THE RELEVANCE OF PHILOSOPHY

TO PSYCHICAL RESEARCH

A Critical Examination of Claims and Methods

by

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ABSTRACT

After a hundred years, psychical research (and experimental parapsychology) are still regarded as pseudosciences. Despite sceptical attacks, however, work continues: the results cannot be lightly dismissed. Evaluation of this work demands philosophical attention, since there are large metaphysical assumptions made, and the experimental results are vulnerable to logical criticism.

The supposed kinds of paranormal phenomena are anatomized, and the current terminology criticized. Broad's basic limiting principles are examined, and the extent to which psychical research might threaten conventional science is considered.

The metaphysical foundations of psychical research are examined. Psychical research takes a dualist view of human beings, although its experiments are behaviouristic. Survival of death is assumed to be a matter susceptible to empirical verification. These assumptions are critically examined.

I argue that ESP neither gives us knowledge, nor is it a form of perception. Although psychical researchers interpret their results in causal terms, this is illegitimate. Backwards causation, therefore, need not be canvassed as an explanation.

As the basis for a scientific discipline, spontaneous cases are inadequate, as they involve underdescription (lack of detail) and overdescription (tendentious
exaggeration). Observational cases, given the absence of theoretical explanations, and the prevalence of fraud, also fail as science. Hume's arguments on testimony, and modern versions of them, are considered.

Experimental parapsychology fails because ESP and PK seem to occur without conscious awareness or significant physiological signs, and in a way that transcends space and time. There is thus no way that science can gain a grip on them. Without a theory and a mechanism, there is nothing but a series of statistical quirks.

The proposed models do not mesh in with the experimental results. Paranormal forces, if they existed, might contaminate all scientific work, and no one could ever know. Taken on its own terms, psychical research is impotent to undertake the task it has set itself.
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John A. Lord

St Antony's Day, 1988
The confusion and barrenness of [para]psychology is not to be explained by calling it a "young science"; its state is not comparable with that of physics, for instance, in its beginnings. ... For in [para]psychology there are experimental methods and conceptual confusion. ... The existence of the experimental method makes us think we have the means of solving the problems that trouble us; though problems and method pass one another by.

PART ONE

INTRODUCTION
At the borders of orthodox scientific thought, there lies a miscellaneous group of theories, topics, and hypotheses which are generally rejected by the majority of scientists. This group, usually labelled "marginal" or "anomalous" sciences, or even (more tendentiously), "pseudosciences", has attracted many vocal proponents and supporters.

There is, however, little that the whole range of these "sciences" has in common, save their rejection by orthodox scientific opinion. In some cases, for instance, orthodox science has been subsequently shown to be mistaken in its failure to accept new ideas. The history of science is replete with examples of such errors. For example, the unwillingness to credit the possibility of meteorites ("stones that fall from the sky", to adopt Thomas Jefferson's dismissive phrase) is frequently referred to as a signal blunder on the part of the scientists of the time. And when Wegener, who had been trained as a meteorologist, put forward his theory of Continental Drift, his work was dismissed by orthodox geologists, who regarded him as an outsider. Time has, however, vindicated Wegener's ideas, which have now become incorporated into the mainstream of geological science. Similarly, Anton Mesmer, who first demonstrated the hypnotic trance state, was attacked by the physicians of his day as a charlatan. His own explanation of what was taking place in his experiments (couched in terms of "animal magnetism") was, it must
be said, of very doubtful value, and probably this inadequacy contributed to the reaction against him. But today, no one seriously doubts the reality of hypnotism, despite the fact that the precise mechanism governing its operation is still not properly understood. In much the same way, acupuncture has been resisted for many years by Western physicians, but has now become cautiously accepted in some quarters; like hypnotism, its mode of operation is still not known, though various possibilities are currently being investigated. What is clear is that the ancient Chinese physiological system that attempted to explain it has been shown to have no basis in reality.

In each of these cases, a serious hypothesis was offered, and, in time, came to be accepted. Some anomalous sciences, however, although they put forward scientific claims, have been shown to be misguided. Phrenology actually took as its foundations certain testable scientific theses (for example, that mental traits and abilities are localized within the brain). Subsequent research has shown this, and its other assumptions, to be false. The science of phrenology has now vanished: all that remains of it is a mildly amusing parlour game. In more recent times, Fliess - with the encouragement of Freud - advanced the hypothesis of biorhythms. Further investigation has, however, failed to furnish any convincing evidence that Fliess was right, and biorhythms seem unlikely to survive.

These cases are really quite different from some others commonly met with in the literature of marginal science: there are certain topics which, though they cannot lay claim to have any serious scientific content, would have a considerable impact on the
relevant areas of science if they could be shown to have some factual basis. If there are alien civilizations despatching flying saucers to the Earth, then biology, astronomy, and astronautics will undergo important changes. Similarly, if the Loch Ness monster or the abominable snowman could be located, then zoology and palaeontology would be presented with exciting new problems to tackle. Such topics as these are not essentially inimical to the scientific enterprise - at their most serious, they seek to expand it.

There are, though, certain other marginal sciences that fly so completely in the face of orthodoxy that acceptance of them would require a total revision of the relevant areas of science, and indeed of our view of the world. In this connexion, the names of Velikovsky and von Däniken are among the most recent to attract popular attention. Biology was able, earlier this century, to come to terms with the fact that the coelacanth was not, as everyone had supposed, extinct: if the Loch Ness monster could be found, it might well present a similar challenge, but the challenge could be met. Now if Velikovsky were correct, then the situation would be very different: physics, astronomy, and anthropology as we know them would effectively cease to exist. There could be no question of assimilating Velikovsky's claims within the present framework of those sciences. If Velikovsky were right, then we should have to demolish them, and carry out the reconstruction on quite different foundations.

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Of all the marginal sciences, though, the one which is most rigorously documented must be psychical research.
In spite of its imposing claims to scientific and intellectual respectability, however, psychical research is still, after a hundred years of systematic work, the focus of heated controversy. Nonetheless, it must be said that psychical research stands in rather greater repute than the majority of pseudosciences. Many books on pseudoscientific topics are shoddily researched and trashily written, and are aimed at a totally uncritical market. The claims that they make are so patently ludicrous that only a fool could delight in them - a point I have made elsewhere (Lord, 1980). After several years' perusal of many books on pseudoscientific topics, I can concur with the judgement of Antony Flew: most of them are (I use his own term) "bunkum"; they are either self-evidently so, or become so when we apply a modicum of critical thinking and investigative flair. Psychical research, and in particular, its experimental wing, parapsychology, is, however, "a horse of quite another color" (Grim, 1982, p.179). That is to say, the quantity and (to an extent) the quality of the evidence offered are just too great for us to dismiss it out of hand whilst still claiming to retain "a good academic conscience."

The issues raised by psychical research are wide-ranging, and, it is sometimes claimed, of quite literally transcendent significance. Psychical research, once accepted, will - we are told - revolutionize our view of humankind, and overturn orthodox science. Important concepts, such as "mind", "knowledge", and "cause" will require to be redefined to accommodate these findings; the profound question of the survival of death would be subject to empirical investigation; people could be shown to have the capacity to acquire information through no known
channels of sense, and to move remote objects by "will-
power".

These are, as I have said, large claims. But can they
be sustained? Psychical research is still not generally
accepted in scientific circles: its credentials,
despite the considerable body of work that has been
carried out, are still not established. Can psychical
research lay claim to scientific status, or are its
findings due to error, artefact, or even fraud?
Clarification of the central issues in so controversial
an area is a task of some importance, for if only some
of the claims that are made can be shown to have a
basis in fact, then their potential importance could
hardly be exaggerated. If, however, psychical research
is flawed, then it should be criticized ruthlessly:
there is no virtue in devoting resources to an
investigation that will continue to yield only paradox
and controversy. In any case, since psychical research
lays claim to scientific status, it cannot complain if
it is judged by the same rigorous standards that more
orthodox sciences apply to themselves.

The question at issue here is: can psychical research —
and, more specifically, experimental parapsychology —
make out a claim to enjoy scientific status? This
question is not simply a straightforward empirical one,
for two reasons. First, although some of the issues do
involve scientific matters, they also pose
metascientific ones (concerning the philosophy of
science) and these demand philosophical treatment.
Secondly, psychical research comprehends problems which
have philosophical implications, and as such are not to
be answered without a measure of philosophical
consideration.
It is my intention in this work to provide an answer to this central question. That answer will be "No". My approach to psychical research will be at the conceptual rather than the scientific level, combining critical readings of the claims made by its proponents with a philosophical consideration of their merits. I should say that it is not my intention here to construct and defend one particular philosophical theory, but to draw such weapons from the philosophical armory as may be necessary to mount an attack upon the foundations of psychical research.

The philosophical literature on psychical research is, in terms of quantity, quite small. It contains two separate strands, which I shall refer to as "philosophical parapsychology" and "the philosophy of parapsychology". My use of these terms is intended as analogous to "philosophical psychology" and "the philosophy of psychology". The former term, now more usually called "the philosophy of mind", is an attempt to elucidate our concepts of the mental, a project which need not draw upon empirical scientific research (indeed, it sometimes contradicts it). The latter is a specialized branch of the philosophy of science which investigates the theories and explanations put forward within the discipline of psychology.

The bulk of the philosophical literature on psychical research falls squarely under the heading of philosophical parapsychology; the writers concerned have addressed themselves to the philosophical difficulties raised by the findings of the psychical researchers, and, generally, have sought to show how certain important concepts (such as "cause" or "mind") might have to be redefined so as to accommodate them. Since, on the face of it, these findings conflict
seriously with our ordinary expectations, considerable conceptual reconstruction has been proffered as a means of resolving the situation. For many of these philosophers (for example, Broad, Price, and Mundle) psychical research has been a major interest for a substantial part of their careers, and they address the topic with a measure of conviction: one is left in no doubt that, to them, the questions posed are of real importance. Many other philosophers who have written on this subject, however, give the impression that they are uncommitted to any belief in the reality of psychical phenomena. What they have found in psychical research is the opportunity to revivify certain long-standing philosophical problems (such as the nature of the relationship between mind and body). Psychical research furnishes such writers with useful puzzle cases around which an argument can be constructed.

In the literature of philosophical parapsychology, we see stated the case for (to use Broad's famous phrase) the relevance of psychical research to philosophy. The philosophy of parapsychology, on the other hand, seeks to demonstrate the inevitability of the relevance of philosophy to psychical research. Against a background of the philosophy of science, it seeks to examine the validity of the theories, experiments, and proofs which are offered by psychical research, usually paying particular attention to the experimental work of the parapsychologists. The literature that falls under this heading is comparatively sparse, though it does contain a number of provocative contributions, most notably from Antony Flew.

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The present work will mainly fall within the category of the philosophy of parapsychology, though I shall consider certain important questions relating to basic conceptual issues. As I have already indicated, I shall reach sceptical conclusions.

After a preliminary outline of the subject matter of psychical research, the phenomena which it investigates, and the terminology which it employs, I shall discuss the question of paranormality: what is it for a phenomenon to be described as paranormal? In Part Two, I shall go on to consider specific philosophical problems raised by psychical research. The first, and perhaps most important, of these is in the area of the philosophy of mind. Psychical research makes assumptions about the nature of mind which are vulnerable to criticism. Although the typical experiments in card-guessing and dice-influencing are essentially behaviouristic in their methods, their underlying presuppositions are derived from a dualist view of human nature. The question of the survival of death, which psychical research tends to assume is subject to straightforward empirical investigation, is also permeated by Platonic-Cartesian dualism. These assumptions are, I believe, fundamentally mistaken.

In relation to cases of so-called extra-sensory perception, I shall next consider certain epistemological issues. Extra-sensory perception is not to be considered, I believe, as a form of either sensation or perception, nor can we claim to acquire knowledge by means of its alleged operation. Causation is another area which has attracted a deal of attention, particularly in relation to the so-called idea of backwards causation. Psychical research, in discussing the results of its experiments, not
infrequently helps itself to causal terminology. Can this be justified? Is there, in fact, any serious reason to assume that a causal factor is at work in such cases?

Having, in Part Two, examined and criticized the foundations of psychical research, I shall in Part Three go on to answer the question I posed earlier in this chapter: can psychical research lay claim to scientific status? As there are, by common consent, three distinct types of case studied by psychical research, I shall take each of these in turn, and show that they are all, in their different ways, unsatisfactory as the bases for a science. My discussion will pay particularly close attention to the experimental cases, since these tend, not surprisingly, to be the ones that have generated most of the excitement in scientific quarters. I shall argue, in Part Four, that experimental parapsychology suffers from a defect that is fatal to its scientific pretensions: if we accept the claims that are entered for the phenomena by the psychical researchers, then it is possible to show that psychical research itself is impotent to investigate them scientifically. The phenomena, once admitted on the psychical researchers' own terms, generate insidious difficulties.

This work will, then, present a negative view of psychical research. Whenever psychical research has put forward its claims, there have always been sceptics prepared to rebut them. If the psychical researchers have striven to demonstrate, and the philosophers to explain, then the sceptics have busily explained away the alleged results, attributing them to fraud, incompetence, statistical artefact, and so on. The sceptic's usual response to the claims of psychical
research is to argue that the phenomena that are reported cannot occur, or did not occur, or - at the very least - did not occur quite as described.

But this form of scepticism suffers from one great demerit: it fails to convince the psychical researchers themselves. When the sceptics claim, as they commonly do, that the experiments have not been successfully repeated (as does Flew, in Grim, 1982), then the psychical researchers will point to what are considered (by some) to be replications of Schmeidler's sheep/goat effect (1958). Allegations of fraud (for example, by Hansel, 1966, 1980) will be countered by an appeal to personal integrity (as in Soal's reply to G.R. Price, in Ludwig, 1978), and expressions of incredulity that so many researchers could be involved in a series of conspiracies (Eysenck, 1958). It will, moreover, be said that orthodox science is itself by no means untarnished by bogus claims (see Broad and Wade, 1982). Even the invocation of Laplace's famous dictum, that "the more extraordinary the claim, the greater must be the weight of evidence in its favour" (as in Iremonger, 1957) will produce the reply that the level of statistical significance called for in parapsychological experiment is substantially higher than that commonly used in, say, psychology or the biological sciences (see Tyrrell, 1947).

No, this form of scepticism has left the psychical researchers untouched: something more radical is called for, and it is this desideratum that I shall attempt to supply. I shall avoid making the usual assumptions of the sceptics, which can, in the heat of debate, easily degenerate into the bald assertion that psychical researchers are either knaves or fools (if paranormal phenomena do not occur, then those who purport to
demonstrate them are either falsifiers of evidence or incompetent dupes). Neither shall I take the common sceptical path of worrying over experimental details, drawing attention along the way to potential methodological pitfalls - a line of criticism seen at its most trenchant in the works of Hansel. The critic, if his object is to persuade the psychical researcher, takes a wrong turning at precisely this point, and he need not be surprised at the vigorous reception he is accorded. In any case, this path is not one that I am qualified to travel down: it properly belongs within the domains of the experimental psychologist (for example, Hyman or Hansel) and of the professional magician (for example, Randi or Christopher). While I shall bear their strictures in mind, I shall not rely on them in the framing of my central arguments.

In answering the question concerning the scientific status of psychical research, I need not, as sceptics have tended to do, deny that paranormal phenomena do, or a fortiori, can occur. In order to demonstrate the impotence of psychical research, I shall accept, for the sake of argument, the reports that are given of allegedly paranormal phenomena. That is to say that I shall assume that witnesses gave their testimony in good faith, and that experiments were properly conducted. What I shall doubt, and I shall doubt them with a Cartesian tenacity, are the fundamental assumptions that underpin such reports, and the tendentious interpretations that are placed upon them.
CHAPTER TWO

THE ANATOMY OF PSYCHICAL RESEARCH

It is important, at the outset, to clarify the kinds of phenomena that psychical research takes to be its proper study. At first sight, these seem to be a very miscellaneous assortment of oddities. But there are, at a deeper level, similarities that tie them together. As well as sketching an outline of psychical research and its subject matter, I shall, in this chapter, discuss some of the terminological problems that are encountered in framing an account of the phenomena at issue. While it is acknowledged by some writers that the vocabulary of the paranormal is confusing, misguided, and even tendentious, the standard available alternative, the "neutral" terminology of Thouless and Wiesner (1947), is not without difficulties.

Defects in terminology create special problems in understanding what might be going on in parapsychological experiments, and these have led some psychical researchers to propose that cases of what are usually claimed to be kinds of either extra-sensory perception (ESP) or psychokinesis (PK) might be reduced to one or two basic types. This idea has something to commend it; however, the effecting of the reduction is not so easy as it might sound, for we do not have any means of ensuring that, having carried it out, we have got it right. The root of the problem is that there are no rules - or even rough guidelines - for establishing
how we can differentiate between the phenomena under consideration.

What, then, is psychical research? The Society for Psychical Research (SPR) laid down the following definition in its Constitution (printed at the commencement of Volume one of the Society's Proceedings): "an organized and systematic attempt to investigate that large group of debatable phenomena designated by such terms as mesmeric, psychical, and Spiritualistic". This "large group" is subsequently glossed as being "an important body of remarkable phenomena which are prima facie inexplicable on any generally recognized hypothesis". This definition is widely accepted, though it should be said that in North America, the term "parapsychology" is usually preferred to "psychical research", possibly because of its scientific resonances. In this work, I shall conform to the usual British usage, and take parapsychology to be a sub-discipline of psychical research, in which statistical techniques are employed by scientifically-trained persons who seek to investigate ESP and PK.

The whole range of topics which psychical research undertakes to study seems, as I have said, to be a disparate collection. We have phenomena such as visible ghosts, invisible poltergeists, seance room effects, out of the body experiences, metal bending, extrasensory perception, and psychokinesis, to take some of the more noteworthy examples, but the list could be extended almost indefinitely. The most systematic research has been carried out, however, in the areas of ESP and PK. The whole range of phenomena which psychical research treats of may, it is generally accepted, be denominated under three separate headings:

1. Spontaneous (or sporadic, or anecdotal) cases
This category comprises "one-off" phenomena which occur unbidden and unexpected, and which rely upon personal testimony for their evidential support. Such phenomena are sometimes reported as being spectacular and are often endowed with a transcendent significance by those who witness them. Ghosts, phantasms of the living, out of the body experiences, and sporadic poltergeist cases all come under this heading.

(2) Observational (or evidential) cases
Here, phenomena are bidden to occur (though they may not always "come when you do call them"), or happen with sufficient regularity to allow a programme of investigation and recording to be set in train. Typical examples would be seance room phenomena (involving mediums), levitation, metal bending, hypnotic regression, and recurrent poltergeist manifestations. Such phenomena may not occur precisely as expected, but they do afford the psychical researcher an opportunity to anticipate them with appropriate preparations.

(3) Experimental cases
This group consists of the laboratory work, employing cards, dice, or other randomizing devices, together with a statistical analysis to arrive at a final result. Such work I take to be paradigmatic of parapsychology. It constitutes the most systematic, thoroughgoing attempt by scientifically trained experimenters to discover the critical preconditions for the occurrence of ESP and PK, and ultimately, it is proposed, their *modus operandi*. In its sheer thoroughness, its insistence on a "clean" laboratory environment, and its application of sound statistical techniques, this research compels close attention. The use of statistical methods was first proposed by Francis Bacon in Century X of his *Sylva Sylvarum*, a posthumously published collection of the vulgar errors of his time. The systematic application of these
methods was pioneered by Barrett and his colleagues in the formative years of the SPR, and further developed by Rhine in the 1930s. Compared with the spontaneous cases, however, such experimental demonstrations are tediously unspectacular, relying as they do upon the application of an appropriate statistical formula to establish the paranormality of an occurrence after it has taken place. Despite this difficulty, such experimental studies present the sceptic with a serious case to answer, in virtue of the rigorous techniques that have, it is claimed, been employed.

When we consider the experimental work, our attention is turned away from such things as ghost stories and spoon-bending to questions of card-guessing and dice-influencing. Thus our focus changes from observed, gross, macrolevel events to inferred microlevel ones. This change may seem to make psychical research more secure from a scientific point of view, but the gain can be shown to be insubstantial, since the interpretation of the data in such inferred cases is not so unproblematic as most parapsychologists assume.

Although psychical research tends to assume that all the three types of case are, ultimately, of a piece, and that the distinctions between them are to be drawn solely in terms of the quality of the evidence, there are no rules available to show us that this assumption is correct. It may even be that we are dealing with three entirely different kinds of phenomena, each with its own essential structure. To take an example, it is sometimes asserted that spontaneous poltergeist cases are continuous with the recurrent ones (which fall into the category of observational cases), and further, that both groups are closely tied in with experimental PK phenomena. Indeed, some parapsychologists follow Roll
in using the abbreviation RSPK (recurrent spontaneous psychokinesis) as a designation for the observational poltergeist phenomena. But this assumption may be entirely unwarranted. Certainly we can say that the quality of the evidence in the three types of case varies considerably. That there is something linking all three of them together (apart from their metaphysical implications) is debatable.

