is bound up with language, I think it is necessary to consider Gagne's treatment of language learning and compare his approach with some current linguistic theories, in particular that proposed by Chomsky. I suggest that only if Gagne is able to provide a satisfactory account of language learning will his descriptions of the conditions necessary for learning be useful in solving the problems associated with teaching mathematics as a language in science.

Recent advances in linguistics and in the new discipline of psycholinguistics have considerably changed the current view of language, and have resulted in an increase in both the quality and the richness of linguistic data that must be accounted for in any theory of learning. One of the major contributors to this advance is Chomsky, who proposed that it is, at least theoretically, possible to construct an infinite number of grammatically correct sentences in a language. According to Chomsky, a sentence is grammatically correct provided it does not contravene any syntactical rules. Hence, although there may be a practical limit on the length of a sentence that is capable of being understood by someone competent in the language, there is no theoretical limit. In any case, whether or not we would wish to impose a practical limit on the length of a sentence, the general point made by Chomsky is that a language contains a very large number of grammatical sentences and fluent speakers of a language are constantly producing and understanding sentences that they have not previously encountered. This inevitably creates problems for any theory of learning. Chomsky makes the point as follows:
"We constantly read and hear new sequences of words, recognise them as sentences, and understand them. It is easy to show that the new events that we accept and understand as sentences are not related to those with which we are familiar by any simple notion of formal (or semantic or statistical) similarity or identity of grammatical frame. Talk of generalisation in this case is entirely pointless and empty. It appears that we recognise a new item as a sentence not because it matches some familiar item in any simple way, but because it is generated by the grammar that each individual has somehow and in some form internalised. And we understand a new sentence, in part, because we are somehow capable of determining the process by which this sentence is derived in this grammar..... The child who learns a language has in some sense constructed the grammar for himself on the basis of his observation of sentences and non-sentences (i.e. corrections by the verbal community). Study of the actual observed ability of a speaker to distinguish sentences from non-sentences, detect ambiguities, etc., apparently forces us to the conclusion that this grammar is of an extremely complex and abstract character, and that the young child has succeeded in carrying out what from the formal point of view, at least, seems to be a remarkable type of theory construction. Furthermore, this task is accomplished in an astonishingly short time, to a large extent independently of intelligence, and in a comparable way by all children. Any theory of learning must cope with these facts.....The fact that all normal children acquire essentially comparable grammars of great complexity with remarkable rapidity suggests that human beings are somehow specially designed to do this, with data handling or 'hypothesis-formulating' ability of unknown character and complexity."

(Chomsky 1967, pp. 170/171)

Although Gagne has not tried to develop a theory of learning, he has tried to classify all learning under one or more of eight different types, and describe the conditions under which this learning occurs most effectively. Since we are interested in language learning, one test of how useful Gagne's work is likely to be can be obtained by seeing how well Gagne is able to describe the sorts of learning processes that Chomsky outlines. Gagne holds that learning is hierarchical and from his eight types of learning we can suggest the following scheme by which a child learns a language: By a process of verbal association based on Stimulus-Response learning and chaining, the
child learns to construct verbal chains. He then learns to discriminate between different stimuli and produce different verbal chains. Later, by concept-learning, he will learn to make a common verbal response to a class of stimuli. This is followed by rule learning. Gagne acknowledges the importance of rule learning and suggests that in "language learning, the individual acquires rules for pronouncing, for spelling, for punctuating, for constructing ordered sentences." (Gagne 1970, p.193)

We can see that this account differs considerably from the sort of outline account given by Chomsky. According to Gagne, the child slowly builds up successive capabilities, with each capability requiring certain specific internal and external sets of conditions, whereas Chomsky suggests that learning occurs irrespective of particular conditions. Also, although rule-learning is important in Gagne's description, it comes at the end of the learning process, after the child has learnt verbal chains and (concrete) concepts. On the other hand, Chomsky suggests that a child must have an innate ability to construct linguistic rules in order to be able to isolate words and sentences from the stream of speech that he is exposed to. Thus, according to Chomsky, if we recognise the fact that language learning is accomplished over a similar period of time by all children, then it cannot rely on particular sets of conditions; a child must simply be able to abstract linguistic rules from the speech he hears.

Obviously, a Stimulus-Response type of learning can describe some aspects of language learning, but it cannot have the importance that Gagne places on it as underlying all learning. We can also show up an inconsistency in Gagne's descriptions of the conditions for rule learning, if we accept Chomsky's suggestion that rule learning is fundamental to language learning. Gagne's conditions for rule learning
can be conveniently summarised as follows:

"Step 1: Inform the learner about the form of the performance to be expected when learning is completed.

Step 2: Question the learner in a way that requires the reinstatement (recall) of the previously learned concepts that make up the rule.

Step 3: Use verbal statements (cues) that will lead the learner to put the rule together, as a chain of concepts, in the proper order.

Step 4: By means of a question, ask the learner to 'demonstrate' one of the more concrete instances of the rule.

Step 5: (Optional, but useful for later instruction): By a suitable question, require the learner to make a verbal statement of the rule."

(Gagné 1970, p.203)

Now obviously, these conditions require that the child has already learnt a language, and so these conditions are clearly not applicable in the case of learning a first language.

Thus, although rule learning is considered important by both Chomsky and Gagne, it is treated in a fundamentally different way by each of them. For Gagne it occurs at the apex of the other types of learning (except problem solving), whereas for Chomsky, it must, in order to explain the facts of language learning, be present from the start of language learning. For Gagne one of the conditions necessary for effective rule learning is the verbal statement of the rule. For Chomsky, the child must construct the rule himself 'internally' and prior to language. Therefore, Gagne's account of language learning contradicts the apparent facts that Chomsky draws our attention to and this must cast doubts on the underlying behavioural assumptions that Gagne's account is based on.
So far I have been considering the learning of a first language; however, in the learning of a second language, and for our purposes we can include mathematics, we have a different situation. It may, of course, be possible to learn a second language in the same way as a first language, but normally the learning is different, since the linguistic rules of the second language can be expressed in terms of the first language. Here, the use of a verbally stated rule as suggested by Gagne, becomes a significant feature. Nonetheless, I have suggested above that Gagne's conditions for rule learning are not necessary, and it is perfectly conceivable that language learning could occur without the use of explicit rules.

I shall now go on to consider whether Gagne's conditions are sufficient, and again, I suggest, there are some serious criticisms that can be proposed. These criticisms arise when we consider how one learns the meaning of words. The only way we can describe the learning of the meaning of words in Gagne's scheme is by concept learning which occurs prior to rule learning. However, we must remember that 'concept' does not have its normal meaning but is given a technical meaning by Gagne, so that concept learning is exhibited in the common response to a class of observable objects. The picture of language that is implied by this, where a word is correlated with a meaning which is the object (or class of objects) for which the word stands, is, as we have seen, one specifically criticised by Wittgenstein. According to Wittgenstein, in such a picture there is no recognition that there is:

"....any difference between kinds of word. If you describe the learning of language in this way you are, I believe, thinking primarily of nouns like 'table', 'chair', 'bread', and of people's names, and only secondarily of the names of certain actions and properties; and of the remaining kinds of word as something that will take care of itself."

(Wittgenstein 1953, 1)
In the case of learning a meaning of a word, I feel that Chomsky fares little better than Gagne. In the later version of his theory, Chomsky produces a grammar containing both syntactic rules and semantic rules (Chomsky 1965), but it is by no means clear that these semantic rules could play any part in language learning, and in any case it is generally recognised (for example see Greene 1972, p.72) that such rules are incapable of completely determining the meaning of words, since the meaning of, at least certain words, also depends on features in the world as well as linguistic rules. A further critique of Katz and Fodor's theory (see Katz and Fodor 1967), on which Chomsky based his semantic rules, that makes the use of semantic rules in language learning doubtful, is given by Bolinger (see Bolinger 1967).

I shall therefore continue to follow the treatment of meaning developed by Wittgenstein, which stresses the relationship between the meaning of a word and its use. As we have seen, this use cannot be governed by rules, and a person does not learn meaning through learning rules. Thus I suggest that the learning of meaning can only be explained in certain limited cases by Gagne's concept learning, and also cannot be explained by means of rule learning. In fact, it would appear that Gagne acknowledges, at least implicitly, that his types of learning do not satisfactorily account for the learning of meaning:

"While the learning of verbal sequences, including those containing meaningful words, may be usefully conceived as a process of 'chaining', it appears doubtful that this conception is capable of encompassing the whole of what is meant by verbal learning. When a student 'learns' a chapter in his history text, for example, one does not expect him to demonstrate this learning by a verbatim oral reproduction of the text. Instead, one may expect him to be able to reproduce the 'ideas' that are contained in the text in the proper order."

(Gagne 1970, pp.147/148)
He concludes that the learning of meaningful propositions may require a different internal processing from verbal learning.

The use of the vague term 'idea', in the above quotation, where concept in its normal sense would have been equally, if not more, appropriate, suggests that Gagne has difficulty in describing the learning of meaningful explanation in terms of one or more of his types of learning. For example, he implies that, as he has defined the word 'concept', the learning of ideas cannot be considered as 'concept learning', and hence that 'concept' cannot be used in place of 'meaning'. Thus, although verbal chaining may be a useful way of conceiving the learning of explanations, Gagne admits that it is not entirely satisfactory. In fact, I would suggest that verbal chaining is an entirely unsatisfactory way of conceiving the learning of explanations and that Gagne is incapable of explaining the learning of meaning and meaningful explanation just because of the behavioural assumptions he has made as a basis of his approach.