In settling such questions, we are hampered by the inadequate and sometimes tendentious terminology employed in psychical research. In some instances, terms have been taken from the popular vocabulary, which originally borrowed them from folklore, mythology, and similar areas. Thus we encounter expressions such as "clairvoyance", "ghosts", or "reincarnation", and these, or similar, words are produced whenever an explanation of some supposedly paranormal phenomenon is called for. But such conveniently available terms carry with them a great deal of conceptual baggage which can be too easily overlooked. We may find ourselves saying, for example, in an unreflecting moment, that we understand what is meant by "reincarnation". However, such an assumption of understanding is often based on an incomplete picture. Thus, if we say that reincarnation is the process that takes place when a mind (or soul, or spirit) informs a new body after the death of the one it had previously occupied, our definition would at least have etymological warrant, and might help someone to elucidate, say, the scriptures of Buddhism. But when we begin to fill in the details of this picture, and to justify each of those details, we shall encounter problems: does the dualist view of the universe actually make sense? If there are two radically different kinds of stuff, then how do they interact?
Why is one kind immortal, and the other highly perishable? By what mechanism does reincarnation work? When we ask these, and other, questions, it quickly becomes apparent that our claim to understand reincarnation was overambitious. In the absence of an adequate definition of what reincarnation is and how it works, the design of a scientific test of "the reincarnation hypothesis" is a fruitless exercise, since, until we have a clear idea of what we seek to demonstrate, we shall be in no position to judge success or failure.

As well as employing several existing words, psychical research has introduced a number of neologisms. Some of these are decidedly tendentious, suggesting that the phenomena operate in certain ways. Thus we have "precognition", implying that ESP is a species of cognition. "Extra-sensory perception" itself disposes us to think of these phenomena as operating in (roughly) the same way as normal perception. "Telepathy" and "teleaesthesia" (the latter was a term coined by Myers) carry more than a suggestion that some kind of feeling or sensation accompanies such occurrences. ESP is sometimes referred to (albeit popularly) as a "sixth sense". Leaving aside the captious objection that modern psychology allows for several more sensory modalities than the traditional five, this usage is unhelpful. Are we to accept that manifestations of ESP come within the province of a single extra modality, or that for each of the five traditional modalities, there is a paranormal dimension, thus allowing for paranormal seeing, paranormal hearing, even paranormal tasting? (This last suggestion is not so bizarre as it might sound, for in its early years, the SPR conducted a series of experiments in which the subjects were invited to guess
the qualities of substances - such as vinegar and sugar - which the experimenters themselves were tasting).

As an alternative to such tendentious terminology, a "neutral" nomenclature has been proposed by Thouless and Wiesner (1947), and is now widely adopted for the phenomena studied by parapsychology. These phenomena are collectively known as "psi", the extra-sensory perception varieties being termed "psi gamma", and psychokinesis "psi kappa". The possibility of devising a neutral nomenclature is, however, fraught with difficulties. First, a supposedly neutral terminology is open to the objection that scientific observation is a theory-laden activity. That is, in undertaking scientific work, we bring to the task a measure of informed presupposition. Secondly, it is generally recognized that the phenomena with which parapsychology deals are inherently problematic, to the extent that they can seem to deny rational understanding. In such circumstances, the adoption of a neutral terminology may actually be less than illuminating.

The desire for neutrality ties in with the demand for "open-mindedness". Sceptics are often called upon to assess the reported phenomena with an open mind; were they to do this, the psychical researchers urge, the controversy would largely be ended. In this, they are, I think, mistaken. For while I might, for example, keep an open mind on a straightforward question, such as the identity of the murderer of Julia Wallace, in paranormal contexts, such a prescription cannot be met. When faced with such astounding phenomena as are alleged to take place in poltergeist cases or seance room manifestations, there is no clear-cut conceptual framework into which they can be fitted. This problem raises doubts as to the possibility of training
researchers to observe paranormal phenomena, especially in the spontaneous and observational cases.

The difficulties that psychical research has to face are sometimes attributed to its being a "young science": its problems in both establishing formal principles and gaining recognition as a genuine branch or sub-branch of the scientific enterprise are put down to immaturity. But is immaturity itself the only cause of these problems? Psychical research has been in existence for almost as long as experimental psychology, which is often itself described as a "young science", but it has to be said that the strides it has taken have far outstripped anything that psychical research can offer. The first psychology laboratory was established in 1881, by Wundt, in Leipzig; the Society for Psychical Research was formed in the following year, and though a large part of its early work was concerned with the collection of anecdotal cases and with the testing of mediums, it set on foot from the very start a number of experimental studies that utilize the same statistical approach that was to be adopted by Rhine and his colleagues at Duke University nearly fifty years later. It is true that these early trials lack the sophistication and "laboratory atmosphere" of much subsequent work; reading the numerous reports in the earliest volumes of the Society's Proceedings, one is often struck by the informality of the design of these experiments, and the rather ingenuous assumptions that underlie them, but it cannot be denied that they are, in substance, of a piece with the later, more systematic work. Part of the reason for the controversy that surrounds this later work is that it relies upon the same metaphysical assumptions that were made by the founding fathers of psychical research, and still runs up against the
terminological difficulties that I have been discussing.

How, then, might we characterize the phenomena at issue? As regards the spontaneous and observational cases, there is no generally agreed typology that we can use to cover them. But, since many psychical researchers lean towards the view that such cases might eventually be subsumed under the headings that are used in the experimental studies (as seen in the case of RSPK), what sort of typology might be appropriate to categorize the experiments? The basic traditional schema (which, it must be said, is often admitted to be inadequate) is as follows:

PSI GAMMA
(1) Clairvoyant ESP
(2) Telepathic ESP
(3) Precognition

PSI KAPPA
(4) Psychokinesis

In type (1), the subject is said to acquire information (about, say, the present target card in the pack) directly from the card itself, without any intervention on the part of the person handling the cards. In type (2), the information is obtained from the person controlling the cards rather from the cards themselves. Type (3) involves the acquisition of information about the identity of the next (or sometimes the next but one or two) card(s) in the pack. Type (4) is completely different (or so it is assumed), in that it involves the transfer of energy to a physical object (say, a die) that is thus caused to land with a chosen face upwards.
It should be noted at the outset that there is a category mistake here, since precognition implies that there is a temporal dimension involved in ESP, which can apply in both clairvoyant and telepathic cases, and also, mutatis mutandis, presumably to PK. Precognition is, then, not a third type of psi gamma, but a special kind of either clairvoyance or telepathy. It is also posited that as well as forward temporal displacement in psi phenomena, there can occur backwards displacement (so that, as well as precognition, we might also have retrocognition). If we are to allow temporal displacements in the ESP cases, we should also grant that they might occur in PK. This means that we should expand our original four term typology to nine, using the notation proposed by Flew (1953): S = simultaneous (for present), P = plus (for future), and M = minus (for past). The new typology would thus read:

(1) M clairvoyance
(2) S clairvoyance
(3) P clairvoyance
(4) M telepathy
(5) S telepathy
(6) P telepathy
(7) M psychokinesis
(8) S psychokinesis
(9) P psychokinesis

Even this version is in need of further refinement, however, since we have not even begun to specify the nature of the transaction that takes place in such cases. It is usually assumed that the "commodity" involved in clairvoyance and telepathy is information, and that in psychokinesis is energy. Thus, in a card-guessing experiment, the person who guesses the cards is usually said to be obtaining information about the target card either directly from the card itself
(in the clairvoyance cases) or from the experimenter who is concentrating on the card (in telepathy). In PK experiments (and we may take the dice-throwing work as a convenient paradigm here), the subject is taken to be influencing the fall of the dice by imparting energy to them.

This seemingly innocuous reading is actually highly suspect, and will call for further examination. For the moment, we should note that, if we accept that information or energy are somehow being transferred, then this could be achieved in two distinct ways. Wheatley (Krippner, 1977, p.162) points out that "information transfer" could be understood as either "perception" or as "communication". These terms are, perhaps, given the context, less than transparent. I should prefer to speak of "yielding and extracting" (for "perception") and "transmitting and receiving" (for "communication"), since these paired words give some idea of what each side of the partnership in a parapsychology experiment might be supposed to be doing, and can also be used to cover the "energy" cases of psychokinesis.

In the first mode, energy or information can be pictured as being passively given up to a person or object: a card might yield information as to its suit to someone actively engaged in guessing it, or a die might actively extract energy from someone passively awaiting its next cast. In the second, the person guessing the cards would be interpreted as passively receiving the information from the card (which was actively "transmitting"), and the die would be taken to have passively received an "energy charge" that had been actively transmitted to it, resulting in its falling with the desired face uppermost. In each case,
then, there is no side to the experimental equation that we can unequivocally describe as being either passive or active; we may read the situation either way and still have no grounds for judging what is actually going on, or indeed, any means of finding out.

The problem of establishing which party to a paranormal transaction is active and which is passive creates intractable problems for parapsychology. It may be that there simply is no way of arriving at a decision, for we must remember that these parties are of widely differing metaphysical constitutions. We have living organisms (not only human beings, but horses, cats, eggs, plants, and even mould cultures). We also have inanimate objects, such as cards, dice, computers, radioactive sources, and tape recorders. Given such disparity in kinds, we need not expect the task of framing a solution to the problem to be a simple one. The easy assumption that there are readily distinguishable agents and patients (as is made by Broad, 1962, p.25) is a serious oversimplification.

An experiment may be presented to us in one particular light, but be subject to alternative readings, each of which can claim to have a plausibility equal to that of its rivals. For instance, if we consider the classic card-guessing experiments, in which the subject is asked to guess the identity of the cards in succession, without anyone else looking at them, the psychical researchers are apt to present this (and we are asked to follow) as being a case of simultaneous clairvoyance. No one else viewed the cards during the experiment, so we feel led to say that telepathy could not have been involved. But there is nothing to license this characterization, save intellectual laziness. For it could no less (and no more) plausibly be proposed
that the experiment should be interpreted as one of precognitive telepathy. After the guessing had been completed, the experimenter picked up the deck of target cards and made out a list of them in their proper order, so that he would be able to compare his list with the subject's guesses. What the subject was doing was not simultaneously obtaining information from the targets, but gaining access to information that would, at a future time, exist within the experimenter's mind after the experiment had been concluded. There is, however, no need to stop there: for it could, equally, be taken to demonstrate precognitive clairvoyance, whereby the subject had access, not to the contents of the experimenter's mind, but to the list of targets that he would write up. We cannot even rule out the possibility that psi kappa, rather than psi gamma, is at work. The subject may be using the "psychic shuffle" (as Rhine has called it) to influence (retroactively) the order of the cards he was required to guess.

There is much more that needs to be said on the whole question of the differentiation of phenomena, and the matter will be further explored in Part Three. The difficulties involved have forced psychical researchers to ask whether some types of psi might not be more basic than others, and to attempt some kind of reduction of the phenomena to one or two basic types. But until we are in a position to untangle the various competitors, there is no safe criterion we could use for effecting the reduction. It would have to be seen as a purely theoretical exercise, designed to remove some of the clutter in the way of our understanding. But until adequately testable theories of the *modus operandi* of psi are forthcoming (and there are, it must be said, no indications that they will be, or even of
what they might be like), we could have no idea if the clutter we were removing was gold or garbage.
If, as I have suggested, the terminology used by psychical researchers in specific instances is problematic, even misleading, is it possible to provide a more general characterization of these phenomena: one that is neither unwarrantedly tendentious nor question-begging, and which might prove more illuminating than the rather bland statement of the SPR? The phenomena have, in the literature, been variously referred to as "preternatural", "supernatural", "anomalous", and "paranormal", with "abnormal" making an occasional appearance. There are important distinctions that can, and should, be drawn between these terms.

"Preternatural" and "supernatural" are words that should, initially at least, be avoided, since they both suggest that the phenomena in question lie outside the range of science's grasp, and so their use, if taken strictly, would rule out the possibility of a scientific investigation from the very beginning (although it should be noted that Scriven has preferred the term "supernatural" consistently in his writings on psychical research; this is idiosyncratic, and his usage of it appears to be interchangeable with other writers' use of the term "paranormal"). It scarcely needs to be said that the sceptics' task would be made very much easier if they could, simply on the basis of linguistic choice, rule out the possibility that psychical research might ever achieve scientific conclusions.
The word "anomalous" offers suggestive possibilities, but we should note that its use requires some care. Its strict meaning is: "having no connexion with (scientific) laws": thus an anomalous phenomenon is one that science fails to capture in its nomological net. However, there is also the looser, everyday sense of "anomalous" that conveys the idea of incompatibility or oddity. So if we are told that "Smith's experiment produced anomalous results", we do not interpret this as meaning that Smith had discovered some important new information that lay beyond current scientific understanding, for which he might be expecting to win a Nobel prize: on the contrary, we take it to indicate that his results failed to make sense, and that his experimental design or procedures should be called into question.

The terms "paranormal" and "abnormal" have proved to be the ones on which attention has been focused, since if psychical research is to establish its claim to scientific status, it might legitimately seek to establish what might be called "the laws of paranormality": "laws of anomaly", on the other hand, is a self-contradictory expression. Abnormality, taken alone, is of course lacking in particularly paranormal interest, since science often reports abnormal occurrences, without having recourse to the terminology of psychical research to describe them. The seminal contributions to the debate on what constitutes paranormality are those of Broad (1953, 1962).

Broad proposes that there are "basic limiting principles" (BLPs) which form the framework of "all our practical activities and our scientific theories" (1953, p.7). These BLPs are accepted without question by "practically everyone who has been brought up within
or under the influence of Western industrial societies". In fact, though, the examples of BLPs which Broad offers (and which he admits are not an exclusive list) are stated in rather austere technical language, and it is extremely doubtful that "practically everyone", before Broad drew them up, could be said to have "accepted them without question". Some of the BLPs, he asserts, "seem to be self-evident", and the others are so "overwhelmingly supported by all the empirical facts" that we do not dispute them. When an event occurs that seems prima facie to conflict with one or more of the BLPs, then such an event is defined by Broad as being ostensibly paranormal.

Some of the BLPs certainly do seem to be self-evident, as, for example, "it is self-evidently impossible that an event should begin to have any effects before it has happened", (though we should notice that some philosophers would disagree). But not all of the BLPs are so transparent. For example, in his enumeration of the BLPs concerning the nature of mind, Broad sketches a dualist account of the person. This seems neither self-evident, nor to have overwhelming empirical support. Moreover, it is not improbable that many Westerners today would opt (possibly uncritically) for a materialist monist view.

Broad also offers, as a foil, a definition of an "abnormal" phenomenon: such a phenomenon will apparently conflict "with a well-established law of nature, but not with any basic limiting principle" (1962, p.4). This is further qualified by saying that some abnormal phenomena do not conflict with a law of nature, but are explicable in terms of such laws and "certain special conditions which are seldom fulfilled in the ordinary course of events". In this connexion,
he cites the case of a baby's being born with two heads, but this scarcely seems to conflict with a law of nature, unless Broad is assuming that such laws are, ultimately, mere generalizations from common experience. This does in fact appear to be Broad's intention, for he notes that "the occurrence of an abnormal phenomenon shows ... that some accepted law of nature does not hold without exception": in other words, laws of nature are to be seen as essentially probabilistic.

But Broad's account is not without its problems. First, his introduction of the idea of laws of nature is subject to a critically important ambiguity. When he says some law of nature does not hold without exception, is he taking the uncontroversial position that the law itself lacks generality: that it has been too tightly drawn for it to include certain rare but not impossible contingencies; or is he - much more radically - suggesting that laws of nature might be subject to momentary suspension, thus allowing not only for normal events, with odd abnormalities, but also for occasional "paranormalities"? This question goes to the heart of the nature of the paranormal and of psychical research. We might imagine, on the one hand, that paranormal phenomena are law-governed, and display regularities which we are, at present, incapable of observing, but which we could, and one day might, discover. But on the other hand, we might just as easily imagine that there are no paranormal laws at all: the paranormal is what we see breaking through when a law of nature (or what we at present assume to be such a law) is subject to temporary suspension.

Secondly, Broad does not make it clear exactly what he takes to be a law of nature; he also speaks of
"scientific theories", and of "the principles of physics", but what connexion holds between these three terms is not discussed. The matter of the relationship between BLPs and scientific theory is a difficult one, and Braude, in the course of a lengthy discussion (1979, p.247-256) asks whether the BLPs are presupposed or implied by scientific theory. Broad, of course, asserts that the BLPs lie at a deeper level than scientific statements: they underpin our fundamental understanding of the world. If, on the other hand, they are merely implied by scientific theory, then their interest is considerably diminished. Braude argues that, if Broad's claim for the BLPs is intelligible, it is almost certainly false. However, the question is susceptible to a more radical solution: far from Broad's principles being "self-evident" or "overwhelmingly supported" by empirical observations (a position which, it should be noted, invokes both a priori and a posteriori knowledge), Broad is taking the alleged paranormal phenomena as his starting point. Although he purports to define the proper study of psychical research by reference to his BLPs, which he considers to be fundamental, he has actually worked in reverse, taking the findings of psychical research as given, and drawing up his limiting principles purposely to suit them. These principles need not be taken as being basic to our conceptual scheme: on the contrary, they have been constructed as an ex post facto response to the claims of psychical research.

Thirdly, Broad does not examine an important question: can basic limiting principles actually change? As Western industrial societies develop, surely their views on many important issues must change to keep pace. Modern technological developments have seriously challenged many standard assumptions, which, although
they do not appear in Broad's shortlist of BLPs, would have been regarded in his lifetime as being basic by the majority of those "brought up within or under the influence of" such societies. We only have to consider the impact of new discoveries in computer technology, embryo research, and life-support machines to realize that traditional ideas about intelligence, life, and death have undergone considerable changes in the last decade. These topics are so basic to our world view that we find the idea of change to be extremely uncomfortable, but we cannot ignore the challenge. So is Broad prepared to allow that his own list of BLPs might be subject to change? Presumably he is, since his concern is to show that psychical research is important, and relevant to philosophy. For if paranormal phenomena occur, then, in order to describe and comprehend them, Western societies will have to cast aside their basic limiting principles, (no doubt to replace them with some others which are compatible with psi). If, however, the BLPs are really to be interpreted as fundamental to our world view, and at a more primitive level than scientific theory, then we shall be forced into the position of saying that psychical research is doomed to remain forever outside the grasp of science. Broad seems, in introducing his BLPs, to be trying to have his cake and eat it: he wishes to plead for the importance of the paranormal to philosophy, while at the same time setting it beyond the reach of investigation.

Fourthly, the notion of a BLP is just too loosely drawn. It is not stated how many principles there might be (Broad, it will be remembered, admits that his list is not exhaustive), and, as Braude rightly notes, we are not offered any general characterization of a BLP: there is no explanation of what "in general, a
phenomenon must conflict with in order to conflict with a BLP" (1979, p. 251). As I have already said, there is a tacit assumption made in psychical research that an "open mind" should be a precondition for interpreting the evidence: if we were to examine this evidence with an unprejudiced eye - with an open mind - then we should find it convincing. Now if, as Broad claims, the BLPs underlie our conceptual scheme, and the phenomena which we asked to consider conflict with, even undercut, that scheme, then we shall have met the prescription of open-mindedness, but at the expense of having no conceptual scheme left which is adequate to the task of assessing the phenomena.

Broad demands that any psychical researcher must ask certain questions about an ostensibly paranormal event: "(1) Did it really happen? Has it been accurately observed and correctly described? (2) Supposing that it did really happen and has been accurately observed and correctly described, does it really conflict with any of the basic limiting principles?" (1953, p. 7). But since Broad explicitly requires an "ostensibly paranormal event" to be one that prima facie conflicts with one or more of the BLPs, his demand will leave us with no means of judging it, for the in the absence of any conceptual scheme, the notion of "description" makes no sense, and there can be no application of "correctness" here either, for we have no yardstick by which to measure it. The introduction of BLPs is thus self-defeating.

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Other proposed definitions of paranormality have usually been couched in terms of the incompatibility of the phenomena with scientific theory, coupling this
with a claim that such incompatibility is, in a significant sense, problematic. Scriven, for example, demands that if a phenomenon is to count as "supernatural" (his preferred term), then we have to show that it "was not explicable in terms of the entities in the pantheon of contemporary physics and psychology" (Thakur, 1976, p. 185); moreover, the phenomena are marked out by "their exceptional idiosyncracy or generic differences from the other phenomena of nature" (p. 181). Braude stipulates that an event is paranormal if it is inexplicable in terms of current scientific theory, cannot be explained scientifically "without major revisions elsewhere in scientific theory", and if it "thwarts our familiar expectations" (1979, p. 260). But Braude does not specify just what revisions we might be called upon to make elsewhere in science, nor does he indicate how, having made them, we could establish their correctness. And his examples of "thwarted expectations" (such as "if a television set turned into Leonard Bernstein") are, if they are intelligible at all, several orders of magnitude more challenging than the reports from the parapsychologists' laboratories. Braude is in fact aware of some of the problems posed by this definition, and offers a much more radical one: that a phenomenon is paranormal if it is in principle inexplicable (by science).

Mabett (1982), in reviewing some of the literature on the definition of "paranormality", analyses it into three categories. The first takes paranormal phenomena to be unexplained, though not in principle inexplicable by science; the second takes them to be inexplicable in principle by science (but not uncaused or totally inexplicable); the third, that they are ultimately inexplicable. It is not clear whether Braude's latter
definition would fall under the second or the third of these headings, and Braude himself admits to uncertainties: if he intends it in the second (weaker) sense, then, should an explanation of, say, telepathy be forthcoming at some future time, then we should have to say that telepathy was not (on this definition) a paranormal phenomenon at all; if he opts for the third (stronger) sense, then, assuming an inexplicable phenomenon to be an impossible one, as Braude does, then there can be no paranormal phenomena at all. In this latter point, Braude is, I believe, mistaken, for he is assuming that any scientific system is (or could be made) complete, so that anything that fell outside its comprehension would be automatically and properly ruled out as impossible. Such a view is difficult to sustain: the history of science is replete with counter-examples, and it should be noted that Meehl and Scriven, in their rejoinder to G.R. Price (Ludwig, 1978), firmly reject any such notion.

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It is often said that the existence of paranormal phenomena threatens to overturn the edifice of science. This claim, which is open to serious question, may be interpreted in two different ways. First, it could mean that numerous standard, apparently well-founded theories in physics, psychology, and other areas are going to have to be abandoned, in order to accommodate the findings of psychical research. Secondly, it could be interpreted as meaning that the operation of paranormal phenomena is so insidious that it effectively undermines the experimental processes of science; this view, which is not so commonly discussed, I shall take up in greater detail in Parts Three and
Four, when I consider the question of the scientific status of psychical research.

Although the standard sceptical arguments against psychical research deny outright that science could be threatened, there is an intermediate view, which has been named "Flewism" by Beloff (Ludwig, 1978, p.367). Flewism is a form of mitigated scepticism which is prepared to accept (at least some of) the reported phenomena, but which interprets them in such an attenuated way that they are capable of carrying little weight. If such a programme could be sustained, then paranormal events are to be explained away as coincidences (interesting ones, perhaps, but coincidences nonetheless), and their potential impact on science would be minimal. Psychical research, on this view, deals in mere oddities.

It is infortunate that though this matter of the conflict between science and the paranormal is often raised, its precise nature is rarely specified. One reason for this is that, as has often been said, psychical research defines its subject matter negatively: it concerns itself with those things that do not fit in. As Mrs Kneale puts it: "psychical research is what is left over when the regular sciences have marked out their territories. It is that collection of facts which physics, biology, psychology, etc., have failed to assimilate" (Ludwig, 1978, p.67).

Nonetheless, the phenomena with which psychical research deals constitute a very odd collection, and some of them, such as apparitions and poltergeists, do not, one could say, fit the scientific picture in any way at all. But then, there are others that perhaps might. Take, as an example, the remarkable navigational
capacity of homing pigeons. For many years, biologists had not been able to offer any satisfying explanation for this, and the possibility that a paranormal factor might be responsible was canvassed by a number of psychical researchers (see, for example, West, 1954, p.129, and Wolstenholme, 1956, pp.156-186). In the event, however, this particular phenomenon has now been largely explained in "normal" terms: the crucial problem is solved, and the solution in no way invokes paranormal capacities, or indeed anything remotely psychical.

That said, the psychical researchers must be given credit for realizing that the problem could admit of wide solutions, and for attempting to furnish one within the terms of their own specialism. As it turned out, their attempt was misguided, but that does not mean that it should never have been made: psychical research is perfectly entitled to put in bids for the right to investigate any phenomenon which is currently unexplained (provided, of course, that it is prepared to come up with a serious explanation at the end of it: simply to say that homing pigeons use clairvoyance is neither a working hypothesis nor an experimental result, although, superficially, it could be read as either).

Any such explanation would, however, have to contain one distinctive feature: parapsychology, to justify its claim to a stall in the scientific marketplace, must not simply essay explanations of phenomena which are, in the present state of scientific knowledge, unexplained (all ground-breaking scientific research, surely, is concerned to do that). What it must do, if it is to make a distinctively parapsychological contribution, is to put forward explanations which are
distinctively parapsychological. This may sound tautological, but what makes the study of, say, physics into the discipline of PHYSICS is not simply that it offers us explanations of physical phenomena in the world: after all, philosophy or theology might attempt to do that. To be PHYSICS, it must couch its explanations in strictly physical terms. To explain physical phenomena in theological terms is not to do "theological physics" (whatever that might be), it is to trade one discipline for another. Similarly, if parapsychology is to command individual attention as an experimental science, it must make its particular contributions in parapsychological terms: it must explain the phenomena in ways which are parapsychologically interesting.

As well as defining its subject matter negatively (those things that do not fit in), psychical research makes use of an important silent assumption in describing its experimental work. This assumption could be called "the polarity principle". The experiments, we are told, are devised with stringent precautions, to ensure that sensory perception and normal inference of the targets are precluded. The use of statistical methods, to assess the odds against chance, then erodes the possibility of explaining away the scores in terms of coincidence. It is then tacitly assumed that, because the obvious normal explanations have been thus eliminated, the only explanation available must be a paranormal one. Only two poles of explanation are allowed, and when one is discounted, the other must apply; this is taken to be a strict dichotomy, allowing no middle ground. In this way, sceptics have been pressed into the position of denying that the precautions were adequate: hence the suggestions of
fraud (as in G.R. Price, or Hansel), or of sensory cues (as in Dingwall, 1973).

But the apparent simplicity of the polarity principle may be misleading, for until we know more about the paranormal, the assumption of a "psi hypothesis" is less than illuminating. To say "having eliminated all forms of sensory contact and chance coincidence, we feel that our results are consistent with the psi hypothesis" is not particularly illuminating, because the "psi hypothesis" can quickly degenerate into the simple statement that there is no satisfactory explanation for these results. Simply to invoke psi is not to explain.

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So can psychical research lay claim to scientific status? This is a question which I shall address in Part Three, but we should note that any positive answer ought to place considerable stress on the possibility of normalization: that is, if science is to comprehend the phenomena, we shall need to understand whatever laws govern their operation. The paranormal event, as it stands, seems to obey no laws at all, and the problem with so much of the present terminology is that it predisposes us to assume that the phenomena do not fall within the ambit of science. Such an objection need not be fatal, however, since, were knowledge to advance, new definitions might be introduced.

But normalization is, for the psychical researcher, a double-edged weapon. On the one side, normalization is the whole purpose of the enterprise: to establish the (so-called) paranormal within the framework of science. But on the other, it could result in the dissolution of
psychical research. As the phenomena become hived off into various specialized areas of science, so psychical research shrinks and withers. We have already seen this taking place with hypnosis, and a similar process is at present overtaking the out of the body experience (OBE). Blackmore, in the course of her exhaustive study (1982a), arrives at a purely psychological hypothesis to account for these experiences: they are to be investigated and explained in terms of altered states of consciousness. There is no "astral projection", and "nothing leaves the body in an OBE" (p.240). But even if psychical research lost the out of the body experience to psychology, it would not mean, she has claimed (1982b), that the psychical researchers should abandon study of them altogether. This rather generous interpretation would only apply, I think, to those who had originally trained as psychologists, and they would have to work within the confines (the paradigm, even) of psychology, and the hypotheses and explanations which they put forward would for that reason be devoid of any specifically parapsychological interest.

The directions that psychical research might take could proliferate indefinitely: in the 1970s, as a result of the work of Targ and Puthoff, people began to talk of paraphysics, though whether this was an attempt to introduce a new discipline alongside parapsychology, or a replacement for it, was never made clear. The phenomena studied by the paraphysicists seem to be of a piece with those studied by "conventional" parapsychologists. The only obvious difference is that paraphysics is the preserve of trained physicists who have (understandably) a bias in favour of finding physical explanations (rather than psychological ones) of the phenomena. But since both schools have been conspicuously short on explanations (both psychological
and physical), there seem to be no grounds for making an adjudication. Such fragmentation might, of course, carry on indefinitely: we might, perhaps, in future years, witness the inauguration of parachemistry, paraphysiology, even parasociology. The lack of any guiding principles of what is to be taken seriously in this domain indicates a seriously impoverished level of attainment in psychical research.

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I have outlined the central subject matter of psychical research, and touched upon some of the problems that must be overcome if it to lay a serious claim to enjoy scientific status. These matters will be dealt with in greater detail in Part Three. In Part Two, I shall concentrate on the essential metaphysical presuppositions of psychical research. It is, I believe, at the most fundamental level that psychical research goes wrong. And if its basic concepts are confused at the very outset of the investigation, then no amount of well-designed experiments (even those that produce statistically interesting results) will serve to demonstrate the importance of allegedly paranormal phenomena.
PART TWO

THE METAPHYSICAL FOUNDATIONS OF PSYCHICAL RESEARCH
CHAPTER FOUR

PSYCHICAL RESEARCH AND THE PHILOSOPHY OF MIND
I: DUALISM

In this chapter and the succeeding one, I shall discuss two central issues which psychical research comprehends. I call them central because the whole research programme depends upon them, and because they are of considerable philosophical importance. First, what is the nature of the relationship between the mind and the body, and secondly, can we claim that human beings (in any significant sense) survive death? The position of psychical research in regard to the former question is a curious one, as many of its proponents presuppose a dualist position, yet persist in producing empirical arguments in order to prove it. I say that this is curious, because dualism is a philosophical theory, and as such should be argued for on a philosophical basis - if the arguments are sound, then empirical results are otiose; but if the philosophical arguments are inadequate, then no amount of well-intentioned experimental work can serve as a proof.

I shall outline some of the ideas that are put forward by psychical researchers concerning this crucial question of the relationship between the mind and the body, and the quasi-religious view of the world that underlies them. Next, I shall draw attention to some problems of the dualist theory, problems which, I believe, render it untenable. The experimental work relies on two significant presuppositions which cut across the presumption of dualism: there is, I shall
argue, a tension between these two positions. In the following chapter, I shall consider the question of survival, which bulks so large in the literature of psychical research.

Psychical research is often stated to be an open-minded examination of the evidence for the paranormal. But there is a "hidden agenda"; what lies behind the collection of cases and the recording of scores is the need to prove a point, and a very large point at that. As Dingwall has put it (Angoff, 1971, p.38), "I do not think I could name half a dozen [parapsychologists] whom I could call objective students who honestly wished to discover the truth. The great majority wanted to prove something or other", and that something or other was a religious belief: for Victorians such as Myers, new scientific discoveries had seriously undermined the foundations of their faiths. The search for telepathy might reveal that mind-to-mind communication was a fact, and if the mind could communicate without using bodily organs, then the mind might be separable from the body. The existence of an ability to guess a few playing cards correctly was going to be used to prove the immortality of the human soul. "Thus the supernatural might be proved by science and psychical research might become, in the words of Sir William Barrett, a handmaid to religion."

This emphasis on religion is pervasive; thus Myers describes psychical research as "the preamble of all religions" (quoted by Flew, 1987, p.15). Conan Doyle, who was more of a Spiritualist than a psychical researcher, nonetheless based his beliefs on the "evidence", precisely the same kind of evidence that the psychical researchers themselves were investigating: "Nothing is secure until the religious
basis is secure, and that spiritualistic movement with which I am proud to be associated is the first attempt ever made in modern times to support faith by actual provable fact" (quoted in Hall, 1978, p.95). Rhine was no exception to this trend, talking ardently of solving "first-rate problems [of theology] with first-rate methods [of science]" (1954, p.206), and looking to parapsychology to offer "the possibility of a scientific approach to the question of the basic nature of man" (1948, p.17)

The truth of dualism is to be taken as understood. From dualism it is short step to the possibility of survival, and thence, to religion. Nearly all writers that have started from this point have followed the same path. C.D. Broad is a significant exception. In the two versions of his BLPs (1953, p.10, and 1962, p.4), we are offered the following:

A. "A necessary ... immediate condition of any mental event is an event in the brain of a living body. ... Mental events which are so inter-connected as to be experiences of the same person are immediately conditioned by brain-events in the same brain. If two mental events are experiences of different persons, they are in general immediately conditioned by brain-events which occur in different brains", and

B. "We take it for granted ... that a person cannot directly initiate or modify by his volition the movements of any thing but certain parts of his own body". Broad also offers (in the later version only) a principle concerning survival, which I shall discuss in the succeeding chapter:

C. "we take for granted that, when a person's body dies, the personal consciousness, which has been associated with it and expressed through it during his lifetime, either ceases altogether, or, if not, ceases
to be able to manifest itself in any way to those still living on earth".

Broad is assuming that the body is accompanied by a mind or a "personal consciousness", which is capable of acting upon the brain and body (and only on the brain and body), and which achieves articulation, as far as we on earth are concerned, through the vehicle of the body. But Broad goes no further, and certainly does not commit himself to any belief in survival or in religion. However, even Broad's limited position is not without its difficulties. He uncritically puts forward a number of doubtful philosophical theses, concerning such problematic matters as the interaction between minds and brains, and the idea of acts of volition, without attempting to justify them.

Principle A is used to specify (negatively) cases of telepathy. The point here is that when an experimenter looks at a target card which happens to be a circle, and the subject records a circle on his scoresheet, we should usually interpret this to mean that two mental events take place, and that these events are dependent on two brain events. What Broad wishes to put forward is the view that in paranormal cases, this picture may not apply: at the crucial moment, there may be some purely mental mediation between the experimenter and the subject, of a sort that by-passes their brains. To guarantee the paranormal interest there must be some form of mediation, of course, since in its absence, all we should have would be mere coincidences. But Broad really is overstating the case, for it is no less cogent to argue that there is mediation operating from brain to brain rather from mind to mind, but that our present technology for neural monitoring is incapable of detecting it. The most that we can say of ESP is
that it is not decisively a purely mental event, but
neither is there any evidence to show that it is a
brain event either. It may just be an anomaly (in
either sense of the word).

Broad's principle B, which would be transgressed if
cases of "paranormal energy transfer" (PK) were to
occur, assumes that only bodily movements are initiated
or modified by a person's "volition". The point of
introducing this notion is that, it is alleged, some
people can, some of the time, use their volition to
move objects directly (that is, without touching them).

There are two points here that call for closer
attention. First, the idea of "acts of willing" or
"acts of volition" is obscure (see Melden, 1961, ch.5).

It is even more so in relation to PK, for in the bodily
cases, we can posit a number of causal links, described
in terms of neural pathways, muscular articulation, and
so on. The philosophical difficulty appears when,
having traced the causal chain back from, say, the arm
that was raised in the air, to the appropriate brain
event, we attempt to import talk of "minds" or
"personal consciousnesses". The chain breaks at just
this point. The problem with PK cases is that there is
no clear chain at all (certainly no demonstrably causal
one). The experimental subject is supposed to perform
an "act of willing"; Rhine puts it as follows: "many
people believe they can mentally influence dice. They
think they are able; when in certain moods, to
influence the fall of dice by the direct action of the
will" (1948, p.80). However, in what this form of
willing consists, and how it sets about its operations
are questions which are never satisfactorily answered.
There are no causal links shown forth (or at least none
that go beyond post hoc ergo propter hoc), and the
nature of the "willing" itself amounts, we may assume, to little more than intense concentration.

The second notion that Broad introduces is that of "direct" movement. He allows that in the case of my raising my arm, I am using my volition directly to achieve that end. If, however, I lift the paperweight from my desk, a process which would also require volition, then I should be said on his account to be lifting it indirectly, a usage which seems perverse in the extreme. If I arranged a series of strings and pulleys, and hoisted the paperweight into the air in this way, then I should quite properly say that I was "lifting it indirectly". But if I reach out, grab it, and bear it aloft - what could be more direct than that? The point is not a purely verbal one. By drawing a distinction between direct and indirect action, Broad is suggesting that the kinds of action that we use in moving parts of our bodies and also in influencing the fall of dice in a PK experiment are of a piece: something that the will (the mind) does to the physical world.

When I lift the paperweight, the story goes, my mind directly moves parts of my body, which does the gross work of lifting at one remove. In the PK experiments, the bodily effort is by-passed: the feat is achieved by pure volition, or will. But I think that Broad has allowed himself to misled by an ambiguity of the word "will". When I lift the paperweight of my own volition, of my own free will, this is a trifling matter, requiring no great concentration. To "will" the paperweight to hover unsupported in the air is, however, a very different matter, requiring, one might imagine, some act of willing in a very much stronger sense. We should note that Rhine, who carried out
experiments in what he called a "tug-of-wills", in which two subjects would attempt to influence the dice in competition with one another, clearly had such an idea in mind.

The problem of the mind and its place in psychical research has pervaded the literature, but rarely are the philosophical implications given adequate consideration. The central question here is: to what extent might empirical discoveries in psychical research clarify (and perhaps modify) our understanding of mental concepts? Large claims are often entered, but can they shown to have substance? The diversity of opinion affords an indication of the confusion. Some writers have held that if telepathy were to be demonstrated, then materialism will have been falsified. H.H. Price (Smythies, 1967) and D.M. Armstrong (1968) have, from diametrically opposed positions, come to such a conclusion. On the other hand, Mundle (Wheatley, 1976), Mellor (1975), and Cooper (Thakur, 1976) have all proposed that telepathy and materialism are, in principle, compatible. Carrington (1932) has imported quasi-Freudian terms in order to explain the operation of ESP (assuming that the invocation of a "subconscious mind" to account for the phenomena would somehow clarify the matter). Beloff (Wolman, 1977) argues that the experimental results point towards a dualist interpretation; Rhine (1948) claims that they actually prove dualism. Wheatley (Krippner, 1977) leans to a monist account. Those who hope to find in psychical research a body of evidence which could be used to solve the "mind-body problem" have been frustrated. Far from psychical research providing us with empirical data upon which we could base a conceptual reconstruction, it has served to
confuse rather than clarify: the philosophical difficulties remain untouched.

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What is wrong with dualism? The theory has a long and profoundly impressive pedigree, is uncritically accepted by many people, and can be said to pervade the language and ideas of Western societies. Nonetheless, it is, I believe, mistaken, and its central ideas infest psychical research. The idea that we consist of two radically different kinds of stuff is deeply counter-intuitive: I am a human being, a person. The possibility that I am actually two separate things, which, by a happy circumstance, maintain a single, synchronized existence for most of the time is, whatever sanction it may find in ordinary language, an improbable option. As Flew has pointed out, "the assumptions of the Platonic-Cartesian way, which in some contexts we find it so easy to make, are nevertheless extraordinary and extraordinarily questionable" (1976, p.108). The dualist view allows that not only is there a close synchronization between my constituent parts, but that one of those parts is the real me, capable of carrying on a continued existence, long after my body has perished.

One might ask, how is this synchronization, this coordination, actually achieved? For the majority of people and for most of the time the coordination appears to be perfect. Even supposing that we can explain this, how do we account for the fact that these fundamentally different substances can act one upon the other? What mechanism can react with something that lacks any material existence? By what conduit can something without substance influence the physical
world? If minds are made of some invisible substance, and we can only observe the workings of our own, then what of other minds - do they exist, or are they merely figments of our imagnation? All we see, according to the dualist, is bodies: gross, material bodies. What of the tenebrous, impalpable spiritual presences behind them? Are they there (in a serious sense) at all? And if they are, how can we tell? It is one thing to identify, to individuate, different physical objects (such as bodies). There are well-established methods for doing this, but for minds, we have no criteria. A mind that is "articulated" through a human body can be identified (and re-identified) by reference to the criteria that we use for bodies and for persons. A disembodied mind cannot be subject to the same rules, for it is, to take just one example, assumed to have no spatial location.

These are just some of the familiar arguments that have been raised against the Platonic-Cartesian dualist picture of the person. There is much to be said for abandoning this theory in favour of a monist view, which has the advantage of economy, and does not suffer the same defects (monist theories, it should be said, are not without defects of their own, but the better forms of monism collapse less readily into incoherence than does dualism).

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Although the experiments in parapsychology are dualistic in their underlying assumptions, their procedures derive from an altogether different school of thought, that of behaviourism. When J.B. Rhine instigated his programme of experimental work in the late 1920s, the American psychological establishment was very firmly behaviouristic. The introspectionist
school had gone into a sharp decline, partly due to its problematic presuppositions, but mainly because of J.B. Watson's polemical attacks, and his proposal of a research programme based on objective physiological data. The Freudian school and its numerous derivatives held a place, but it was not to assume any central importance until the 1930s, as a consequence of the emigration from Middle Europe.

It is important to appreciate that Rhine, who was not by training a psychologist, and MacDougall, his mentor, were both opposed to Watson's "revolution" in psychology (Rhine, it should be noted, had religious reasons: he was an ardent Baptist, and had considered, after taking his doctorate in botany, becoming a minister). Both men sincerely felt that the new behaviourist paradigm told less than the whole truth about the nature of mankind. To Rhine this nature was still a "profound mystery" (1948, p.11): "we hear very little about the problem of what we are" (p.13). If paranormal phenomena could be demonstrated scientifically, then the "traditional belief in man's spiritual nature" (p.16) would be vindicated. We could show that there was more to human behaviour than a mere collection of conditioned reflexes: "More recently a number of psychologists have recognized that behaviourism 'leaves Hamlet out of the play'" (1954, p.164). A fortiori, the paranormal might point the way to a proof of the existence of the human soul (a claim Rhine explicitly endorses). Such souls might also lay claim to some form of immortality, though this is admitted to be more problematic: "the question is merely, 'Is there anything extra-physical or spiritual in human personality?' The experimental answer is yes. There is now evidence that such an extra-physical factor exists in man. The soul hypothesis as defined
has been established, but only as defined. Not the supernatural character of the soul, not its divine origin, its transmigration, its immortality — indeed nothing has been dealt with so far but its elemental reality" (1948, pp.165-166).