This inability to describe the learning of explanations places a serious limitation on the extent to which Gagne's "Conditions of Learning" can help with the problems associated with teaching mathematics as a language for science. Although in the above quotation Gagne explicitly rejects the idea that explanations can be learnt by a process of verbal chaining, he contradicts himself when later considering mathematics learning:

".....mathematics is preponderantly composed of intellectual skills and not very much verbal information. Regarding the latter,.....these are the items that must be 'memorised', or stored mainly as verbal chains. But all the rest is a set of intellectual skills....."

(Gagne 1970, pp.246/247)
Although Gagne is using what appears to be a technical term 'verbal information', with the implication that information can be stored (as in files), so that the memorising of information is analogous to the filing of information, this technical term can, in this context, only refer to explanations. While mathematics does contain what could be classed as verbal information, for example, "the area of a square is the product of its length and breadth", it also contains explanations which cannot simply be stored. As Gagne himself points out, if these explanations were simply stored as chains, then one could only expect the student to reproduce these verbatim.

Now I noted earlier (page 109) that Toulmin stressed the importance of explanations, as opposed to skills, in science. If therefore, mathematics is to play the role of a language in science, then it must play an important part in the explanatory aspects of science. Hence, even if we were to accept Gagne's claim that mathematics (that is pure mathematics) is composed of intellectual skills and verbal information that is simply memorised (and I, for one, would not accept this), we could not accept this in the case of mathematics that is used in science. Thus, particularly when we are concerned with the role of mathematics as a language of science, we are concerned with just those aspects of learning that cannot be described in terms of Gagne's types of learning.

In order for mathematics to be used as a language, it obviously must have meaning in the context in which it is being used. I suggest that in the same way that the meaning of a word cannot be given by a rule, nor can the meaning of a mathematical symbol or expression. Clearly symbols and expressions in mathematics are defined, by means
or a rule, but this does not define their meaning, in that it does not define how these symbols and expressions are to be used linguistically in science. It is perfectly true that some symbols are used to refer to physical entities, but this does not mean that we can explain the meaning of mathematical symbols by 'pointing at' the physical entity for which they stand. To suggest this is again to have a too simple view of language, and although in the case of, for example, the symbols 'p' for pressure, 'v' for velocity, etc., we can point to a physical entity, this is not the case for symbols such as $\frac{dy}{dx}$, $\int$, etc. Also even in the case of p, v, etc., by pointing to the physical entity for which they stand, we have not shown how we are going to use these symbols.

5.4 Criticisms of the behavioural framework

In order to demonstrate that the objections I have brought against Gagne's account of learning are fundamental ones that cannot be removed by alterations within the behavioural framework, I shall present some of the criticisms Chomsky brings against Skinner in his review of Skinner's "Verbal Behaviour". As Chomsky later explains:

"I had intended this review not specifically as a criticism of Skinner's speculations regarding language, but rather as a more general critique of behaviourist (I would now prefer to say 'empiricist') speculation as to the nature of higher mental processes..... The conclusion that I hoped to establish in the review, by discussing these speculations in their most explicit and detailed form, was that the general point of view is largely mythology, and that its widespread acceptance is not the result of empirical support, persuasive reasoning, or the absence of a plausible alternative."

(Chomsky 1967, p.142)
In the review Chomsky criticises Skinner's use of the terms 'stimulus', 'response' and 'reinforcement':

"The notions 'stimulus', 'response', 'reinforcement' are relatively well defined with respect to the bar-pressing experiments (with rats) and others similarly restricted. Before we can extend them to real life behaviour, however, certain difficulties must be faced. We must decide, first of all, whether any physical event to which the organism is capable of reacting is to be called a stimulus on a given occasion, or only one to which the organism in fact reacts; and correspondingly, we must decide whether any part of behaviour is to be called a response, or only one connected with stimuli in lawful ways. Questions of this sort pose something of a dilemma for the experimental psychologist. If he accepts the broad definitions, characterising any physical event impinging on the organism as stimulus and any part of the organism's behaviour as a response, he must conclude that behaviour has not been demonstrated to be lawful...... If we accept the narrower definitions then behaviour is lawful by definition (if it consists of responses); but this fact is of limited significance, since most of what the animal does will simply not be considered behaviour. Hence the psychologist either must admit that behaviour is not lawful......, or must restrict his attention to those highly limited areas in which it is lawful."

(Chomsky 1967, p.147)

This dilemma, as to what is to count as stimulus and similarly what is to count as response is clearly one that ought to be faced by Gagne:

"An observer of learning must deal with an input, an output, and a functioning entity in between. The input is a stimulus situation (S), which includes the varieties of changes in physical energy that reach the learner through his senses....The stimulus situation is in general....outside the learner and can be identified and described in the terms of physical science.... The output R, is also in a real sense outside the learner. It is a response of a set of responses that produces an identifiable product."