Such beliefs are not uncommon in the literature of psychical research. H.H. Price has claimed, for example, that "the man who denies survival [of death] is certainly on dangerous ground. But I think that he still has a leg, or half a leg, to stand on" (Smythies, 1967, p.35). But he does not offer any good reasons for taking such a dismissive position. A Platonic yearning for dualism, which has been discussed by Robinson, (in Ludwig, 1978, pp. 87-88), and which is taken to license all manner of phantasies about a "life beyond", is one of the central ideas in Rhine's world-view; the other is a passionate distrust of mechanistic explanations. We can, I think, see a broadly similar pattern in the work of Arthur Koestler, who wrote a vigorously polemical work attacking behaviourist psychology, organized an international conference attacking reductionism, and wrote fluent and provocative works on parapsychology.

Koestler's anti-mechanistic tendencies are clearly displayed in his defence of the biologist Paul Kammerer, where he attempts to rehabilitate some of Lamarck's ideas: "If Darwin was wrong in some important respects, that does not mean that Lamarck was right. But it might mean that Lamarck was not completely and entirely wrong" (1971, pp.125-126). It is interesting that Rhine anticipated Koestler in this matter: in a tribute to his mentor MacDougall, he wrote that "although by that time [the 1920s] biologists had almost all rejected the old hypothesis of Lamarck ...
in favour of other theories more in keeping with the mechanistic trend of the age he did not hesitate to reopen the question. ... He has given his results to science, regardless of the fact that in so doing he stands practically alone in his conclusion" (1937/1950, p.36). Rhine, it seems, is drawn to Lamarckianism, at least in part because it is out of step with the "mechanistic trend". This is a poor reason for accepting a scientific theory, but in any case, it misses the point. Both Lamarck and Darwin postulated mechanistic theories. The mechanism for Darwinian evolution was discovered, and that for Lamarckianism was not, and the theory is immeasurably the poorer for this failure. It is no answer to say that Lamarckian inheritance actually happens, but in a non-mechanistic, spiritual way.

Given that Rhine was so implacably hostile to behaviourist psychology, and to mechanistic explanations in general, it is odd, to say the least, that the programme of parapsychological experiment to which he devoted his career should be firmly based on behaviouristic principles and practice. In the classic work on card-guessing and dice-influencing, no use is made of introspective techniques (Rhine actually rules this out as impossible, in Hook, 1960, p.73), nor indeed of psychoanalytical or psychophysiological models. The technique simply demands that the subject's performances with cards or dice are compared with the outcomes, and the final result is matched against the mean chance expectation.

This does not appear, at first, to have much in common with the behaviourism first propounded by J.B. Watson: there is, for example, no talk of reflexes, neural pathways, or conditioning, although Rhine's use of
rewards for correct guesses (1948, p.120) has much in common with the behaviourist's reinforcement. However, I shall show that Rhine's experimental approach is solidly based on behaviouristic principles.

By the end of the nineteenth century, experimental psychology had a toe-hold on scientific credibility. It had established two major lines of enquiry: the psychophysiological and the introspectionist. The former wing concerned itself with exacting measurement, particularly in the area of perception, which formed the foundation for the subdiscipline of psychophysics. The latter concentrated on the "softer" aspects of mental life, relying on observers who had been trained to introspect their inner states and processes, and report on them.

Now Watson repudiated everything that introspectionism stood for: he pinned his scientific faith firmly to the psychophysiological school, and acknowledged the work of Pavlov as his starting point. Watson's attacks on dualism are as intemperate as those of Rhine on behaviourism: in his more polemical moments, he is apt to dismiss any talk of "mental" activity whatsoever. It is hard to know how to read Watson, for he changes frequently from denying the scientific utility of mental concepts (methodological behaviourism) to denying the existence of mind altogether (metaphysical behaviourism) (Lord, 1977). But what emerges from his attempt to fashion a thoroughgoing scientific psychology is a form of words which behaviourists ever since have regarded as a fundamental creed: "The behaviourist asks: Why don't we make what we can observe the real field of psychology? Let us limit ourselves to things that can be observed, and formulate laws concerning only these things. Now what can we
observe? We can observe behavior - what the organism does or says" (1930, p.6).

This demand for interpersonally perceivable data lies at the heart of Rhine's experimental work. Watson, unable to pin down Thought under laboratory conditions, found that laryngeal movement (essentially, talking to oneself) was accessible, and boldly redefined "thought" as laryngeation: no further investigation of "thinking" as such would be necessary; close attention to the larynx would suffice. In much the same way, Rhine, unable to detect any ESP occurring, resorted to statistical tests with Zener cards. Rhine limited his observations to what his subjects did or said, or, more accurately, what they wrote on their scoresheets, for no serious attempt was made to discover what they were doing to guess as they did.

The experiments were conducted on a "black box" principle, which derives from the familiar behaviouristic model of stimulus and response. The experimental subject is presented with a presumed stimulus (that is, the "agent" concentrates on the next card), and she responds accordingly (the "patient" writes her guess on the scoresheet). But no consideration is given to the problem of finding out what is actually going on in each of the black boxes during the experiment. Rhine, in his very experimental design, is precluding the possibility that we might discover anything about ESP or PK, except that they can be assumed, on purely statistical grounds, to occur. His position in the behaviourist tradition is closer to that of B.F. Skinner than of J.B. Watson, for Skinner takes the whole of the organism as his basic unit of analysis, whereas Watson was interested in the underlying neurophysiological structures. Rhine simply
reports gross behaviour patterns, without making any attempt to probe beneath them.

Now it is true that a commitment to methodological behaviourism does not entail a commitment to metaphysical behaviourism, but in Rhine's landmark experiments, we have a situation where the former principle is embraced in order to disprove the latter. It might be that Rhine, intent upon demolishing the behaviourist edifice that bulked so large in the psychology of his day, had deliberately designed his experiments according to that model, for the purpose of attacking behaviourism from within, as a Trojan horse, but there is nowhere in his writings any hint that this was his purpose, and, in any case, I suspect that Rhine lacked the intellectual subtlety for such an approach.

But there is a further problem with this black box model: because no attempt is made to look inside the boxes, no one knows just what might be happening when somebody makes a correct guess. And even the person making the guesses is in no position to say, because, as has sometimes been noticed, paranormal phenomena of both the ESP and PK varieties occur without conscious awareness.

I shall refer to this as the "unconsciousness principle", and shall have more to say on it in Part Three. Now there is a great deal of loose talk, especially in the semi-popular literature, about "projecting thoughts", or of thoughts being "transferred directly from one mind to another" (Rhine, 1948, p.18), but because of the unconsciousness principle, these are forms of words that go well beyond what we can legitimately assert. Rhine was aware of the
problem, and mentions it twice in *New worlds of the mind* (1954):

Let me emphasize, too, that psi is profoundly unconscious ... it is simply not capable of being dragged into consciousness unconverted and direct. Such seems to be the case as it stands today. There are not even any good leads to conscious control in sight (p.92).

Later, and more trenchantly, he declares: that psi is unconscious has already been characterised as being the most important psychological fact about it. It can be emphasized further that the operation of psi is really unconscious. It is unconscious in a different degree or way from experiences that are merely forgotten or repressed or left out of consciousness by the shift of attention or preoccupation with an object of concentrated interest. The operation of the psi function is, so far as the researcher can indicate, irrecoverably unconscious (p.174).

Recent research has not altered this situation.

It follows from this that no one can be said to use psi, or to have a psi ability: asking an experimental subject to co-operate by using her ESP is rather like asking her to digest her food. Any attendant willing, concentration, or mental imagery is irrelevant to the matter in hand. Similarly, when she writes down her guess on a scoresheet, all she is doing is making a mark on a piece of paper: she is not recording a "psi thought", nor telling of a "psi experience", since there are none. No special feeling accompanies the two or three correct "paranormal" guesses that would mark them out from the five "normal" ones in a run of twenty-five cards. But if the subject doing the guessing is not conscious of what she may be doing, the person who is "transmitting" is in no better case. He does not know (no one does) how to "transmit a thought", and psychical researchers have failed to supply a prescription. He may close his eyes, screw up his face, tense his muscles, and concentrate furiously, keeping
the image of the target before his mind's eye the while. But it is not clear just how this can be said to be a projection. Given the unconsciousness principle, there seems to be no ready way of discovering what is going on. Thouless (1972) thinks that this problem might be solved by the ingenuity of future experimenters, but we should need to know so much more that such sentiments seem merely to be the hollow expression of a pious hope.

However, talk of "abilities" and "performances" is still frequently met with. For example, Celia Green claims that she "worked out how to do ESP" (1971, p.17), though the remarks she subsequently offers are little more than descriptions of so-called psi-conducive states rather than a genuine recipe for bringing ESP about (the distinction is an important one: if I have to learn a large body of material, say for an examination, then peace and quiet may be necessary for my being in a "learning-conducive state", but knowing that in no way helps me to set about the actual business of doing the learning).

In 1983, the Bulletin of the Foundation for Research into the Nature of Man described (anonymously) a recently devised computer program which offers "a game of mind over matter", in which the player tries "to mentally influence the number of spots that will be shown on a pair of dice that are displayed on a VDU screen" every time a key is pressed (No.30, pp.1-2). But how this influence works, in physical terms, and how we make it work, psychologically, are points that are never discussed. Similar examples could be produced indefinitely, but I shall add just one more: Louisa Rhine, although she notes that psi is unconscious (1971), still comments on the "forms of psi experience"
(p.46), and on "a general capacity to know without the senses or to move objects by mental force" (p.33). To assume that psi is unconscious and at the same time to speak of "experiencing" and "knowing" in such contexts is to take an untenable position.

To avoid the problems inherent in such terms, we might prefer to be neutral, and to speak of "capacities", (though I admit to some misgivings here, since capacities and abilities can be hard to untangle). There is a considerable discussion as to whether a psi capacity is spread more or less uniformly through the population, or whether it is the prerogative of a few select individuals. Thus Wheatley considers that the evidence points to the view that "most people simply do not have psychic experiences or capabilities at all" (Krippner, 1977, p.166), whereas Targ and Puthoff, in respect of their own subjects at least, say that "it turns out they're all gifted" (1977, p.69). Lurking behind this is what might be called the evolutionary problem: it is an index of the state of psychical research that some writers are able to argue that psi is an ancient relic of our heritage, which, with increasing civilization, we are losing, and others that it is a new power, which we are on the threshold of developing.

Psychical research is in a state of confusion. It does not help us to clarify the nature of our mental concepts, but merely muddies the waters further. Its attempt to reanimate the Platonic theory of the person fails to consider the philosophical problems involved, and its black box experiments and the unconscious nature of the phenomena both point the way towards a damning critique of the scientific status of parapsychology. However, psychical research places
particular stress on the importance of the question of survival: I shall now turn to a consideration of this matter.
The question of the survival of death is far too large a topic to be fully discussed in a single chapter. Nonetheless, it occupies such a central role in psychical research that it cannot be avoided. As I have already noted, psychical research tends to assume that the survival issue is a purely empirical one, which can be settled by recourse to the evidence. Psychical research, as a scientific enterprise, is uniquely qualified to undertake the collection and evaluation of such evidence. That, at least, is the unspoken assumption. The hope of many was and is that after centuries of doubt, religious beliefs might be put on firm foundations.

But even if solid evidence were to be forthcoming (and I shall argue that this is by no means as straightforward a possibility as it might at first sound) it would not settle the matter conclusively. Survival, for example, is not to be equated with immortality, nor does it imply the existence of a Deity. Proof of survival, tout court, then, would not go very far towards establishing what many people seek. It may be, for example, that we do survive, but only for five years before annihilation overtakes us. If such a situation obtained, we should be able to talk of survival, but it would not obviously imbue the Universe with any extra meaning or purpose.
I am assuming, of course, that evidence could be produced and assessed. There are good reasons for thinking that, in principle, this is not the case. For the evidence would have to be of a very strange sort. The most direct evidence, it might be urged, would be that presented to me: when I am dead, I shall know whether I survive or not. Even this will not answer all my questions about survival, though. Does everyone survive, or am I one of the exceptions? But to talk of "evidence" in such a context is to overstate the case; evidence needs to be interpreted within a conceptual framework, and after death, I (as a survivor) am going to be so radically altered, as are my surrounding circumstances, that the reliability of my interpretation might seriously be called into question. More strongly, we could argue that in the personal case there is no such thing as evidence. If I do not survive, then ex hypothesi, I cannot have evidence against survival, and if I do, then evidence is irrelevant. Finding myself to be surviving, I should no more seek evidence to prove that I was than a fully awake person would try to establish that she was not dreaming.

These problems aside, it is clear that for practical reasons we must seek indirect evidence of survival. In psychical research, such evidence is of two kinds. First, there is the information that is acquired from mediums who claim to be in touch with the dead, and to act as a conduit between their world and ours. Secondly, there are cases that suggest some form of reincarnation, in which a living person has some memories of a "past life". Both these types of case (which derive, it should be noted, from different religious traditions) presuppose that something is there to survive: it has a non-physical nature, and can
continue to exist either as a separate entity on a "spiritual plane", or in virtue of its capacity for informing a new physical body. These views need not be read as mutually exclusive.

In an attempt to introduce a measure of scientific control, tests have been devised using messages enciphered in a manner known only to one person. If before their death the cipher proved impenetrable, but subsequent to their death it could be broken through the agency of a medium, then this would be strongly suggestive of survival. But such an interpretation depends upon a substantial leap of inference. Thouless, who made a special study of such messages, and left his own (1963, p.130; 1972, pp.162-164) was especially concerned to rule out the possibility of decipherment by ESP. He tested various people, to see if they could, using telepathy, discover the cipher key from him. They failed. If, now that he has died, the messages could be revealed, then this, he would have urged, is strong evidence for survival. But the inferential leap is made at precisely this point: the assumption made is that telepathy generally takes place simultaneously. The medium who failed to crack Thouless's cipher five years before his death but who succeeded five years after may not have been in post mortem contact with him at all, but relying on retrocognitive telepathy, or even on precognitive clairvoyance (reading off the account that was to be published some months later).

The canons of reasoning, common-sense, and economy to which Thouless helps himself cannot so readily be held to apply in psychical research. Such tests as Thouless proposes might be suggestive of survival, but we could not say more than that. It is not that any one of the possible paranormal explanations that we might put
forward is more or less preferable, or more or less economical; the point is that there is no way of telling which is to be accepted.

Writers on mediums and the survival question often refer to the material known as the "cross-correspondences", a considerable body of cases collected by the SPR, in which automatic writings produced independently by different mediums can be compared. When comparisons are made between the different scripts, occasional striking coincidences can be found. This is taken by some to be evidence for survival. It must be said, however, that these coincidences occur sporadically, with no detectable pattern, and are of a trivial nature. The bulk of the material is enormous, and much of it remains unpublished. The task of assessing it thus a formidable one. I can only say that, despite assertions to the contrary, ESP is here a no less cogent paranormal explanation of the evidence than the survival hypothesis.

The preference for an explanation in terms of ESP rather than survival was argued for by Dodds (1932), who pointed out that there was strong independent evidence for telepathy, whereas the spiritualistic interpretation was fraught with difficulties. But we can put the case a little more forcibly: there is no method for excluding telepathy or clairvoyance from the displays of mediums. Once we accept the possibility of ESP, we cannot ignore it simply because it does not suit our present purpose. Admittedly, such ESP would have to work at a higher level of efficiency than we should expect from the card-guessing experiments, hence the name that is often given to the supposed quality: "super-ESP".
The problem may be stated by means of an example. Let us imagine that a medium contacts a "spirit", who reveals an item of factual information about a member of the sitter's family. If this information can be verified, then it suggests that the medium was genuinely in touch with the spirit. If it cannot be verified, then it is, as evidence, worthless, and must be discounted. The means of confirmation must be available. Let us say that the information is then checked in the relevant parish registers, and found to be correct. On the face of it, here is a piece of evidence for survival. But it could, just as convincingly, be interpreted as an occurrence of super-clairvoyance, whereby the medium was able to gain access to the contents of the parish registers by paranormal means.

Gauld (1982, p.15) admits the intractability of this problem, but feels that, because the idea of unconscious super-ESP is "unobservable", it is profitless to advance it; the postulation of super-ESP is "barren of further consequences". This may be so, but it seems more like a powerful argument for finding out about ESP before going on to consider survival, rather than for dismissing it out of hand. We should also note that "ordinary ESP" (if we may call it that) is equally unobservable, yet parapsychologists continue to investigate it. Once we admit the possibility of ESP, then it contaminates all serious evidence for survival.

The cases of ostensible reincarnation have been actively studied by Stevenson (for example, 1975 and 1979). There are serious methodological problems in these reports, and their interpretation is by no means unproblematic (see Wilson, 1981, pp.53-63). What is
particularly noteworthy is the absence of any law-like regularities in the experiences: there seem to be no rules governing reincarnation. If psychical research is to make good its claim to scientific status, it must not only present us with claims that have been investigated by scientific methods - it should also be able to show that its findings hang together in a systematic pattern.

The interest of the psychical researchers in survival is, of course, intimately connected with the dualist picture: if a physical body is accompanied by a non-material consciousness, then perhaps that consciousness can detach itself from the mortal encumbrance of its material shell. Such detachment need not require the death of the physical body: the out of the body experience (OBE) is a case in point. When somebody reports having had an OBE during sleep, during which they were able to have access to information not normally available, then many psychical researchers would be inclined to refer to this as an OBE. But as in the survival cases, there is nothing to be said against an account in terms of ESP. There is actually something very odd about such an OBE account: Leggett, an enthusiastic dualist, claims that "a human being can feel emotionally and think discursively independently of the physical body" (1977, p.7), and it is clear from his argument that this separation is to be thought of as an actually occurring one, and not simply a bare conceptual possibility. From the cases that he cites, however, it is clear that Leggett's witnesses were not just feeling and thinking - they were also seeing (it is a commonplace feature of OBE reports that they include some reference to standing apart from or looking down upon "my body"). The Cartesian picture of mind as unextended thinking substance has been
illegitimately expanded here to include such blatantly physical features as spatial location and perceptual capacities.

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There is one further question concerning survival which I must consider, and that is the status of Broad's basic limiting principle concerning survival, which I mentioned briefly in the previous chapter. In most contexts, I think we can agree with Broad when he says that we tacitly accept the following: "when a person's body dies, the personal consciousness ... either ceases altogether, or, if not, ceases to be able to manifest itself in any way to those still living on earth" (1962, p.4). Broad, in framing his BLPs, makes it plain that he is considering the tenets that are held in Western industrial societies, and which form the framework for our practical and scientific activities. For those purposes, the principle is tacitly accepted: the question of survival is not even raised in such contexts. Nonetheless, there are contexts where it occurs, and which have nothing to do with psychical research (Broad, it will be recalled, uses the BLPs to specify the subject matter of psychical research: if something conflicts with a BLP, then it is ostensibly paranormal).

During the daily round - boiling a breakfast egg, replying to letters, or driving home from work - the survival question does not impinge at all. But if we take ourselves to church on a Sunday, or become enmeshed in a mid-week discussion on theology, then the matter of survival suddenly snaps into sharp focus. In such contexts, the issues are aired with a directness that cuts across the BLP completely. And yet Broad
takes this principle to be fundamental to our conceptual scheme. Can it be then that the vicar whose sermon invokes the joys of the Kingdom of Heaven is indulging in self-contradiction? Or even, as Broad seems to be claiming, that he is giving a lecture on psychical research? The majority of theologians would, I think, deny both interpretations. Religious language occupies a special place in our conceptual scheme, and has its own particular rules. Broad is making the assumption that language has but a single function, one that can be characterized by drawing a boundary of limiting principles around it. But this is to take a procrustean view of the uses of language: although survival has no place in scientific discourse, it is central to eschatology.

It should also be said that Broad's principle is less than illuminating: we either, he says, do not survive, or we do survive (but are unable to communicate the fact to those on earth). As far as we on earth are concerned, there is no way of deciding between the alternatives. The difference in their implications is, however, enormous. Broad's principle boils down to the uninteresting claim that either we survive or we do not; it would be hard to disagree.

Scriven (1956) makes the interesting point that, no matter how sophisticated our explanatory schemata might become, there will always be certain questions that will resist a full and satisfactory explanation. This category he refers to as the "supernatural"; the origin of the universe, the evolution of mankind, and the survival of death are all ultimate questions that fall under such a heading. We can probe them, but firm conclusions will always elude us. In the case of survival, part of the problem lies not in the absence
of evidence, but in the difficulty of deciding what is to count as evidence. Bishop Butler said that the question of survival, "as it is the most important question which can possibly be asked, so it is the most intelligible one which can be expressed in language" (quoted in Flew, 1953, p.62). But, when we consider how we might set about answering it, it becomes obvious that the question is far from clear.

Certainly, as far as experimental parapsychology is concerned, there is no need to import any assumptions concerning survival in order to construct a paranormal account of the results. By stripping away untestable explanatory hypotheses, psychical research can move closer to a scientific position. Survival may well take place, but psychical research cannot prove it, and nor does it need to assume it.
There are certain epistemological issues which have an important bearing on psychical research. If ESP is a form of perception, and if the results of its operation can properly be said to give us knowledge, then the importance of psychical research can scarcely be overstated. If, on the other hand, no act of perceiving takes place, no information is acquired, and the results amount to no more than a series of striking coincidences, then psychical research is in no position to offer any epistemological insights. In this chapter, I shall examine these matters, concentrating on experimental cases of ESP (PK, having no obvious epistemic component, will not be discussed). I shall argue that the answer to both questions (concerning perception and knowledge) is negative, and provide an alternative characterization of the experimental findings: one that, although it is not without problems, takes the available evidence as far as it can go.