(original italics) (Gagne 1970, pp.33/34)

However, Gagne tries to avoid the dilemma by suggesting that:

"Events in (the learner's) environment affect the learner's senses, and start chains of nervous impulses that are organised by his central nervous system, specifically, by his brain. This nervous activity occurs in certain sequences and patterns

-119-
"that alter the nature of the organising process itself, and this effect is exhibited as learning. Finally, the nervous activity is translated into action that may be observed as the movement of muscles in executing responses of various sorts."

(Gagne 1970, p.4)

Thus Gagne suggests that a stimulus is not any physical event that impinges on the learner, but one that affects the nervous system, similarly, a response is not any behaviour by the learner but one connected to stimuli by activity of the central nervous system. However, as Gagne points out: "The nature of the connection between an S and an R cannot be directly observed" (p.34). But, we can suggest, if the connection cannot be observed, then the terms 'Stimulus' and 'Response' cannot be defined in terms of the connection, the connection can only be inferred from observing stimuli and responses. In any case, I do not think that a definition in terms of nervous activity would be of any use since even if this activity could be observed, there would remain the difficulty of establishing a one-to-one relationship between events in the physical environment and events in the nervous system.

The above arguments, I think, support Chomsky's contention that the general use of the terms 'stimulus' and 'response' merely give an "illusion of a rigorous scientific theory" (Chomsky 1967, p.147). Gagne's supposed definitions of these terms by reference to the activity of the central nervous system cannot be in fact considered as definitions but simply as expressions of the belief that, given suitable developments in neuro-physiology, some neutral connection between stimulus and response will be established. However, to infer this neutral connection implies that one can identify a stimulus and a response independently. In fact if we look more closely at Gagne's use of the terms 'stimulus situation' and 'response', as opposed to
his definitions, we see that by 'stimulus situation' Gagne normally means the teaching situation or a specific aspect of the teaching situation, and by 'response' Gagne normally means the behaviour that the teacher wishes to bring about. Unless stimulus and response acquire their meaning in this way, independently of each other and of activity of the central nervous system, the following learning scheme does not make sense:

"Stimulus ——> (nothing) to Stimulus ——> Response"

(Gagne 1970, p.6)

For, if the stimulus resulted in nothing, then it would not be a stimulus. If on the other hand 'Stimulus' is a stimulus, then it must have resulted in something, and so the term 'Response' used in the second but not the first case must be reserved for specific sorts of responses, namely those acceptable to the teacher.

If it is accepted that the only purpose served by the terms 'Stimulus' and 'Response' is to give Gagne's descriptions the illusion of objectivity, then it is clear that Gagne is not simply describing the conditions of learning. In any case, as I have already mentioned, Popper dismisses the possibility of simply observing. Using the term 'theory' in the way it is used by Popper in the quotation given in section 2.3, we can say that Gagne is not simply observing facts, but interpreting them in the light of a theory of behaviourism. This suggestion gains further support from analysing the language in which Gagne's descriptions are given. For example, the conditions Gagne gives for 'Verbal Association' include the following terms: 'chains of Ss-R's', 'discrimination of stimuli', 'mediating connection', etc., as well as the terms 'stimulus', 'response' and 'reinforcement'. (See Gagne 1970, p.141/142)
By considering Gagne's use of terms such as 'Stimulus' and 'Response', I hope I have gone at least some way towards demonstrating that the behavioural directives do not result, or do not necessarily result, in an objective scientific approach, but sometimes only an illusion of one. As Chomsky suggests, their acceptance is not the result of empirical support or persuasive reasoning. Thus there is no reason to believe that learning can only be described by theories based on behavioural directives, and some good reasons for believing it cannot.
I mentioned at the beginning of this thesis that the results obtained from a theoretical investigation could not be the same as those obtained from an empirical investigation. Nor could they be a substitute for empirical results. I do not even wish to suggest that the problems associated with mathematics for science can only be solved by a theoretical investigation, but rather that they cannot be solved by the sorts of empirical investigations currently used in educational technology. Thus my philosophical investigation is not a substitute for an empirical one, but a precursor. I have not tried to give answers to questions, I have tried to show that the questions being asked are the wrong ones. For clearly, if the questions are inappropriate, then no amount of empirical research will yield right answers. However, for someone who expects answers, it may seem that the thesis is irrelevant and inconclusive. Yet, until such a person recognises the need to look more critically at the questions he is asking and the assumptions he is making, there is little that can be said to persuade him to revise these opinions. On the other hand, there may be someone who recognises this need, but who is not sure what has been achieved in the thesis. It may therefore be useful to briefly trace the thread of the various arguments as they have developed from chapter to chapter.