The question at issue here can be stated as follows: "if I am participating in a card-guessing experiment, and the current target card is a circle, and moreover I guess that it is a circle, then can I legitimately claim that I (in some way) perceived that it was a circle, and that this perception entitled me to enter a claim to knowledge?" The question of what we might term, as a convenient shorthand, "paranormal knowledge" is a significant feature in the literature. Broad, in
his basic limiting principles, takes it for granted, as does Ayer, though from a different perspective (1956, p.187). Among parapsychologists, Eysenck (1958, p.106), Rhine (1948, p.24), and Thouless (1972, p.32) all accept it. The idea that knowledge is central to the question is a pervasive one, and talk of knowledge and information transfer is a consequence of talk of perception.

Broad's BLPs make a natural starting point for this discussion. In the two formulations, he offers a total of four principles:
A. "It is impossible for a person to perceive a physical event or a material thing except by means of sensations which that event or thing produces in his mind".
B. "We take it for granted that a person A cannot know what other experiences another person B is now having or has had", except by certain well-known methods.
C. "It is impossible for a person to know ... that an event ... happened at such and such a place and time", except by certain well-known methods.
D. "We take it for granted ... that a person cannot foresee ... any event which has not yet happened".
(Conflated from Broad, 1953, pp.10-12, and 1962, pp.3-4).

It is striking that Broad accepts that both perception and knowledge are involved here. It will be recalled that he posits a negative definition of paranormality: if something conflicts with a BLP, then the conflict is to be taken as the criterion of its (ostensible) paranormality. So by setting up his epistemological principles in terms of perception and knowledge, Broad is inescapably committing himself to the view that a correct guess in an experiment is to be interpreted as
a case of knowing. Thus Broad begs the crucial question: "is it legitimate to speak of acquiring knowledge by paranormal means?"

Broad, in sentences that I have, for the sake of brevity, not quoted, offers a number of specific methods of having or acquiring knowledge, such as personal awareness, memory, inference, and so on. But are clairvoyance and telepathy also to be interpreted as methods that people can employ, or are they merely names that we give to odd coincidences?

In an ESP experiment using Zener cards, there will be twenty five trials for each shuffle. The results will be purely statistical, of course, and as such, they will require interpretation. Simply to say that this subject scored significantly above the mean chance expectation in a long series will not be sufficient. An extra premiss must be added to the argument: "such a deviation cannot reasonably be attributed to normal processes, but must have been brought about paranormally". The subject, it is implied, is doing something in the course of the experiment. Now if this were the case, then the subjects should be able to reflect on their performances. If they have access to information by paranormal perceptions, then we should expect this reflexion to bring forth a sense of confidence, of sureness about the results of particular guesses. But this rarely happens - there is still no way of telling the five out of twenty five "ordinary guesses" from the two or three "paranormal" ones. As I have said in Chapter Four, psi occurs without conscious awareness. H.H. Price is, I think, partly correct when he says that "the experience on his [the subject's] side seems to be exactly the same both when he is right and when he is wrong. If he were knowing in the correct
cases and only guessing in the incorrect ones, there ought to be an experienced difference between the two" (Wheatley, 1976, p.116). I say "partly correct" here, because the cases that might involve knowing would actually be of two kinds: the exciting paranormal ones, and the uninteresting lucky guesses. The wrong ones would be, not guesses, but unlucky guesses.

The use of perceptual terminology in the context of ESP or psi gamma must be taken to be purely metaphorical. There is no sensory organ which can be said to be the recipient of ESP, and there is no conscious awareness on the part of experimental subjects of the operation of any "sixth sense". The cards are turned, the guesses are recorded, and that is all that happens: there is no causally efficacious mediation between the two operations. No one perceives anything, they simply record their guesses. It is only when the statistical analysis has been carried out that any serious talk of paranormality appears.

So if the subjects cannot be said to be perceiving, can they lay any claim to have knowledge of the target cards? Of course, not all knowledge depends upon direct perception: we know that Caesar conquered England in 55 B.C., although there is no one alive who could be called as a witness; we can also be said acquire knowledge by inference or by induction. But in the psi cases, there seems to be no link between the target and the guess. The scores may be so high as to be puzzling, and may thus be thought to call for an explanation, but it is illegitimate to assume that any such explanation could be couched in terms of knowledge.

The classic analysis of knowledge is the Justified True Belief theory (JTB), which dates back to Plato. On this
account, a claim to have knowledge is analysable into three components: a person A can claim to know proposition P if and only if (i) P is true, (ii) A believes that P, and (iii) A is justified in believing that P. As Gettier notes (Phillips Griffiths, 1967, p.144), recent formulations of JTB have offered slightly different components, while retaining the essential structure. Thus Chisholm substitutes "accepts" for "believes" in (ii), and demands "adequate evidence" rather than "justification" in (iii). Ayer asks that A should be "sure that P is true", rather than merely believing in (ii), and moreover "has the right to be sure" for (iii). Whichever version we might prefer, it is clear that JTB does not admit paranormal knowledge claims. It is clear that criterion (i), that of truth, is met: the target card is a circle. However, the subject is not required to believe this; he may do, he may not. He may be asked to note those guesses where he felt particularly confident, and he may do rather well on these, but there is still a large gulf between "guessing confidently" and "believing", and there cannot, in such cases, be any question of justification. Subjects do not in general report any kind of imagery associated with their guessing: they do not see a circle or a letter C before their mind's eye, nor do they hear an inner voice whispering the word "circle". They just guess, and record their choice in the prescribed manner. A guess can be right or wrong, but it cannot be justified, since if it had a genuine justification, it would stand higher in any epistemic scale that we could construct.

Even if we were to adopt the versions of JTB offered by Chisholm and Ayer, the case for paranormal knowledge claims could not be made out. Chisholm's preference for "A accepts P" over "A believes that P" certainly might
accommodate paranormal guessing, but his demand for "adequate evidence" could not be met. There cannot be adequate - or, indeed, any - evidence in such cases: that is what makes them so noteworthy. Ayer's stress on "being sure" is also incompatible with any paranormal knowledge claim. If we accept JTB, in any version, then whatever might be going on in a parapsychology experiment cannot be referred to as being a case of knowledge.

Other currently-held views on knowledge would similarly exclude paranormal knowledge claims. Austin's performative theory (1970, pp.76-116) offers an analysis of knowledge claims: "when I say 'I know', I give others my word: I give others my authority for saying 'S is P'" (p.99). Of course, in the parapsychology case, the experimental subject is in no position to give any such assurances. The bulk of such subjects get vastly more wrong calls than right ones, and simply because someone scores seven instead of five cards in an experiment, it is quite unreasonable to suppose that this entitles them to give their warrant about the identity of the next card to appear.

Paranormal knowledge claims would also fail on Goldman's causal theory (1967), which requires that facts be causally linked to beliefs. Although psychical research sometimes makes a show of pretending to establish causal links, its view of causality is ambiguous. I shall explore this matter in more detail in the next chapter, but I shall note for the present that there are no demonstrable causal chains to be found in psychical research, and so any attempt to analyse knowledge claims using Goldman's theory is doomed from the outset.
I conclude that paranormal occurrences such as telepathy and clairvoyance fail to give us any right to claim to have knowledge. Attempts to introduce notions such as "confidence" or "conviction" should also be regarded as suspect, since there are no rational grounds for the choices made (see Thouless, 1972, p.38), and any confidence that a subject reports in a particular call amounts to nothing more than a mere hunch.

Can we give a less controversial description of these experimental cases? Parapsychologists seem too adventurous in claiming that the psi hypothesis is the only serious explanatory candidate. First, because the psi hypothesis does not actually explain anything at all, it merely asserts that something peculiar takes place that can sometimes have rather odd results. Secondly, because to claim that a number of anomalous results in card-guessing experiments must be a demonstration of ESP is to jump to conclusions. As Ayer has put it: "to talk of extrasensory perception is not to give any explanation of the subject's performance but merely to stake the claim that an explanation is called for" (1965, p.50). Much the same position was earlier taken by Robinson (Ludwig, 1978, p.81): "to call a coincidence a case of telepathy is not to explain it, but precisely to deny the possibility of one sort of explanation of it". Against this view, Scriven (Thakur, 1976, p.193) says that, in certain contexts, it is "perfectly appropriate for somebody to offer as an explanation of a puzzling phenomenon the hypothesis that it is due to telepathy; by this he or she means to convey the fact that transfer of information is occurring other than by the typical means, and the existence of this information in the mind of some individual associated with the
experimenter is a necessary condition for success". I do not think that Scriven's formula will work: although he refers to Robinson's argument as an "attempt to dispose of the supernatural by a piece of logical legerdemain", he does in fact do little more than reproduce what Robinson had said a quarter of a century before: to "explain" something as a case of telepathy is simply to exclude "normal" explanations. All Scriven can offer is a claim that the telepathy hypothesis has "plenty of meat" in it, "meat that can be tested. For example, it denies that ordinary sensory transfer is occurring, and it denies that clairvoyance would be an adequate explanation of the events". The claim that the telepathy hypothesis is open to testing must be regarded as being extremely doubtful, and the possibility of differentiating it from clairvoyance is small.

What is needed is a description of ESP which is testable and also lacking in tendentiousness. The best account is, I would suggest, to be couched in terms of guessing or intuiting: experiments in so-called ESP demonstrate that some people are better at guessing than others. We need not be surprised at this: the experiments may have been designed to demonstrate the reality of ESP, but if so, they were designed remarkably badly. The experiments are simply guessing tests, and subjects are invited to guess the target cards, not to "have a paranormal experience", or to "concentrate their psychic powers", or to "employ clairvoyance". The experiments have proved just what the design apparently set out to test - that some people are better at guessing than we should expect them to be. A similar interpretation can be applied to cases of precognition, which can be glossed as examples of guessing what is going to happen next, and PK could
be thought of in terms of luck: some people are luckier
(with dice, or whatever) than we should expect them to
be.

This guessing hypothesis is more familiar, more down to
earth, than the flat claim that some people, some of
the time can obtain information through unknown
channels. It serves to bring the results within our
conceptual reach. Explanations must come to an end at
some point, and philosophical clarity is served by
posing an explanation that falls within our reach
(guessing) rather than one that falls outside it (ESP).
Such an explanation is also more economical (though
this point should not be regarded as being conclusive).

There is, to echo Scriven, plenty of meat in this
hypothesis, meat that can be tested: for example, it
denies that all people guess in line with mean chance
expectation, and offers psychologists a potentially
fruitful opportunity to investigate a relatively
unexplored human activity. Moreover, it avoids the
problem, remarked upon by Flew (1953, pp.117-118), of
how we are to distinguish between the five "ordinary"
guesses and the two or three "paranormal" ones: we
cannot, because they are all of the same kind. To this
extent, the guessing hypothesis has greater explanatory
force than any rival alternatives.

The attempt to bring the experimental results under the
descriptions of "guessing" and "luck" is not a new
programme: Spencer Brown (1957, p.112) mentions it
obliquely, and Flew (1953, p.130) offers guessing "as a
convenient stopgap way of thinking of the phenomena":
"if we must have a model, in terms of which to think of
experimental psi-gamma and to try to make it
intelligible to ourselves, then the model of guessing
would be a great deal better than those of perception, communication by radio, or the fabulous onstage activities of ghostly minds or even - if these can be called models - those of cognition (jargon for 'knowing') or thought-transference. Flew is right to rail against the popularly-held but philosophically unsound pictures that are sometimes put forward. However, his proposal of guessing as a model for understanding ESP seems unhelpful: the experiments are, after all, just card-guessing trials, and the significant results are merely anomalies in guessing behaviour. As such, it is less than illuminating to propose a model of them that is couched in terms of guessing. We do not gain a grip on the concept of guessing by proposing to understand it in terms of guessing.

Ayer states bluntly: "the only thing that is remarkable about the subject who is credited with extrasensory perception is that he is consistently rather better at guessing cards than the ordinary run of people have shown themselves to be" (1965, p.61). However, guessing and luck have rarely been taken seriously enough by the psychical researchers. If the subjects have no rational grounds, as Thouless has said (1972, p.38), then surely they are just guessing, but Thouless is unwilling to take such a common-sense solution. If the cases of so-called ESP were something more than mere guessing (and of course there is a dark suggestion that they are), then the subjects should have some grounds for their choices. As they do not, I conclude that the guessing hypothesis, while it is not without its difficulties, offers a better picture of the experimental findings than any of the so-called explanations that are couched in paranormal terms. It avoids any tendentious talk of "perception" or of "knowledge", and says no more and no
less about the experimental results than can legitimately be asserted.
Causality raises two important questions for psychical research. First, in cases that are assumed to involve ESP or PK, is it legitimate for us to introduce the idea of causation into our descriptions of the events and processes that we might assume to take place? Secondly, if a causal account of such events can be supplied, then how are we to deal with those cases that seemingly involve some kind of temporal displacement (as in precognition)? Any causal account of these will have to involve the difficult notion of backwards causation.

If, however, it can be shown that a causal account cannot be made available, then psychical research will face some testing difficulties: all it could offer would be a series of bare experimental correlations, linked together on little more than the basis of post hoc, ergo propter hoc, and, if we allow backwards causation, ante hoc, ergo propter hoc.

Broad, in his list of the basic limiting principles, puts forward three concerning causality. However, these only appear in the earlier version (1953, p.9). The later version (1969) makes no reference to causes, an omission which may not be without significance. The difficulties that stand in the way of providing even a remotely plausible causal account have often been remarked upon.
If we consider a typical card-guessing experiment, then the events that are observed might be characterized under two heads: first, there is someone who looks at cards, one by one, and concentrates on the symbols displayed; secondly, there is another person who is recording her guesses. If we find that the two sets of record sheets (showing targets and guesses) coincide to a degree that seems to us to lie outside the mean chance expectation, then we feel tempted to posit a relationship between the two classes of event. The habits of our thinking may push us further, towards making a tacit presumption that this relationship might be characterized in causal terms. However, such a move would be illegitimate: psychical research would have to utilize devices such as concomitant variation, in which experiments are repeated, varying each significant factor in turn, and observing the resulting differences in the experimental outcome. Psychical research is sometimes criticized for failing to do this, but as I shall demonstrate in Part Three, it is incapable of carrying out such tests. All we are offered is the suggestion that there ought to be some sort of relationship - for if there is no relationship, then there can be no point in investigating mere coincidences. Although he admits that he is unable to find any causal connexion, Rhine says: "one will naturally continue trying to think about psi phenomena in terms of causation" (1954, p.70). Of course, there is a considerable gap between trying to think of them in that way, and actually demonstrating it.

Psychical research thus finds itself faced with three options with regard to the question of causation: (1) the relationship that we assumed to hold between the two classes of event is an illusory one, and
correlation between the two sets of records is an artefact, a "co-incidence"; (2) a relationship does hold, and it is moreover causal. However, we cannot, as yet, give a satisfactory account of it until further work on paranormal phenomena has been carried out; (3) a relationship holds, but not a causal one. I have in mind here such proposals as Jung's synchronicity (1972) - which he describes as "an acausal connecting principle", or Kammerer's "law of series" (see Koestler, 1971). Both these notions (they scarcely deserve to be dignified with the titles of "principles" or "laws") depend upon the presumption of the existence of "meaningful coincidences". It might be suggested that there is a fourth possibility: that paranormal events are subject to causal laws, but that the laws in question are of a "higher order": paracausal, rather than merely causal. But it is hard to see any future in such a proposal, since the cashability of "paracausation" must be seriously doubted.

Braude, in a detailed critique (1979, pp.217-241), has effectively disposed of Jung's synchronicity, and even Koestler does not pretend that Kammerer's work is much more than an interesting curiosity. Part of the problem is that the idea of a coincidence (a meaningful one, as opposed to a mere one) is difficult to pin down. There are no rules here; anything we choose might be allowed. If something strikes us as being interesting, we might add it to our collection of oddities. Thus, to adapt the kind of case that Kammerer discusses, suppose that I go to a recital at which Beethoven's Opus 132 Quartet is the main work on the programme. I am to sit in Row I, Seat 32, and my cloakroom ticket is number 132. So far, I think Kammerer might have been impressed. But it
does not stop there, for I take a cab home, and note that its licence number is 123. Now am I allowed to count that as another hit, or a near miss, or is it just ... another number? I have no way of determining the answer to this question.

The difficulty is not unlike that raised by many popular superstitions. We are often told, for example, that it is "bad luck" to walk under a ladder, or to spill salt. Even worse, to break a mirror will bring down seven years of bad luck on our heads. The consequences sound dire, but how are we to recognize them, should they occur? It is easy to pin any misfortune on "bad luck", but if my lawn mower breaks down next Spring after ten years of trouble-free operation, it may well be due to obsolescence and my reluctance to have it regularly serviced, rather than my penchant for walking under ladders whenever they cross my path. As there are no rules for making an interpretation, and no method for demonstrating links between two unrelated incidents (save of course my own private predilections) we can dispense with option (3).

Option (1) is, of course, without specific paranormal interest, so psychical research is left with (2). What problems lie in the way of providing a causal account of paranormal phenomena? A necessary condition for positing the existence of a causal relationship between two events is that they shall be constantly conjoined (whether that is a sufficient condition is not relevant to the present discussion). In paranormal cases, there is no constancy in the conjunction. Such events as take place are perceived as irregularities in the normal pattern (seven correct guesses, rather than five...), and it is this irregularity that makes them noteworthy.
Although we have talk of transferring energy and information (which certainly sound like causally mediated processes), we can only measure the extent of the transfer indirectly: nothing binds the two sides of the supposed transaction together. One participant has the information, and the other is supposed to acquire it. But is it transferred from one to the other? No, it simply "appears". If psi occurs, then it seems to operate without any kind of means, and psychical researchers, while still thinking of psi as being essentially causal, have failed to offer any convincing explanation of its modus operandi. In the absence of any means, such as a bodily organ to control it, psi is just the name that is given to some rather odd statistics. This is not causation; it is more like magic - it happens on what might be called the "Abracadabra principle".

What of the cases involving temporal displacements, such as precognition? Of course, any causal description of these is going to have to take account of backwards causation. As Brier puts it (1974, p.xi): "of the three kinds of ESP, precognition is the one which raises most of the philosophical questions. ... How might we explain precognition? This is a different case [from clairvoyance or telepathy]. We find it difficult if not impossible to imagine waves that go into the future and return to the present bearing information about where (and when) they have been. How can events in the future, events which do not yet exist, make their presence known to those living in the present?" The problem only exists because we are tempted to think of psi - as we think of most events and processes - in causal terms. Brier's aim is to vindicate precognition;
as he notes, some sceptics have based their rejection of precognition on the impossibility of backwards causation. If, however, it can be shown that backwards causation is not an incoherent concept, then such an objection would be confounded. But Brier's programme will only go part of the way: it would not, of itself, show that precognition is possible, still less that it happens, but merely that one argument against it would have to be discarded.

There is an extensive literature on backwards causation, which Brier has comprehensively reviewed. I shall not recapitulate the main points here. I shall only say that in the absence of a satisfactory causal account of non-temporally displaced psi, consideration of even more intractable areas seems premature.

There are special problems encountered in the cases where a temporal displacement is inferred. First, why should it be that sometimes the effect occurs (as near as can be judged) simultaneously, and in other cases, there is a temporal gap (either forwards or backwards)? What governs this apparently arbitrary difference? Secondly, how are we to specify the range of operation of these displacements? Experimental demonstrations tend to concentrate on quite small displacements between call and target, typically of the order of plus or minus one or two (as seen in the famous - though now discredited - work by Soal on Basil Shackleton). But what of much wider displacements, say of the order of minus 478, or plus 253? Small displacements are relatively easy to detect, measure, and assess; large ones have so many difficulties in the way of their discovery that it is not surprising that they have been neglected.
Thirdly, these displacements need not be only temporal: they could also be spatial. It is possible to imagine that a subject in Beloff's Edinburgh laboratory in 1980 was scoring only in line with mean chance expectation on the targets that Beloff had prepared, but was using "postcognition" to score very highly on a series of targets that Rhine had prepared at Duke in 1930. Because of obvious practical considerations, such possibilities can never be adequately investigated, but that does not mean that psychical researchers can dismiss them out of hand as being idle flights of fancy. The problem in the way of a causal account here is that we simply have two events, separate in both time and space, and with nothing to bind them together.

There are also displacements reported in the sporadic cases, and here, supplying a causal account is just as difficult. First, we have to untangle straightforward coincidences from not only ostensibly precognitive events, but also from predictions involving rational inference (a means which would be excluded in the card-guessing experiments). The method of counting hits by reference to probability does not apply in sporadic cases, for there is no way to assess the odds against chance. To calculate the probability of a particular event, we should have to slice up the universe into a series of discrete events, and we have no commonly agreed formula that will allow us to do this. How many events take place in a day? The answer to this question depends on a metaphysical stipulation of what is to count as a single event, and no formula for determining it is logically prior to any other (see Braude, 1979, p.230 for an excellent discussion of this problem).