The first chapter started from the suggestion that the mathematics used in a science should not be considered in isolation from that science. By looking at Kuhn's view of science, from an educational context, it was possible to suggest that some of the problems science students have with mathematics are due to the different standards
and values expected of them in the two classes. Mathematicians and
scientists are committed to different disciplinary matrices, they
share different forms of life. The science student learning math¬
ematics is not simply engaged in cognitive learning, he is also, in
a way that is intricately connected, learning different values and,
in a sense, different attitudes towards his work. Although it is
generally recognised that science students have attitudinal or af-
fective problems with mathematics, this is too often construed in
terms of whether or not they like mathematics, whether or not they
think it is important, and so on. The answers to these questions
are, I think, totally irrelevant to the student's major attitude
problems which, I suggested, arise out of inappropriate values in
such things as accuracy, consistency, simplicity, elegance, etc.
It is in this sort of area that I feel empirical research into af-
fective problems should be directed.

In chapter two I moved on to look at the systems approach to educational
problems, and at the assumptions underlying this approach. The systems
approach is claimed to be 'scientific', but, as I tried to establish,
its methodological directives derive from a particular view of science
This view of science, which originated with the logical positivists,
has been criticised by many philosophers of science. One of the im-
plications of this widespread questioning of logical positivism or
logical empiricism is that a scientific status cannot be claimed for
the systems approach simply because it is based on logical empiricism,
and that, therefore, the systems approach may be susceptible to a
philosophical critique. This is just what I have attempted in trying
to show that a word such as 'understanding' cannot be replaced by a
behavioural objective. The idea that it is possible to specify all
educational outcomes by means of precise behavioural objectives stems from an inappropriate theory of meaning, namely the verification theory, which in turn rests on a philosophical confusion about how we use words such as 'understanding'. If understanding could be verified by whether or not certain behaviour was exhibited, then it would be possible, and probably desirable, to find more precise specifications of this behaviour. However, as I tried to show later, in chapter four, 'understanding' is not this sort of a word; we can produce no descriptions of behaviour which, if they occurred, would necessarily entail understanding.

In criticising the systems approach, I have not tried to deny that some educational outcomes can be expressed by means of behavioural objectives, nor that there might be considerable gain from this. In particular it might be that where mathematics is used as a tool some mathematical skills, required by the science student, could be specified in fairly precise behavioural form. A systems approach to the teaching of these skills could well result in a considerable improvement on conventional courses. On the other hand, it must be realised that I have tended to emphasise the distinction between the uses of mathematics as a tool and as a language in order to stress that the mathematics cannot simply be replaced by a calculating machine. In practice there is likely to be considerable overlap between these two uses, possibly to such an extent that they could not be taught separately. In which case, there is the real danger that by using the systems approach, the language aspects of mathematics are under-valued.

In chapter three I took a closer look at concepts in mathematics and
science, and at some studies that have been made to describe what is involved in so-called concept learning. The assumption made in these studies is that a word is a label for a concept, so that, for example, 'energy' stands for a certain concept in the same way that 'Jack' stands for a certain person. Such studies have relevance to the problems associated with mathematics for science since it is thought that when a person learns mathematics, he not only learns various skills, but also various concepts. It is these concepts, such as function, limit, integral, that the science student needs, as well as skills such as being able to differentiate and integrate. If the above assumptions were valid we could suggest that some of the science students' problems could be removed as follows: an analysis of the science course would show up those mathematical concepts required, which could then be taught, in an efficient way, to the student, through the satisfaction of various general conditions for concept learning. Students could then be tested to see whether these concepts had been transferred to the science class, in the same way that they can be tested to see whether they have transferred certain skills.

What then is wrong with these proposals? I suggest that what is wrong is the particular picture of a concept they are based on. Wittgenstein opposes this picture, of a concept word being the name of a general attribute or of a class, with a picture of a concept being given by the use of a word in a language-game. By means of this expression 'language-game', Wittgenstein draws a powerful analogy between speaking a language and playing a game, and one effect is to stress that speaking a language is an activity governed by certain rules. Two consequences follow immediately from this. The first is that if learning a concept is learning the use of a word in a
language-game, then the learning of concepts no more involves a particular type of learning than the learning how to play games. In the same way that we do not say that a person is involved in a particular type of learning (game learning) when learning how to play, for example, tennis, chess, patience, ring-a-ring-a-roses or Russian roulette, so we ought not to speak of a particular type of learning (concept learning) when a person learns the meaning of, for example, energy, table, high, tolerance, derivative. If we are tempted to say that there must be something in common to the learning of different games or different concepts, then I can only repeat Wittgenstein's advice: "Don't say: 'There must be something common....' - but look and see whether there is anything in common at all." (Wittgenstein 1953, 66)

The second consequence is that we cannot talk of transferring concepts if this involves moving from one language-game to another. Thus although the same words, such as 'function', 'convergence', 'plane', and the same symbols, such as 'x', \( \frac{dy}{dx} \), \( \frac{\partial y}{\partial x} \), are used in both mathematics and science, we are not necessarily dealing with the same concepts, since these words and symbols are not simply labels. The uses and functions of these words and symbols will differ in mathematics and science, although they will clearly be related, and, to the extent that they differ, we are dealing with different concepts. It is for this reason that the student is not simply faced with a 'transference problem', although this fact is effectively concealed, both by an inappropriate and too simple view of a concept, and also by isolating the mathematics from the science.

Having, I hope, shown that it is not possible to tackle the trans-
ference of concepts in the same way that one might tackle the transference of skills, I returned to a consideration of understanding, since, I felt, it was only through an analysis of understanding that it was possible to get to the root of the problems associated with mathematics for science. In chapter four, therefore, I developed an argument that was introduced in section 2.5 where I denied that it was possible to give descriptions of behaviour that necessarily entailed understanding. I felt it equally necessary to also discount an apparently opposed claim, namely that understanding was something, as it were, ineffable, some unobservable mental state. Again using arguments first put forward by Wittgenstein, I tried to draw attention to the particular circumstances in which we say that someone understands. Just because we cannot give behavioural descriptions that entail understanding, we are not precluded from saying that, in particular circumstances, certain behaviour counts as understanding, that is, is an observable criterion for the correct use of the word 'understanding'. In trying to find behavioural descriptions that entail understanding, or mental states that are the essence of understanding, we are confused by the idea that 'understanding' refers to something. To remove this confusion we must look at how the word 'understanding' is actually used.

It is this idea, that 'understanding' refers to something, that appears to have confused Bloom and his colleagues. They recognised that 'understanding' is used in different sorts of circumstances and for different sorts of behaviour, but construed this as implying that there were therefore different types of understanding. Working from these assumptions they tried to remove ambiguities in the meaning of the word 'understanding' by classifying the various behaviours.
There are several aspects of this endeavour that can be criticised: first, the failure to realise that the objectives they were working from were not precise descriptions of behaviour, and hence that they were not classifying behaviour. Second, their assumption, which as we have seen was a methodological directive imposed by behaviourism, that there is a hierarchical ordering of behaviours from simple to complex. Thus they confused an easy/difficult ordering of objectives with an elementary/complex ordering of behaviours. Third, their belief that they had produced precise definitions of the various terms in the taxonomy and that these terms, precisely defined, could replace vaguer words such as understanding. Although teachers were able to use these terms with a high degree of agreement, this is almost certainly due, not to the precision of the definitions, but to their having learnt the use of these terms from the examples of objectives that were given. These objectives, as we saw, contained words such as 'translate', 'relate', 'judge', which are not significantly more precise than 'understand', and therefore, since the meaning of the terms of the taxonomy are based on these words, the terms of the taxonomy are not more precise, when applied to the student's behaviour, than words such as 'understand'. (They are, however, more precise when used to classify objectives.)

The purpose of these criticisms was not, however, to show that the taxonomy was of no use, but to demonstrate how it is possible to be led astray by an inadequate conception of understanding. As I mentioned the taxonomy has proved useful to teachers and examiners, and no doubt will continue to do so in the future. This usefulness arises because it enables teachers to classify the objectives and test questions they are using, and thus to extend the types of objectives and questions. It does not arise because the teacher is able
to use more precise words to classify a student's actual behaviour.

In the final chapter I took a closer look at a behavioural approach to learning. Again we saw a commitment to certain methodological directives: behaviour is described in terms of stimulus input and response output, it is built up from simple behaviour, and an inductive approach is used, which in Gagne's case is carried to extremes since he claims that he has simply described learning and has not produced a theory at all. The crucial test of Gagne's work was in the ability to describe the conditions necessary for the learning of language, and we saw that it did not stand up very well to what Chomsky claims are indisputable facts. In complete opposition to Gagne, Chomsky suggests that language learning is independent of conditions and that it starts from rather than concludes with, the child's ability to learn rules. Moreover, Chomsky's criticisms are criticisms of the whole behavioural, or what he now prefers to call, empirical, approach. If these criticisms are valid, then we cannot hope to significantly improve a work such as Gagne's. Instead we must start again from different assumptions.

It might be felt that this summary does no more than justify the criticism that in this thesis I have not directed my attention towards the main problems: I have not said what understanding is, only what it is not; I have not said how we might bring about understanding, nor how to decide when understanding has been brought about. Any teacher reading this thesis for answers to his questions: "How do I get science students to understand mathematics?", "How do I test to see whether they do understand?", "How do I know when students really understand?", is bound to be disappointed, since, not only
have I failed to answer these questions, I have also tried to suggest that they are questions that cannot be answered, at least, not in the way he wants; they are the wrong sorts of questions. Thus, understanding, despite (some might say because of) the last hundred odd pages, seems as vague as ever.