Secondly, even if we could arrive at a generally agreed formula, how wide is the temporal gap that we are
prepared to tolerate between the precognitive warning and the event itself? Here, the situation is the reverse of that which we find in the experimental cases; there, short gaps have attracted the attention (plus one or two). In sporadic cases, a very much larger temporal gap is demanded, since this is the only way to preclude rational inference. Thus there is something very noteworthy in a prediction made in 1879 that by 1979, the United Kingdom would have a woman Prime Minister named Thatcher, whereas such a prediction made on the eve of the 1979 general election is parapsychologically uninteresting. So by allowing, in the sporadic cases, that a prediction made over a long period is more impressive than one made over a short period (assuming that they both turn out to be true) we effectively rule out the possibility of determining the odds against chance until the last trump has sounded. And when we try to assess the possibility that a case is mere coincidence, then the probability method of the parapsychologist's laboratory again fails us, for the longer a temporal gap we allow, then the greater is the possibility of a striking coincidence occurring.

G.G. Taylor (1983), in a sceptical analysis of precognition, opts for a definition of precognitive cases which allows for description (but not explanation). That description is to be couched in terms of coincidence. So-called precognition is then conceived of in non-causal terms. Taylor seems to me to be right in refusing to allow causal descriptions, not only because, as he says, the notion of backwards causation is incoherent, but also because a proper causal account of non-displaced psi occurrences has not been provided.
Even if it could, it might turn out to be less than interesting: causal explanations offer a rich texture of interconnexions; events mesh in with other events: one thing, as the saying goes, leads to another, and we can trace the binding threads. Backwards causation, on the other hand seems an impoverished thing; all we are offered is a single instance of a supposed causal relationship. There are no backwards causal chains that stretch from the future to the past, only one-off occurrences that conveniently save the appearances (see Braude, 1986).

If we wished to attempt a causal description, it would have to look something like this: imagine a card-guessing experiment, which yields plus-two displacements. We can describe this in two ways, either my guess at time t causes the target at t+2 to be in accord, or the target at t+2 causes my guess to be in accord. The former explanation is in terms of forwards causation, the latter of backwards causation. How are we to choose? It could be argued that the card is already there in the pack, and my guessing is not going to make it change places, so we should prefer the backwards version (though if we change the "randomizing equipment" from Zener cards to a console of five coloured flashing lights, then such an objection would lose its force).

But though there is a show of causal talk here, there is no serious account of what actually happens. In sporadic cases, causal explanation is just as difficult. One might imagine a case where I dream of an enormous volcanic eruption, in which many people are killed. It is usually claimed that a forwards causal account (in which my dream causes the eruption) would be counter-intuitive, and so a backwards account should
be offered (in which the eruption in the future can, even before it has come about, cause my dream). But psychical research, by its very tenets, cannot permit itself the luxury of allowing its thinking to be conditioned by mere assumptions about what seems reasonable. It is not without interest to note that Braude, in his earlier book (1979, p.32) helps himself to the notions of reasonableness and plausibility to argue in favour of the highly unreasonable and unplausible notion of retroactive PK. (Though it should be noted that in his more recent work (1986, ch.5) he has had second thoughts).
PART THREE

PSYCHICAL RESEARCH: SCIENCE OR PSEUDOSCIENCE?
CHAPTER EIGHT

SPONTANEOUS CASES

In Part Three, I shall examine the crucially important question: is psychical research a genuinely scientific enterprise? I shall approach this question in three stages, looking at spontaneous, observational, and experimental cases in turn. I shall argue that none of these can support the presumption of scientific status. As we have seen, psychical research makes some very large claims for the phenomena which it studies. I have shown that these claims, so enthusiastically entered, are not sufficiently made out: the dualist picture of the human being is filled with paradox and pitfalls; the question of survival of death is not to be taken as a straightforward empirical one, merely requiring careful sifting of already established evidence; psi does not give us knowledge or information, and, if it happens in any meaningful sense at all, does so without causal mediation. Moreover, psi seems to happen without any conscious awareness on the part of those participating in experiments. Given these difficulties, can we say that psychical research is a science?

With regard to the spontaneous cases, there are five particular problems that stand in the way. First, if psychical research is to make a bid for scientific credibility, then there are good reasons for thinking that the spontaneous cases should not occupy too great a part within it. Scientific disciplines rely on theory and experiment. Spontaneous cases, as their name suggests, are extremely difficult to tackle
scientifically, since they are unique occurrences, and do not obviously admit of any generalized interpretation. A good story can be a fascinating thing, but no science can subsist for long in mere tales and curiosities. If odd, unexplained phenomena were to be observed, then scientists might well note them and pursue them. But gradually, as knowledge is gained, controlled experimental results supplant the purely anecdotal stuff, which, while it might offer provocative insights to anthropologists and social psychologists, remains at the pre- or sub-scientific level.

Secondly, I should imagine that most psychical researchers would agree with my last point, and say that spontaneous cases are best understood as suggestive pointers, indicating potentially fruitful areas for developing hypotheses. Such hypotheses could then be subject to experimental testing (see, for example, Thouless, 1972, pp.16-17). The crucial idea here is experimental testing. Too much "theoretical" writing in psychical research is little more than romantic speculation of the most untramelled kind (see Heywood, 1978, pp.201-221 for a review of some typical examples). By now, though, experimental parapsychology has generated its own momentum, and its programme should be capable of standing alone, without having to rely on spontaneous cases.

Thirdly, as many investigators in this field have found, eyewitness testimony is frequently a very unreliable source of information. Psychologists and criminologists are well aware of the startling discrepancies which appear when the evidence from different eyewitnesses is collated and compared (see Loftus, 1979 for an overview of the current state of
knowledge). The classic experiment in this field is that of Allport (1947), in which a lecture is dramatically interrupted, and the students are then asked to write up their recollections of it. But psychical researchers - most notably Hodgson (1887) and Besterman (1932) - have made original experimental contributions to the field. Clearly there are circumstances when we have to accept eyewitness testimony - to reject it altogether would condemn us to solipsism. But our acceptance should be tempered by careful, critical sifting, ready to detect any discrepancy or self-deception.

The value of eyewitness testimony varies, however, according to the kind of event that is described. Let us assume that there are three types of event: normal, abnormal, and paranormal. In normal cases, which involve no unusual circumstances, eyewitness testimony can, provided due account is taken of the vagaries of memory, be accepted as broadly reliable. Abnormal cases - those which present us with totally unexpected situations, such as a violently interrupted lecture or a bank robbery, and which occur so swiftly that we do not have time to re-orientate ourselves to the new circumstances - offer special problems. Here, the testimony of different witnesses can differ widely. Paranormal events are even more problematic, for here, there is no normal standard that we can re-orientate ourselves to. If one were, for example, to be a victim of an aircraft hijack - an abnormal occurrence - one's recollection of the first few minutes might be very confused indeed. After that, one might begin to adjust, to take proper note of persons and events, so that, after the ordeal, one was in a position to give a thorough, accurate description of what transpired. In supposedly paranormal cases (such as "seeing a ghost"),
however, there is no opportunity for adjustment to the normal.

The Society for Psychical Research, it must be said, fell victim from its very beginnings to a series not only of frauds and hoaxes but also honest but mistaken reports. The Society seemed to believe that social standing offered a satisfactory guarantee of an informant's veracity, as the Hornby case demonstrates. Judge Hornby's apparently paranormal experience (first published in the SPR's Proceedings Volume two, 1884) was — to the discredit of the Society — suppressed when the volume was reprinted because it had been shown to be utterly unreliable in all its significant circumstantial details (a full account may be found in Hall, 1980, pp.65-68). The learned judge's personal and professional credentials were impeccable, but his story could not withstand even elementary critical scrutiny. This was no isolated instance: in 1886, Gurney, Myers, and Podmore published (under the auspices of the SPR) a collection of first-hand accounts of "phantasms of the living". Its reception in the contemporary press was less than cordial (see Hall, 1980, pp.72-78 for a sample. It is noteworthy that C.S. Peirce is listed among the critics). The officers of the SPR were accused of having been indefensibly lax in the matter of securing documentary verification of the reports that they had published.

Fourthly, even if we feel inclined to accept that many of the reports of spontaneous cases are reliable, it has to be said that they present severe problems of interpretation. There are two particular faults to which these accounts are prone. First, there is a tendency to "overdescribe", to report things in such a way as to beg the very question at issue: "is this
phenomenon paranormal?" There is a natural human urge to tell a good story, and many spontaneous cases exemplify this strongly. For example, in the notorious case of the haunting of Borley Rectory, Harry Price wrote that the belt of a coat worn by a visitor was "lifted and dropped again by an unseen hand" (quoted in Dingwall, 1956, p.3). The presentation signals clearly to us that a paranormal interpretation is intended. But compare that statement with "I noticed that my belt caught on something for a moment". Phenomenologically, the two versions are interchangeable, but how bald, flat, and unsensational the latter is, when compared with the former. Note the way that Price nudges us in the direction that he wishes us to go: it was a hand, and not a door knob or a nail, say, that caught up the belt, and an unseen one at that. But was it unseen because it was genuinely invisible, or simply because nobody happened to look for it? Notice too the subtle insinuation of intentionality in that use of the word "lifted". I shall not labour the point; Price's choice of words disposes us to view the events in a certain light. An interpretation is being foisted on us, and this kind of exaggeration is not uncommon in accounts of spontaneous cases.

Secondly, many reports are severely "underdescribed": there is not sufficient detail given to allow us to make a serious critical evaluation. Take this case, culled from Volume 33 of the SPR's Proceedings by Tyrrell (1947, pp.63-64):

The percipient was married and lived in India [wrote Tyrrell]. Her half-brother, Edward W. Bowyer-Bower, was an officer in the R.F.C. In the early morning of 19th March 1917, he was shot down and killed in France. "My brother" she [the witness] says, "appeared to me on the 19th March, 1917. At the time I was either sewing or talking to my baby - I cannot remember quite what I was doing at that moment." The
baby was on the bed. I had a very strong feeling that I must turn round; on doing so I saw my brother, Edward W. Bowyer-Bower. Thinking he was alive and had been sent out to India, I was simply delighted to see him and turned round quickly to put baby in a safe place on the bed, so that I could go on talking to my brother, then turned again and put my hand out to him, when I found he was not there. I thought he was only joking, so I called him and looked everywhere I could think of looking. It was only when I could not find him that I became very frightened and the awful fear that he might be dead [sic]. I think it was two o'clock the baby was christened and in the church I felt he was there, but I could not see him. Two weeks later I saw in the paper he was missing yet I could not bring myself to believe he had passed away." Allowing for the difference of time, the vision coincided very nearly with the time of the accident.

I have quoted this case in detail to demonstrate the difficulties involved in understanding precisely what happened. To begin with, we need to know the time of day. "The early morning" is not nearly precise enough: was it light or dark? Why cannot the witness remember what she was about at the critical moment? Perhaps it was very early in the morning; perhaps the baby had woken her; perhaps she was waiting for it to go back to sleep, and occupying her time in sewing ... a single candle burns, she is drowsy, and thinks, in a moment of near sleep, that her brother is in the room with her ... Perhaps it was like that, perhaps it was not. We do not know - we cannot know - on the basis of her account. The witness baldly asserts: "I saw my brother", but was he sitting, standing, pacing the room? How did he look: happy, wretched, in pain? How did she recognize him? What was he wearing? (If she saw an "astral body" was it wearing "astral uniform"?) Did the apparition speak, and if so, what did it say? (Note that the sister wished to "go on talking"). There are many more questions that we need to ask before we can
offer even a tentative assessment, but all we are given is the sketchiest outline. This case is not untypical.

Even more underdescribed is an incident reported to the SPR by Misses H.M. and L. Bourne. It was first published in the Journal (Volume six), and was subsequently reprinted by both Myers (1903) and Tyrrell (1947). On the day in question, the Bourne family was out hunting. The two sisters saw their father some distance away, apparently in some distress, and signalling to them to join him. They rode over, losing sight of him because of a dip in the ground, and when they gained the spot where he had been, there was no one in sight. Their father, who had not, he said later, been in any kind of danger or distress, denied having been in that particular field, or having signalled to them. His horse was said to be unmistakable (see Myers, 1903, Volume one, pp.651-653). The case must be said to be mildly puzzling, but in her statement, Miss L. Bourne adds a further detail: "As my father waved his hat I clearly saw the Lincoln and Bennett mark inside [that is, the hatters' label] though from the distance we were apart it ought to be utterly impossible for me to have seen it. At the time I mentioned seeing the mark in the hat, though the strangeness of seeing it did not strike me till afterwards". What are we to make of this? What did Miss Bourne see? How are we to evaluate her testimony in the light of this curious detail?

An account which steered a middle course between both overdescription and underdescription would avoid any tendentious assumptions about paranormality. As an example, consider the Borley incident referred to above. A proper, neutral account would have given us the name of the visitor, the dates and times when the
incident occurred and when it was reported, particulars of any witnesses, and a simple statement of what was known to have happened: unseen hands should remain unseen. When couched in such terms, the case might well be considered unworthy of further enquiry.

Fifthly, if I seem to be labouring the point about the difficulties in accepting testimony in spontaneous cases, it is for a very good reason. There is a view, one more often implied than directly argued for, that science is, at bottom, nothing more than a collection of eyewitness reports, a series of observational records. (If it were, of course, then psychical research would have vindicated its claim to a place in the pantheon of science, since its collection of reports is vast.) This idea has its roots in the empiricist theory of knowledge and the Baconian conception of science, which emphasize the priority of experience and experiment over theory. But such a view is mistaken: science is not to be equated with mere fact-gathering, any more than lepidoptery is with butterfly collecting. Scientific observation and experiment are preconditioned by theoretical awareness: the observation fits the theory, rather than the other way round. The attempt to give scientific respectability to spontaneous cases fails because ultimately, science aims for generality, not particulars. As Medawar (1969, p. 128) has said:

The ballast of factual information so far from being just about to sink us, is growing daily less. The factual burden of a science varies inversely with its degree of maturity. As a science advances, particular facts are comprehended within, and therefore in a sense annihilated by, general statements of steadily increasing explanatory power and compass — whereupon the facts need no longer be known explicitly, i.e. spelled out and kept in mind. In all sciences we are being progressively relieved
of the burden of singular instances, the tyranny of the particular. We need no longer record the fall of every apple.

Medawar is, of course, describing the situation in mature sciences, and although by common consent psychical research could not claim such a status, his point clearly shows how any science must be more than the sum of its experimental results. In the absence of a theoretical standpoint, individual observations are nothing more than individual observations.

I have dealt in a sceptical fashion with the spontaneous cases but I should be dishonest if I did not add that I find many such reports fascinating. Here surely is the immediate source of our interest in the paranormal: it would be a shallow person who did not admit to a little tingle of excitement and wonder at some of these narratives. Even if all of them were to turn out to be nothing more than error, delusion, or fabrication, they would still offer the promise of provocative insights into the peculiarities of human psychology.
The observational cases are often supposed by psychical researchers to offer evidence for the paranormal that is superior in quality to that afforded by the spontaneous cases, yet beneath that of the experimental work. I shall discuss a number of particular problems regarding their interpretation, and the difficulties involved in assessing the feats of the medium Daniel Home. In the course of this discussion, I shall attempt to show that the observational cases, despite the claims that are made, offer no basis for establishing a science of psychical research.

In principle, observational cases are amenable to public demonstration. They are usually seen as offering direct, though fugitive, evidence for the paranormal. In this they may be contrasted with the spontaneous cases, which by definition occur sporadically and without warning, and the experimental cases, where the paranormality is subsequently inferred through statistical analysis. So it might be claimed that if Uri Geller could definitely be witnessed to bend spoons on demand, without using any physical methods, then the matter would be closed; sceptics would simply have to learn to adapt to the facts. Many sceptics take the view that it should be possible to arrange a straightforward demonstration of some paranormal event, such as spoon bending, with safeguards against cheating written into the protocols. The demonstration would be held before a committee of expert observers who would
deliver their verdict. It is vital to draw the rules very tightly, and to secure the assent of the psychic under test, otherwise subsequent failure can be too easily explained away. A number of magicians, among them Houdini, Randi, and Berglas, have offered large cash prizes to any psychic who can produce a paranormal phenomenon under properly controlled conditions. The problem with such demonstrations is that we have no commonly agreed standard of what "settling the matter" might amount to in such contexts. In the absence of any theory-based explanation, the phenomena are bizarre anomalies, and there are no guidelines to enable us to interpret and evaluate them. There have been instances where the experts have disagreed, such as the "Margery" case (see Mauskopf, 1980).

One reason that is often given for the failure of psychics to produce their phenomena is that the presence of sceptics inhibits their abilities (see, for example, Inglis in Roll, 1981, p.150). The sceptic, it is suggested, produces "negative vibrations" which interfere with psychic powers. However, in a BBC television programme (QED, broadcast on 8th March 1983), James Randi was shown masquerading under a heavy disguise and a pseudonym (Adam Jersin, an anagram of his stage name) as a volunteer from the audience at one of Geller's stage performances. The usual phenomena were punctilious in their attendance and there was no apparent inhibition (see Randi, 1983 for a written account). Nonetheless, the fugitive nature of the phenomena under properly controlled conditions has disturbing implications. John Taylor, in the period when he was (credulously) investigating the paranormal, noted that "this feature of [metal] bending not happening when the object is being watched - 'the shyness effect' - is very common" (1975, p.69). It did
not occur to him until several years later that there might be a simpler explanation for this: fraud coupled with inadequate observation (Taylor, 1980).

The introduction of paranormality as a possible explanation for certain peculiar events can generate more difficulties than it solves, as potential explanatory candidates can be allowed to proliferate unchecked. Without a theoretical perspective, we quickly find that "anything goes". For example, there are numerous poltergeist cases that have been attributed by sceptics to earth tremors - a vase falls from a shelf, and the psychical researcher claims this to be a "poltergeist manifestation". The sceptic on the other hand explains it away by reference to geological and seismic phenomena. Cornell, who has made a special study of the poltergeist (Gauld, 1979), has attempted to show that this sceptical view is inadequate. He found a condemned building and rigged a heavy duty vibrating machine to it. Vases were placed upon shelves within the building, but even when the mechanical vibrations exceeded anything that would be experienced in an earth tremor, they were not dislodged (this demonstration was televised on Arthur C. Clarke's ITV programme *World of strange powers*, broadcast on 10th May 1985. A written account may be found in Fairley, 1985, pp.45-46). Cornell no doubt thought that this disposed of the idea that tremors could be responsible for any poltergeist manifestations, the corollary being that a paranormal explanation would thereby be vindicated. But once we allow paranormality to be canvassed, the issue becomes confused, for it is possible to interpret Cornell's demonstration as anything but conclusive.
Let us imagine that most, even all, cases of mysteriously falling vases can be attributed to seismic disturbances, but that there are, as Cornell would like us to believe, also a few (a very few) poltergeists or mischievous spirits. It might be that one of these, anxious to maintain a reputation, and unfussy in the matter of choosing its victims, might have sabotaged Cornell's demonstration by holding on to the vases. The sceptic of course would regard such a suggestion as frivolous, but Cornell, once he has admitted the possibility of a paranormal explanation, cannot treat it so lightly. How, we might ask, does he propose to discover the truth of the matter?

In the absence of an observational demonstration combined with theory, these cases are of no higher quality than the spontaneous cases: both depend on eyewitness testimony, which is subject to the problems that I have discussed in the previous chapter. However, many of the witnesses have been trained scientists, and it has been implied that because such people are, so to speak, "skilled in the art of observation", their reports should carry especial weight. It is a form of *ad hominem* argument, which accords credit to the proponent, rather than, as is usually the case, reflecting adversely on the opponent. As Hyman notes (McConnell, 1981, pp.156-164), many distinguished scientists have given their personal testimony to the occurrence of paranormal phenomena, thinking perhaps to add scientific lustre to psychical research. But in the face of such mysterious events, the scientist is in no better case than the remainder of humanity - the language game of science does not yet stretch to accommodate such things. Indeed the scientist may labour under a disadvantage:
Any magician will tell you that scientists are the easiest persons in the world to fool. It is not hard to understand why. In their laboratories the equipment is just what it seems. There are no hidden mirrors or secret compartments or concealed magnets. If an assistant puts chemical \( A \) in a beaker he doesn't (usually) surreptitiously switch it for chemical \( B \). The thinking of a scientist is rational, based on a lifetime of experience with a rational world. But the methods of magic are irrational and totally outside a scientist's experience. (Gardner, 1983, p.92)

It has to be said that most, if not all, of the phenomena that are typical of the observational cases can be produced by magicians. This does not imply of course that because the product is similar, the process by which it is brought about must be the same. There is an argument that has become, in recent years, associated with the name of Randi, and which psychical researchers (with a measure of justification) attack. It runs something like this: "Geller can bend spoons, and claims he does it by paranormal means. I, Randi, can also bend spoons, but by legerdemain. You, the observer, cannot tell the difference between our two performances. Therefore Geller uses legerdemain, while pretending to have paranormal powers." As an application of the principle of economy, this argument is not without merit, but it cannot be regarded as conclusive. It is true that Randi (1980) believes that Geller, and many other psychics are fraudulent, but nowhere in his published writings does he put forward an unambiguous version of the argument that is sometimes foisted on him. In 1985, I discussed this very point with him at the London Conference of the Committee for the Scientific Investigation of Claims of the Paranormal, and he repudiated absolutely any such view. Randi's lesson is, I think, somewhat more subtle: Geller (to our astonishment) bends a spoon, and moreover claims to do it by using paranormal powers.
Randi bends a spoon, and admits that he uses sleight of hand. And we cannot tell the difference ... Now if Randi can fool us, should we not re-assess Geller, since we only have his word for it that his spoon was bent paranormally? The charge against Geller is a serious one, since if he is using legerdemain, then he is doubly dishonest - first in claiming that he does not employ conjuring tricks, and secondly in explicitly stating that he uses paranormal powers. Randi (1983a) urges psychical researchers to take great care in evaluating psychics, and to enlist the help of expert magicians in the judgement of observational phenomena.