My answers to such points are as follows. In the first place, I confess myself to feel as ignorant as any other teacher when asked how, in general, to get students to understand. There is no simple answer to this question, and that this must be so is, perhaps, more readily appreciated once one realises that understanding is not a state, mental or otherwise, which the teacher is trying to bring about. The attempt to look for some set of general conditions which will ensure understanding, is a misconceived one. The teacher is therefore thrown back on his own experience.

A similar sort of reply must also be given to the question of how, in general, is it possible to ascertain when students have understood. Understanding is not a state, and hence there can be no formal criteria for understanding having been achieved. When a teacher judges whether or not a student understands, he is judging whether or not the student's behaviour, in the particular circumstances, justifies the use of the expression 'He understands'. The teacher is not matching this behaviour against a set of criteria. The ability to judge is not something that is learnt in a formal way, it is learnt through learning a language-game, which in turn is learnt through sharing a form of life with others.

However, when teachers become educational researchers (and here I include myself) they are tempted to ask 'what is understanding?',

-131-
'what conditions must be satisfied so that I know a student under-
stands?' Thus they forget that they have previously judged, on many
occasions, whether or not a student understands. Where before they
were able to judge without formal criteria, they now feel that only
with formal criteria, which, moreover, must exist, can certain judg-
ments be made. Only with the aid of formal criteria can one really
know that a student understands.

This, Wittgenstein would suggest, is a philosophical confusion that
has been brought about by being misled by a misinterpretation of the
form of language (see Wittgenstein 1953, 111 and 112) "A picture
held us captive" (115): the picture of understanding as a state.
The answer Wittgenstein suggests for such an obsession is therapy,
and Wittgenstein's therapeutic method is to assemble a collection
of what he provocatively calls 'reminders'. "The work of the philo-
sopher consists in assembling reminders for a particular purpose."
(127) One purpose of this thesis is to 'remind' teachers how they
use the word 'understanding'.

I hope that the usefulness of these reminders can be seen if we return
to a question that arose in the Prologue: if a student does not under-
stand the mathematics used in his science class, is this because he
has not transferred his understanding from his mathematics class, or
because he did not understand in the first place? Now this question
seems at first sight one to which there must be an answer, and more-
over, one which could, and should, be answered empirically. However
this appearance is, I would suggest, deceptive; we cannot arrive at
a single answer empirically. The reason for this is that the question
implies a false picture of understanding: namely that understanding
is a state which the student achieves, and, once achieved the student's future performance is somehow guaranteed. The picture suggests that we are asking whether the student achieved this 'state' in his mathematics class, but somehow slipped from it between his mathematics and science classes, or whether he never achieved this state at all.

The therapeutic treatment required to remove this misleading picture is to remind ourselves what sorts of criteria we would accept for saying that the student has understood, and I suggest, the criteria we obtain will depend on whom we ask. Suppose we ask the student himself whether he understood. The answer he gives will depend, perhaps, on whether he had a certain 'feeling' of understanding, or whether he had a feeling of puzzlement, or was able to answer all his homework questions, and so on. However, if he said that he had a certain feeling of understanding (and we are probably all familiar with this feeling) it would be wrong to say he was identifying a certain state (say a mental state). This feeling stands in need of outward criteria and, in some circumstances, but not in others, answering all the homework questions provides this criterion. If now we were to ask the mathematics teacher, he would use criteria such as whether the student had been able to do his homework, whether he asked intelligent questions in class, whether he was able to explain why he had answered a question in a certain way, and so on. However, again the satisfaction of these criteria does not point to the achievement of a particular state, nor does it guarantee that the student will be able to answer problems on the same topic set in his science class. Thus, the science teacher, because he is using a different set of criteria again (although not necessarily completely different) may well conclude that the same student does not understand the topic which the mathematics teacher had thought the student did understand.
Yet clearly, this difference is not due to any failure on the part of the student to 'transfer' his understanding.

I would not wish to imply however, that the criteria for understanding are arbitrary, nor that two people will never agree. Agreement, as Wittgenstein suggests, is agreement not only in definitions but also in judgements, it involves agreement not only in opinions but in forms of life. Thus agreement is part of learning a paradigm, part of a commitment to a disciplinary matrix. The disagreement between the mathematician and the scientist is due to a difference in their forms of life, which results in their applying different criteria. Agreement about understanding will, of course, never be absolute, but one would not expect the same order of disagreement between two scientists or two mathematicians as between a scientist and a mathematician. We are perhaps tempted to say that the mathematician and the scientist are talking about different types of understanding, but to do so, again is confusing since it implies that understanding is a state, and that different types of understanding correspond to different states. All we need do is recognise that they use the word 'understanding' in different ways, and accept different criteria for its correct use.

This now looks as if our problems could be solved by the scientist writing down the criteria he would accept for understanding so that the mathematician teaching science students is able to apply the same criteria. This however, grossly over-simplifies the situation, although clearly any solution must be something along these lines, since we must have the science teacher and the mathematics teacher applying the same criteria. Equally clearly, it must be possible for the mathe-
inatics teacher to learn the science teacher's criteria, since the science teacher must have learnt them himself. However, he did not learn them from a list; he learnt them, as Kuhn suggests, by seeing examples, by doing problems with pen and paper, by carrying out laboratory experiments, and so on. In short, he learnt them through the paradigms which guide normal science. It is not that the list of such criteria is infinite, but indefinite. The mathematician cannot be given it, he must learn the criteria in the same way the scientist learnt them.

This discussion illustrates, I hope, why I feel that a philosophical investigation is a necessary precursor to an empirical investigation into some of the problems associated with teaching mathematics to science students. It does not give any answers, although it does suggest the sorts of directions in which to look for answers; it does not arrive at any results, but it suggests why one was not able to obtain the results one thought one wanted. However, although I consider my thesis to be a precursor to, and certainly not a substitute for, an empirical investigation, I have not set out to produce directives for an empirical investigation, and I think it would be inappropriate to give specific proposals for further research, despite the fact that there are at least two areas in which I could do so. For example, I have suggested that affective problems experienced by science students may well be due to the different values expected by science and mathematics teachers, and I have indicated the sorts of values involved. I have also suggested that the concepts in mathematics and science are not identical, because of the different uses of the same words and symbols in the two subjects. Thus, two lines of research might be to investigate further these different
Now clearly, I would not wish to discourage research in these two areas, but I feel that there are important reasons why such research would not be very fruitful. The problems science students have with mathematics arise because mathematics and science are different subjects, they do involve different language-games, and teachers of mathematics and science share different forms of life. One cannot, by investigating the different values held by mathematicians and by scientists, persuade them both to accept the same values; nor can one persuade them both to use mathematical terms in the same way.

The problems will not be solved, overnight as it were, by science teachers handing mathematics teachers a list of values they should adopt, and a list of ways in which they should use mathematical terms. In this sense the problems associated with mathematics for science are not problems that can be solved simply by research; they are a collection of problems, varying from course to course, from institution to institution, showing various similarities and differences with each other.

The above two areas are therefore not, I feel topics for future research, but rather, areas for discussion between the particular mathematics and science teachers involved on a course. What is primarily required is not research into different values and different uses of mathematical terms, (although I do not wish to suggest that such research is of no use) but an exploration by the people involved in teaching, into how their values and use of mathematical terms differ.
It is, of course, possible to make a few tentative suggestions as to how this process might be encouraged, and these could include large scale alterations, ranging from architectural modifications to enable mathematicians and scientists to meet more easily, to administrative changes in departmental and inter-departmental organisation. Or they might involve relatively small scale changes such as team-teaching, weekly seminars, and mathematics and science teachers sitting in on each other's lectures and tutorials. However, the extent to which students' difficulties will be reduced by such changes will inevitably depend, not on the scale of the changes, but on the people involved. No amount of administrative alterations will overcome suspicion or resentment on the part of the mathematics or science teacher who feels threatened by interference with his course. Team-teaching meetings can easily degenerate into horse-trading between mathematicians and scientists of one topic against another. And so on. Nonetheless, the fact that there can be no assured way by which the problems associated with teaching mathematics to science students can be reduced must not be allowed to obscure an important, positive contribution that I hope the thesis has made; mathematics and science teachers must begin to share the same forms of life.
REFERENCES


Bondi, H., Mathematics, the Universities and Social Change, in the Science Teacher 1966

Bruckheimer, M., and Gowar, N. W., Maths Education with a Purpose, in New Scientist, 14th November 1968

Bruner, J., Goodnow, J., and Austin, G., 1956, A Study of Thinking Wiley.


-138-
Eraut, M., Selecting on Evaluation Strategy, talk given at a one-day Seminar on Course Evaluation at University of Surrey, 9th March 1973


Kuhn, T. S., 1970, The Structure of Scientific Revolutions, 2nd Edn (enlarged), University of Chicago

Magee, B., 1973, Popper, Fontana modern masters


Malvern, D. D., The Problem of Transfer between Mathematics and Physics in the Sixth Form and of Minimising the Degree of Mismatch of the Syllabus, Sixth Form Mathematics Curriculum Project Maths - Physics Topic Paper, mp5, 1971


Ryder, Sister M. B., Behavioural Objectives: An Introduction for Physics Teachers in The Physics Teacher, October 1970


Toulmin, S., 1972, Human Understanding, Vol I, Oxford University Press