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Although the question of fraud casts a long shadow over the claims that are made, the sceptic must eventually come to a consideration of the case of Daniel Home, since he is usually singled out as the one medium who was never detected in trickery. Beloff calls Home a "special case", and writes that "He, at any rate, was never discredited whatever his enemies may have said or thought about him and whatever reservations may have been voiced by later critics" (Wolman, 1977, p.7). The claim that Home was never exposed originated with Myers (see Podmore, 1902/1963, p.230), and was given currency by Podmore, who, though he had doubts, asserted that "Almost every professional medium has been detected in producing results by trickery. ... But Home ... forms an apparent exception. I am not aware that clear proof of imposture was ever brought forward against him" (1897, p.111).

After this great gap in time, a proper evaluation of Home's powers is not possible. From an historical point of view, the witnesses are all long dead, and from a
scientific point of view, the phenomena as described fail to connect with any current theory, and are thus not susceptible to a satisfying explanation (though a future breakthrough in theory could lead to a revaluation of Home's feats). However, because of Home's importance in the history of psychical research, I shall offer some notes of a sceptical nature.

Was Home genuine - the one good deed in the naughty world of the spiritualists? I cannot believe that he was. In the first place, is not Home's eminence in the field suspicious in itself? Let us examine, say, one thousand mediums; nine hundred and ninety nine are demonstrably fraudulent or subsequently confess to having used trickery. One alone remains - is he therefore genuine? We should beware of judging him as an isolated success: he should be seen in the context of the others who were found wanting.

Secondly, most of Home's repertoire consisted of effects that could be duplicated by magicians (and actually were, by Addison - see Wyndham, 1937, p.281). Podmore (1897, p.110) notes three phenomena - levitation, elongation of his body, and the handling of hot substances - which "must apparently be excepted from this generalisation". I shall discuss Home's alleged levitations shortly, but Podmore is wrong in saying that bodily elongation and handling hot coals cannot be performed by magicians. Both are part of the stock-in-trade of a number of them (see Christopher, 1971, pp.183-184, and Rawcliffe, 1952/1959, pp.291-296).

Further, some of Home's phenomena sound suspiciously easy to fake. To take just one example, his celebrated ability to play an accordion - held one-handed in a
wire cage - is a feat that would be easier to achieve by trickery than, say, playing a trombone under similar conditions (but isn't there, in any case, something depressingly ludicrous in the idea of revelations from the spirit world being transmitted through the agency of something so vulgar as a concertina?) Whether Home used trickery or not, he had a thorough knowledge of the conjuring techniques used by fraudulent mediums, and actually wrote a book on the subject.

Although the SPR has very strict standards regarding "mediums" who have been detected in deceit, a number of psychical researchers have an oddly lax attitude towards them. The argument that they have often urged takes the following form: "Mme X performed satisfactorily on such and such occasions, and no trickery could be detected. On the day in question, however, owing to ... exhaustion ... the presence of sceptics ... or whatever, her powers were at a low ebb, and so as not to disappoint her sitters, she resorted to the use of conjuring methods to produce the expected phenomena". This argument depends on two tacit assumptions: first that paranormal phenomena are dependent on some peculiar individual ability, and secondly, that there are no invariable laws governing the manifestation of such phenomena. Just as it might be said that laws of nature are subject to temporary suspension, allowing the paranormal events to take place, so any "paranormal laws" that we might care to postulate are likewise subject to occasional extinction, thus allowing the laws of nature to re-assert their powers.

Proponents of this argument (for example, Inglis, 1977) produce it with such facility that it seems no longer to occur to them just how improbable a piece of
reasoning it is. Most of us have really very little idea how conjuring tricks work – magicians would long since have gone out of business if we did. But the argument invites us to believe that several unusual individuals not only possess manifest "psychic abilities" but have also undertaken (for what reason is never made clear) a course in conjuring techniques, requiring many months of concentrated practice to acquire a moderate facility. I confess to finding this conjunction of alleged paranormal powers with magical ability very suspicious indeed, not least in Home’s own case.

Thirdly, the claim that Home was never publicly exposed, while strictly true, tells less than the whole story. It has been said that Olcott found him out in "acts of deception" (Wyndham, 1937, p.279), and certainly something unpleasant happened during his sojourn in France in 1857, when he was the guest of Napoleon III and Eugenie. It has been surmised that he was found out and sent packing. Dr Barthez, the Emperor's physician, wrote in a letter dated 25th September 1857 that "The Empress can only plead that the Home of today is no longer the Home of yesterday; that he has lost his power; and that he is attempting to replace it by recourse to trickery". Eugenie had assumed that Home was honest, since no one "would have the impudence to deceive the Emperor and herself in such a fashion for twelve months on end" (for a fuller account of this episode, see Wyndham, 1937, pp.82-85). If Home had actually been found out, then there were very good reasons for keeping the unsavoury facts concealed: it would not do for the Emperor to appear as the gull of an adventurer.
Here, I think, we come to the nub of the Home question: he exhibited his talents under the patronage of rich, distinguished, and powerful people. Royalty was not averse to entertaining him. It has been said that he never accepted payment for his seances; true, perhaps, but he was not above receiving lavish hospitality and extravagant gifts from some of the leading houses of Europe. Attendance at his seances was by invitation only, but from time to time, token sceptics might be included. While their opinions might be not be in doubt, their freedom to investigate certainly was: someone invited by the Tsar of all the Russias to witness the miracles of his honoured guest would, I suspect, feel decidedly inhibited in voicing disbelief and backing it with proof. Home was performing for the benefit of the converted, the credulous, and the conventional. It is important to remember that, however enthusiastic Home's supporters were, his public reception was often less than cordial, and several writers in the contemporary press accorded him unpleasant notices. The reported phenomena were not regarded as knock-down proofs by these people. If Home were genuine, why did he not take up their challenges?

Fourthly, the records of Home's feats often suffer from the problems of overdescription and underdescription that I have discussed in the previous chapter. His most celebrated phenomenon, the levitation of his body, is a case in point. There are several accounts of this, but when different versions of the same event are compared, they present such inconsistencies that ultimately, they fail to carry conviction (Hall, 1965). One is left with the unpalatable conclusion that, had Home indeed succeeded in his remarkable feat, his observers were not competent to describe it accurately. It is often claimed that all Home's phenomena were produced in
lighted rooms. However, Beloff says that although "Home normally operated in good illumination, that is, with ample candlelight or gaslight", the celebrated levitations always took place in conditions of "low illumination" (Wolman, 1977, pp.8,9). In fact, the illumination was so low as to be non-existent. Take this account, which I have conflated from those of different witnesses to one of the levitations: before the blinds had been drawn, it was so dark that "we could see, but barely distinguish, our hands upon the table" ... the blinds being drawn, "The room was thrown into deeper darkness than before"; ... Mr Bell "saw Home's figure pass from one side of the window to the other" (quoted by Wyndham, 1937, pp.101-102). In the circumstances, I find Mr Bell's feat every bit as remarkable as Home's.

Although the observational cases are sometimes said to offer the best, most direct opportunity for convincing even the hardened sceptic, they cannot be said to have succeeded. Fraud and self-deception contaminate them to such a degree that controversy must continue to surround them. Certainly, they are no basis for a scientific discipline.
Hume's essay on miracles holds a place of particular importance in the consideration of spontaneous and observational cases, and it has also been suggested that its arguments can be deployed against the experimental cases, although this has been resisted by some psychical researchers. Having, in the two previous chapters, considered some of the problems concerning the evaluation and acceptance of spontaneous and observational cases, I shall take Hume's discussion of miracles as a pivot between them and the experimental cases. After outlining the main points of Hume's argument, and noting its relevance to psychical research, I shall discuss the related matter of what Hume's response would have been had he himself witnessed a miracle - a matter he does not take up in the essay on miracles. The succeeding chapter will examine some recent attempts to rework Hume's arguments.

Hume begins the main part of his discussion by stating that stronger evidence is to be preferred to weaker, and that the "evidence for the truth of our senses" is stronger than the testimony of others (1975, p.109). Wisdom counsels that we proportion our belief to the evidence. That evidence we must weigh in the light of past experience. Testimony, he continues, is something that we depend upon, but individual instances can, in the light of our experience, be accepted or rejected. We may accept it because we know that memory is...
"tenacious", and because people incline towards telling the truth. We are more cautious if there is contrary testimony, and the quality of the witnesses must also weigh with us in making our judgement.

There are, however, reports of some events that are so extraordinary that they lie quite outside the normal run of our experience. This, Hume says, gives us "another degree of assurance against the fact which [the witnesses] endeavour to establish" (p.113). For an event to count as a miracle, there must be a violation in the laws of nature (which have become established through "firm and unalterable experience" (p.114)). Miracles do not happen "in the common course of nature". Uniform experience, which establishes what is to count as a law of nature, counts against our acceptance of miracles, but it also helps us, on the basis of polar opposites, to define what a miracle is. The term "miracle" only functions against a background of uniform experience.

Hume now takes the important step of positing an agency whereby miracles come about: "A miracle may be accurately defined, a transgression of a law of nature by a particular volition of the Diety, or by the interposition of some invisible agent" (p.115). Could any testimony ever be sufficient to establish a miracle? If a person were to testify to having witnessed a miracle, Hume would ask himself "whether it be more probable that this person should either deceive or be deceived, or that the fact which he relates should really have happened". "No testimony is sufficient to establish a miracle, unless the testimony is of such a kind, that its falsehood would be more miraculous than the fact which it endeavours to establish".
Hume is not prepared to allow that his condition has been met, and he offers four reasons for this:

(1) the testimony, in terms of the sheer number of genuinely credible witnesses, is of too low a standard;
(2) credulity and "greediness" for miracles are not uncommon phenomena – people can be extremely suggestible, and sometimes downright gullible;
(3) Miracles "are observed chiefly to abound among ignorant and barbarous nations". When we examine the histories of such communities, and contemplate their beliefs, "we are apt to imagine ourselves transported into some new world, where the whole frame of nature is disjointed, and every element performs its operations in a different manner from what it does at present" (p.119). This arises from the underdescription often to be found in such narratives. As Smithurst has observed: "Some of what is said [concerning Greek myths] one is at a loss to know how to take literally, as Bullfinch, 'Chiron was the wisest and justest of all the centaurs and at his death Jupiter placed him among the stars as the constellation Sagittarius.' Can this be a faithful rendering of what they believed? and if so, what on earth was it they believed?" (1981, p.28).
(4) Religions depend upon the acceptance of miracles, and the different religious traditions (and their associated miracles) are usually mutually exclusive. The miracles reported in the Christian faith would normally be denied by, say, the Buddhists, and vice versa. As a consequence of this, no single miracle could secure universal support.

We should note that Hume defines a miracle as depending upon the interposition of an agent. The agent is taken to be God or some other spiritual entity. Hume is, then, positing an explanation for the events that are reported. For many (Hume included) the explanation must
be regarded as unsatisfactory, but it is an explanation nonetheless. It is couched in theological terms, and it runs more or less as follows:
There is a God, who has created the Universe, and ordered the natural course of events and processes that take place within it. These occurrences run automatically, without further divine intervention. God is, however, omnipotent and reserves for Himself the power to intervene in the processes of His creation and turn aside the natural order of events. When such an intervention occurs, it is termed a miracle.

On this account, miracles are assumed to be more than merely unusual, and when they are claimed to occur, cause for comment: "Nothing is esteemed a miracle, if it ever happens in the common course of nature" (p.115). But however satisfactory this account might seem in theological terms, it is clearly inadequate for scientific purposes, since it offers us no opportunity for making predictions. If God can intervene when, where, and in whatever way He wishes (on a divine whim), then there can be no regularities from which scientific methods could derive explanatory formulas: science would be incapable of arriving at any well-grounded conclusions concerning the operation of either the miraculous or the mundane. In psychical research, we find a very similar situation. There, the phenomena that are reported are supposed to be the product of the shadowy half of the dualist partnership. The mind, it is suggested, is usually only capable of acting on the physical world through the agency of the body. However, it can sporadically break out of its carnal confines, to bring about a paranormal phenomenon. If, therefore, we choose to suggest that the subject matter of psychical research is of the same kind as Hume's miracles, it will be apparent that any attempt to set
on foot a serious scientific investigation will be
doomed.

Hume, in framing his definition, posits an actual
mechanism for the bringing about of miracles. The
psychical researchers, beyond vague and often
misleading talk of "the mind" and "its powers" do not
allow themselves to become so specific when discussing
psi. How, we want to ask, does the "mind" actually do
it? J.B. Rhine writes of the "lack of a conscious clue
to its operation" (Hook, 1960, p.72), and Roll notes
that "intelligible explanations of psi phenomena are
conspicuously lacking" (White, 1976, p.3). Any agency
seems to be absent.

It is of course possible to continue working in the
hope that an agency or a mechanism could be uncovered,
just as it is possible to imagine that all psi
occurrences are brought about by the interposition of
the Deity. We might even propose that psi occurs
without any mechanism or intervention at all - as a
purely anomalous event. Until psychical research is in
a position to argue for one of these options over the
others, it is in no position to claim any sort of
scientific status.

Hume's arguments have not gone uncriticized. Inglis,
who calls the essay on miracles "scepticism's first
manifesto" (1977, p.138), claims that recurrent
spontaneous cases - such as poltergeists - demonstrate
that the principle of the uniformity of nature, which
Hume relies on, does not hold: nature is subject to
occasional breaks in continuity. But all such cases
rest on testimony, and testimony is not something apart
from the natural order, it is itself a natural
phenomenon. The human beings giving their testimony are
part of the pattern, and their reliability must be judged in the same way that we judge their statements. Just because someone says that they saw something does not, of itself, make it true that they did, and sensible people do not believe everything they are told.

Hume has been accused of being prepared to accept only that testimony which would suit his world view: a form of question begging. However, the alternative stance is not without its difficulties. If the presumption of a natural order (a presumption that pervasively informs our thinking) can be said to amount to question begging, then what alternative could the psychical researchers themselves propose that does not also beg the question? Testimony can only be judged in the context of a natural order, and the presumption of such an order, far from begging the question, is a prerequisite for the making of such judgements.

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There is a special way of establishing that a miracle has taken place, of course, and that is to be an eyewitness oneself: to anyone in this position, the testimony of others is an irrelevance, and Hume, as I have said, does not deal with such cases in the essay on miracles. One could say that his intention in that work was not to dispose of miracles altogether, but to offer a check to them, a defensive rather than offensive move (Flew, 1961, p.174), which will put an end to at least some of the impertinent solicitations. However, it is possible to frame an answer to the question "what might Hume have said if he had been a witness?" In his Dialogues concerning natural religion (1976, p.173) he has Cleanthes ask the company what
they might think if "an articulate Voice were heard in the Clouds, much louder and more melodious than any which human Art cou’d ever reach: Suppose, that this Voice were extended in the same Instant over all Nations, and spoke to each Nation in its own Language and Dialect"; would this not, he asks, be firm proof of "a benevolent Being, superior to Mankind"? I think Cleanthes was wrong in making this assumption, as was Hanson (1971, pp.313-314), in setting out a remarkably similar thought experiment:

Suppose ... that on next Tuesday morning, just after our breakfast, all of us in this one world are knocked to our knees by a percussive and ear-shattering thunderclap. Snow swirls; leaves drop from trees; the earth heaves and buckles; buildings topple and towers tumble; the sky is ablaze with an eerie, silvery light. Just then, as all the people of this world look up, the heavens open - the clouds pull apart - revealing an unbelievably immense and radiant Zeus-like figure, towering up above us like a hundred Everests. He frownsdarkly as lightning plays across the features of his Michaelangeloid face. He then points down - at me! - and exclaims for every man, woman and child to hear: "I have had quite enough of your too-clever logic chopping and word-watching in matters of theology. Be assured, N.R. Hanson, that I do most certainly exist."

Nor is this to be conceived of as a private transaction between the ultimate Divinity and myself - for everyone in the world witnessed, "knew by acquaintance", what had transpired between the heavens and myself and all men heard what was entoned to me from on high. TV cameras and audio-tapes also recorded this event for all posterity.

Please do not dismiss this example as a playful, irreverent Disney-oid contrivance. The conceptual point here is that if such a remarkable event were to transpire, I for one should certainly be convinced that God does exist. That matter of fact would have been settled once and for all time.

Both Cleanthes and Hanson assume that such visions would actually settle the matter. In both cases, the
apparition gives us reason to think that His nature is divine, but there are a number of questions to be answered before we are obliged to accept such claims at their face value. Certainly, the eyewitnesses report something very strange happening, but can they be sure that the vision was of God? Could it perhaps have been a mischievous Cartesian demon? Even if it were God, to what extent would that prove all the things that some people say about Him - that He created the Universe, and all that therein is, that He is omniscient, and will preside over the end of the world, sitting in judgement on all men, and so on and so forth?

Even if we set such considerations aside, the cases are still by no means straightforward: how, for example, is the voice of Cleanthes' God actually produced? What kind of vocal chords would be necessary to generate the volume required? How does the voice succeed in speaking in every different human language simultaneously? This is not a closely worked out thought experiment, it is little more than a crudely magnified picture of a large man with a loud voice, and really much the same could be said of Hanson's version. The moment we attempt to fill in the details of such a picture, we are left with nothing but unanswered questions: what sort of bone structure does this figure have? How does its brain control the movements of its limbs? (See Smithurst, 1981 for many other similar questions). Faced with these "miracles", we have no means of knowing how to evaluate them: as Austin says (1962, p.24), "it is not generally known, or agreed what seeing ghosts is", and there is, by the same token, no generally agreed idea of what "seeing God is" either. The pictures that we are offered need to be more closely worked out before we can even begin to pronounce on them.
It is not without interest to note that Francis Hitching, in a paper read in the late 1970s to the SPR, remarked that hostile sceptics might come to accept paranormal phenomena if they themselves underwent a personal confrontation with some experience that they could not explain. As Hitching says: "probably the most convincing event for the sceptic is his/her own naturally occurring psi experience. Time and again, lay and scientific sceptics have shifted their beliefs after a dramatic, but by no means convincing, psi experience".

Presumably what Hitching has in mind is an overwhelming "road to Damascus" experience that would bring about a permanent change in the outlook of the hostile critic. But how many such changes occur and are actually documented? If this has happened "time and again", it is odd that it is so little referred to. It is also odd that, earlier in his lecture, Hitching complains that some sceptics are "religious" in their doubting: they demonstrate, he feels, a fanatic disregard for the truly scientific principles of psychical research. But for him then to demand a "religious" conversion to "scientific" principles seems absurd. Personal experiences which involve anomalies are notoriously hard to describe and to evaluate (see, for example, Blackmore on the OBE in Shapin, 1983, pp.97-98), and no matter how important a person finds her own experience, that is not to say that her interpretation of it or her estimate of its significance is to be accepted as conclusive. In any case, it has been claimed that sceptics tend to have lower scores in ESP experiments than do believers (the so-called sheep-goat effect); asking the sceptic to oblige by having a psi experience may well meet with failure.
It is sometimes difficult to be sympathetic to the often bizarre suggestions that are put forward by many of the psychical researchers. Some people are able to perceive miracles, numinosity, and Divine intervention wherever they go. Most of us do not, indeed cannot. The events that we see are, in essentials, the same. Our interpretations of them are what sets them apart.

Someone tells me that a vase hovered in mid-air, then hurtled across the room, smashing to pieces against the wall. I was not present, but saw the secretly-taken video recording of the same event, and know that the vase obeyed the laws of physics when it was surreptitiously thrown by a young girl.

It is easy to make reference to a mental picture that one has and to claim that it "makes sense" (and therefore, we shall be told, makes "logical sense"). In such a way, an incompletely worked-out picture can be put forward as being viable. But suppose that physics were to be adjusted to allow for the occasional hovering vase. Would physics (or, indeed, the rest of science) be the richer for it? On the contrary; the structure of science is cantilevered together. Remove a single beam, and you risk the collapse of the whole structure. Miracles are underdescribed, and indeterminate on many important points. Hanson's vision does not settle the matter conclusively: it is merely an incompletely worked-out picture, and would require supplementation on many particulars before we could even begin a serious assessment.

Most of the events that are touted as miracles are, however, far less spectacular than Hanson's apparition. The miracle might be observed by a believer and a
sceptic, and one would cry it up as a significant proof that the Universe was imbued with hidden meaning, and the other would not even notice it: the miracle would pass him by. As John Wisdom has said (1953, p.153) "It is possible to have before one's eyes all the items of a pattern and still to miss the pattern"; it is also possible for someone to find a pattern where it is not immediately apparent that one exists. Let me illustrate this by reference to an incident that happened to my mother and her cousin (this is not, then, just another case I have read about - I have discussed it with my mother, and have the best of reasons for thinking her account to be accurate in all significant particulars). They were visiting Liverpool on a shopping trip, and they knew that an old house which had been owned by some members of our family for many years was being demolished. When they had finished their visits to the shops, they drove out to the suburb in order to catch one last glimpse of the house before it was razed. They arrived at the critical moment, for the demolition workers, having stripped what was salvageable, were firing the house. As their car drew up, flames burst from the windows of the shell. To them, it was as shattering as the burning of Valhalla. This was no "mere coincidence". I regret to say that my own mundane reaction was: "how fortunate you were to arrive in time". What my mother still regards as an astonishing and numinous event, I consider to be a lucky, but not entirely fortuitous, coincidence. Her miracle (if one chooses to call it such), has passed me by. The factual description in both our cases is the same, but our two interpretations of these facts are irreconcilable. Of course, she was there and I was not, but even so, it is not that my response casts any doubt on her veracity; I simply see the event in a very different light from her.
The conceptual baggage that each of us carries around and the presuppositions that we bring to bear in the interpretation of such cases are vitally important. Different people have what Hare has called different "bliks" (Flew, 1955), and facts are interpreted within the frameworks imposed by such bliks. It is not unreasonable to suppose, then, that had Hume seen an event which some might proclaim to be a miracle, he would refuse to acknowledge it as such — not because he doubted that it had happened (his attention having been distracted, for instance) — but because his interpretation of the facts was radically different.
CHAPTER ELEVEN

MODERN VERSIONS OF HUME'S ARGUMENT

The argument employed by Hume in his essay on miracles should strictly only be employed against spontaneous and observational cases of psi, where testimony is crucial. There is some justification, however, for stretching it to cover the experiments (in which the statistical scores rather than the testimonial records are the significant factor). Hume, in introducing his principle of the "laws of nature", is firmly basing his argument in an appeal to the uniformity of experience. Not only are paranormal phenomena outside our uniform everyday experience, they are themselves evanescent, and do not appear to obey any laws.

Flew argues (in his edition of the essay) that Hume's thesis can be extended to meet the experimental cases because parapsychology has failed to supply a repeatable experiment: all we are offered is yet more testimony, dressed up in the language of a scientific paper. It is not unfair to say, as Spencer Brown (1957) has done, that the results of an experiment that cannot be repeated are themselves little better than an anecdote. The long history of fraud in psychical research, which amounts to an old-established tradition, should also give us pause. These are good reasons for adapting Hume's position to the most recent work.

Two other sceptics have discussed the experimental results of parapsychology with a particular eye to the

In his two books (1966, 1980), Hansel takes up a stance which rests only partly on Hume. He takes a number of key experiments from the literature of parapsychology which have been singled out by its proponents as being "conclusive", and considers in each case whether there was any possibility of fraud or error: "if ... it can be reasonably ascertained that trickery or error have been eliminated through the employment of a completely watertight experimental procedure then the experiment can provide evidence to support the hypothesis of extrasensory perception" (1966, p.22). Hansel then argues that the major "conclusive" experiments were actually conducted under conditions much less tight than we had been led to suppose, and so cheating and misrecording were possible. Hansel is thus able to demonstrate (to his own satisfaction, not necessarily that of the psychical researchers) that the experiments were very far from being conclusive. Thus, for example, in the Pearce-Pratt series at Duke in 1933-34, where the two "participants" were supposed to be in separate buildings, 150 yards apart, no one checked to see if Pratt was actually closeted in the Library. Yet their physical separation was supposed to be, says Hansel, "a most important control feature of the experiment" (1966, p.81).

So far, Hansel, working from the standpoint of experimental psychology, takes an a posteriori line towards psi: "a vast amount of experimental work has failed to give a satisfactory demonstration of its occurrence" (1959, p.177). He accepts that the experiments have produced results which call for an explanation, and supplies one in terms of inadequate
design and execution, which allow errors to creep in unnoticed, and trickery to be perpetrated undetected. However, he also hints at a deeper objection: "it is not unreasonable to assume that telepathy cannot occur in view of the a priori arguments against it" (1959, p.177), and "it could, however, be argued that the probability that ESP exists is insignificantly small and the probability of fraud quite appreciable" (1966, pp.21-22). This position comes very close to Hume's, but Hansel is careful not to subscribe to it wholeheartedly. He does, though, accept Hume's principle of the uniformity of nature: "if Uri Geller, for example, really could predict the fall of a die correctly eight times in eight attempts and if he could repeat this demonstration many times before independent critical experimenters, the assumption of impossibility would eventually have to be rejected" (1980, p.301), and "if a process really does exist in nature, this fact will eventually silence all objections" (1966, p.22).

Hansel is not, then, taking a strictly Humean line - he is not, despite Palmer's accusation (Krippner, 1978, p.64) one of those sceptics who "play the Hume game". It is true that Hansel's views owe a good deal to Hume, but his importance as a sceptical critic lies in his a posteriori dissection of many experiments and his remorseless cataloguing of their shortcomings. For Hansel, it is sufficient to show that fraud could have occurred, not that it did, or how. In discussing the Soal-Goldney experiments, for example, he suggests a number of possible ways in which the results could have been produced by manipulation. As it turned out, the method used was different (Markwick, 1978), but Hansel's importance lies in the fact that he asked the question and attempted to answer it. From such
trenchant criticism, there might come better designed and controlled experiments, and this, presumably, should be welcomed by psychical researchers. Hansel has been labelled as an a priorist by, among others, Cyril Burt and by Targ and Puthoff (see Hansel, 1980, p.301), but this charge cannot be sustained. Another critic, G.R. Price, is, however, most definite in his espousal of Hume's position.

Price's celebrated article originally appeared in 1955 (it was reprinted, with the replies it stimulated, in Ludwig, 1978, and all subsequent references are to this collection). Price, unlike Hansel, takes a firm a priori stance against paranormal phenomena: "My opinion concerning the findings of the parapsychologists is that many of them are dependent on clerical and statistical errors and unintentional use of sensory clues, and that all extrachance results are dependent on deliberate fraud or mildly abnormal mental conditions" (p.49). This position is firmly in agreement with Hume's, which Price acknowledges as his starting point. He then goes on to say that "ESP is incompatible with current scientific theory". However, Price then changes tack, and proposes that ESP conflicts with Broad's basic limiting principles, which lie, he feels at a more fundamental level than scientific theories. Price is prepared, for the sake of argument, to take Rhine and Soal at their own evaluation, as having demonstrated something "truly revolutionary" and "radically contradictory to contemporary thought". "The parapsychologists are themselves agreed almost unanimously that psi phenomena are completely incompatible with modern physics" (p.150).
There is a tension in Price's account: on the one hand, he claims that psi conflicts with "current scientific theory" and "modern physics", and on the other, that it runs counter to "basic principles"; "I accept Broad's analysis", he says, "and incorporate it as part of the present argument" (p.150). But Broad introduces the principles as a means of defining psi; Price seems to want to use them as an a priori argument against the existence of psi. But in the remaining twenty one pages of his paper, Price confines his attention to certain incompatibilities at a "less fundamental level", and makes no further reference to Broad's principles (beyond saying that we make "certain generalisations concerning observable phenomena", some of which are laws of science, others, "so fundamental that we rarely name them").

For Price, reports of paranormal phenomena are to be explained in terms of fraud. The crucial passage is as follows: "When we consider the possibility of fraud, almost invariably we think of particular individuals and ask ourselves whether it is possible that this particular man, this Professor X, could be dishonest. The probability seems small. But the procedure is incorrect. The correct procedure is to consider that we very likely would not have heard of Professor X at all except for his psychic findings" (p.157). Price then discusses Soal's work, in the course of which, he proposes a number of methods whereby the results could have been produced by fraudulent means. He concludes by presenting a number of designs that "appear to be reasonably fraudproof"; however, this vitiates his sceptical approach, since if paranormal phenomena are impossible on a priori grounds, there is small point in carrying out further empirical research.
Price’s paper drew a number of responses, including one from Soal, commenting in “some amazement” on this “diatribe of unsupported conjecture”, but failing to confront the major issue – Price was actually trying to make sense of Soal’s results, to place them in an explanatory framework, whereas Soal, by using the standard vocabulary of “telepathy” and so on, does not offer any serious explanation at all, he simply alleges that something odd must be going on somewhere.

Rhine felt that the criticism was “on the whole, a good event for parapsychology” because it revealed that the results posed a serious threat to mechanistic science. But the most substantial reply to Price came from Meehl and Scriven. They argue that Price’s attack can only succeed if ESP is incompatible with modern science and if modern science is complete and correct. They point out, quite correctly, that Price attempts to demonstrate the incompatibility of ESP with physics, “rather than relying on Broad’s philosophical analysis” (p.188), and moreover that it would be rash to claim that revolutionary discoveries in science could never occur. Scriven has expanded on these criticisms (Thakur, 1976, pp.191-193) by saying that the positions of Hume and Price are “far too strong”, in that they assume that “no revolutionary theory in science could ever be accepted, since it involves overthrowing pre-existing and very well-supported theories”. But this argument, while it can be deployed against Price, who invokes the principles of science in order to frame his attack, fails to touch Hume. Laws of nature are what Hume specifically appeals to, not laws of science. Hume’s laws of nature are wider in their range than laws of science, and there is a distinction to be drawn between the kind of case that Hume puts forward (that a bar of lead cannot float unsupported in the air) and
specific laws of science or of physics. Scientific theories change, but there are certain facts - part of our common stock of unchanging everyday observations - that any theory must accommodate, however revolutionary it may appear to be.

Attempts by psychical researchers to challenge the validity of Hume's arguments and by sceptics to redeploy them in the context of experimental parapsychology have not always been successful. Hansel bases his criticisms largely on a posteriori grounds, and makes only passing reference to a priori objections. Price is confused over the different kinds of a priori objections that can be raised. However, Flew's insistence on repeatability (as against anecdote) and his pointing up the prevalence of fraud in psychical research serve to remind us that Hume's arguments still have an important place in the debate concerning the scientific status of experimental parapsychology.
CHAPTER TWELVE

EXPERIMENTAL CASES:

I. THE FUNDAMENTAL ASSUMPTIONS

I have argued that psychical research makes certain untenable philosophical assumptions that vitiate its research programme, and that its claim to scientific status cannot rest on spontaneous or evidential cases alone. But what of the evidence provided by the experimental work? Surely, here, it will be suggested, are findings that cannot be ignored. Certainly, it would be little more than pure prejudice to dismiss them out of hand: the experiments present, as I have said, the most serious case for the sceptic to answer. The results of these experiments are doubtless intriguing, but what foundations do they rest on, and what explanation can parapsychologists offer for their results?

Thouless (1972, p.19) has suggested that the experiments are of two kinds. One is designed to demonstrate the occurrence of psi, the other to reveal its modes of operation. This is, though, little more than ex post facto rationalization. The former kind simply records some statistical anomalies (the "psi hypothesis" is defined only in statistical terms) without providing any explanation for them, and the latter yields no hard information at all. This, I suggest, is because of the underlying assumptions that are made. I now want to examine those assumptions, and
consider some of the problems in explanation that are encountered.

I have, in Chapter Four, already argued that the experimental work falls within the behaviourist tradition of psychology. The form of experimentation that Barrett and his colleagues pioneered in the last century, and which J.B. Rhine consolidated in the period between the wars, has not materially altered. There is a randomizing device (such as a pack of Zener cards) which generates targets, one or more participants (or people who are, it is assumed, participating), a scoresheet, and a relatively simple statistical method for analysing the results. Over the years, the mechanics may have been changed (computerized random number generators, for example, rather than hand-shuffled cards), but these are merely changes in technique - the essentials remain the same.

One result of this behaviouristic form of experiment is that while input and output are recorded (targets and guesses are both listed) no effort is made to look into the processes that are going on inside the experimental subject, who is treated as a black box. The problem of finding out what is supposed to be happening is compounded by what I have termed the unconsciousness principle. There seems to be no such thing as a "psi experience" - no one reports any particular imagery or sensation which can be linked to a better than average performance in a card-guessing experiment. All that the subjects seem to do is guess cards, and there is nothing to suggest that there is any experienced difference between thinking of a card and getting it right, and thinking of another and getting it wrong.
The nearest thing that we seem to have to a conscious experience is to be found in those cases where the subject indicates that he has a feeling of confidence about particular guesses. The experimental literature on this question has been reviewed by Carpenter (Volman, 1977, pp.219-222), who suggests - with proper caution - that certain techniques "seem to hold out some promise for success in aiding self-reflectivity". But all the cases he cites involve little more than hunches: we have no mention of "awareness" or "conscious experience". These experiments fail to take us beyond the idea of guessing, which I have already discussed. All that is added is a gloss: guessing can sometimes become "confident guessing", but any amount of confidence will not enable us to describe what actually happens in the course of the experiment.

It is not only the experimental cases that are subject to this difficulty. In some spontaneous cases of psi gamma which were recorded by L.E. Rhine (1971, pp.45-46), there was a measure of conviction on the part of the people making the reports. These "conviction cases" seem to be "instances in which the ESP message had somehow been made conscious. Consequently they were examined to see if the persons betrayed any method by which the transfer into consciousness had been accomplished. However, no such indication could be found. The persons simply believed without any reason."

The failure to find any psychological experience would not be fatal, however, if some physiological factor could be uncovered. The discovery of, say, some neurophysiological event or process which occurs during a parapsychological experiment, and which is positively correlated with the hits scored would certainly count as a major breakthrough. Neppe's work (as an example,
see his 1983 paper) is an interesting pointer in this
direction, but further and better results must be
obtained before serious predictions can be made.

The work of the pseudonymous Lloyd (1973) should also
be considered in this context. His experiment, which
has been favourably mentioned by Owen and Sparrow
(1976, p.5), could have been one of the most important
pieces of work ever carried out in parapsychology, if
only "Lloyd" had taken the trouble to design it
adequately. The subjects were asked to create "a mental
image" of a cup of coffee, and to attempt "to
psychically communicate this image to the receiver".
"This technique", he says, "requires considerable
practice". While this was going on, "Lloyd" monitored
various neural events in his subjects. He discovered
that there was a significant positive correlation
between the occurrence of psi and events in the brain.
It should be said that this impressive result is less
exciting than it sounds, for the scores were not taken
singly but were averaged over the whole series;
subsequent attempts to replicate have been
unsuccessful, and "Lloyd's" work is treated with
caution by parapsychologists (Wolman, 1977, pp.708,
719). In any case, the experiment is tendentious to a
fault: "Lloyd" speaks of "what is believed to be a
transmitted thought" (p.74) without any adequate
characterization of what a "transmitted thought" might
be like, or how we might "practise" transmitting them,
still less how we might "believe in one" when we saw
one, or indeed recognize one as such when it was
transmitted to us.

Because we never see the ESP or PK in action, the
parapsychologists have been forced to rely on
statistical techniques to detect their occurrence. The
cards are turned, the dice are rolled, and the scores recorded. Only after the event, and after a lengthy session of checking and calculation, can we propose that a paranormal event has taken place. This situation is particularly striking in the PK cases: here, the assumed effect is very different from what we might expect to see. Statistical results are far less compelling than actually being able to see the dice moving about without being touched or interfered with in any way.

The use of statistics in science is an accepted standard technique, but there are signs that for parapsychology, it has become an end in itself. Drury, in discussing some problems involved in medical diagnosis (1973, p.9), quotes at length from the nineteenth century French physician, Bernard. The points made are so apposite that I reproduce it in full:

In every science we must recognise two classes of phenomena, those whose cause is already defined; next those whose cause is still undefined. With phenomena whose cause is defined statistics have nothing to do; they would even be absurd. As soon as the circumstances of an experiment are well-known we stop gathering statistics. ... Only when a phenomenon includes conditions as yet undefined, can we compile statistics; we must learn therefore that we compile statistics only when we cannot possibly help it; for in my opinion statistics can never yield scientific truth, and therefore cannot establish any final scientific method.

Statistics can bring to birth only conjectural sciences; they can never produce active experimental sciences, i.e. sciences which regulate phenomena according to definite laws. By statistics we get a conjecture of greater or less probability about a given case, but never any certainty, never any absolute determinism. Of course statistics may guide a physician's prognosis; to that extent they may be useful. I do not therefore reject the use of
statistics in medicine, but I condemn not trying to get beyond them and believing in statistics as the foundation of medical science.

Parapsychologists overvalue their results in just the way that Bernard warns against: the statistics certainly do look curious, and suggest that something odd is going on that should not be going on. But something more is required: the statistics must not only look curious, they must be significant. The actual level of significance is arbitrarily defined, and Canter (1985, p.2) condemns it as "unscientific posturing" to place much faith in it: "the curiousness of this reliance on arbitrary statistical levels to provide an indication of significance is further highlighted when it is appreciated that none of the great theoretical contributors to psychology have ever relied on statistical significance as the main defence of their case". Rhine, however, is much more sanguine: statistical significance means "simply that by general agreement among scientists, it would be justifiable to consider that the results require some other explanation than chance; in a word, they are lawful or reliable" (1948, p.34). The use of the word "lawful" in this context is strange, since the point of collecting the statistics in the first place was to try to establish laws, so that further figure-gathering would be unnecessary. Rhine seems not to want to get beyond his statistics, and at the same time to make a specious claim to scientific credibility.

Tyrrell (1947, pp.103-104) holds that it is proper for each person to fix their own point of significance, adding that because the experiments of Rhine and Soal "have yielded results so enormously far above chance ... the point of significance is unimportant". So much doubt has been cast on these experiments in recent
years that Tyrrell's quixotic proposal would now find few supporters.

Statistically significant deviation from the mean chance expectation is taken as the criterion for saying that a paranormal occurrence has taken place. Although Rhine talks of "some other explanation than chance" it is never seriously in doubt that the only other one that he is prepared to countenance is the hypothesis of paranormality. The statistics do not function as evidence for this hypothesis, they are used to define it. So whenever a score deviates from the mean chance expectation, it is assumed that it must be brought about paranormally. But in spite of the cliché, figures do not speak for themselves - they have to be interpreted, and it is sound interpretation that is signally lacking; all we have are some rather odd statistics and a very tendentious interpretation. As Spencer Brown puts it: "Any attempt to randomize, of which tables of random numbers and psychical research experiments are both typical examples, will lead all too frequently to the curious results which have been thought in the past by psychical researchers to be evidence of telepathy and whatnot" (1957, pp.116-117).

There are two other important features of the parapsychological experiments that we should note, and these concern the apparent efficacy of psi regardless of any limitations of space and time. The postulation of precognition and of other varieties of temporally displaced phenomena shows that parapsychology is clearly disposed to accept the possibility that time can be transcended, and can even flow backwards. In just the same way, it seems that space is no barrier to psi; no matter what the distance, the strength of the "psi signal" is not significantly attenuated. The
inverse square law does not hold. Even over substantial distances, there is no detectable impairment. For Barrett (1912, pp.108-109), this was decisive proof of the non-physical nature of psi phenomena: they "do not belong to the material plane, and therefore the laws of the physical universe are inapplicable to them".

A similar position was reached by Tyrrell, in his critique of the radiation theory of psi. If telepathy, he says, were to obey the inverse square law, then "a person who could transmit a telepathic message across the ocean would produce an enormously more powerful effect across a table" (1947, p.69). How this "more powerful effect" might manifest itself is not specified. Would the subjects find themselves wrenched from their seats by the force of the radiation, or would they simply score a few more correct guesses?

It might be suggested that the inverse square law does actually apply, as Meehl and Scriven propose (Ludwig, 1978, p.188), but that the telepathic signal, though considerably attenuated, is still capable of functioning. Whether we accept the radiation theory or not (and there seems little to be said in its favour), there can be no doubt that, if the inverse square law does hold, there is no way of demonstrating it. Psi seems to be quite unlike any known form of electromagnetic radiation, penetrating Faraday cages and travelling over vast distances without any discernible lessening of its power.
In the introduction to his edition of Hume's essay on miracles (1985), Flew notes three major problems that lie in the way of the acceptance of the claims made by parapsychology. These are the prevalence of fraud and self-deception, the lack of any repeatability of the phenomena on demand, especially in other laboratories, and the glaring absence of any remotely plausible theory to account for the results.

Flew's position is an interesting one, developing as it has over more than thirty years. In the early 1950s, he was seriously - but cautiously - interested, and expressed the hope that experiments would continue, so that some underlying principles might be discovered. On the question of untenable philosophical theories which so often form a part of psychical research, he has always been firmly critical. Now, his position has become much more sceptical: inadequate philosophical models such as Platonic-Cartesian dualism have not been abandoned by psychical researchers, and genuine understanding of the experimental results seems as elusive as ever, despite the accumulation of even more results over the intervening years. But Flew is no ordinary sceptic: he treats the results with care, rather than disdain, and is prepared to accept them as being statistical quirks or mere coincidences. He does not dismiss them out of hand, as many sceptics do, but
neither does he jump to all manner of conclusions about what is being proved. This position, known as Flewism, raises some interesting questions in the philosophy of parapsychology.

I have already discussed some aspects of fraud in relation to the spontaneous and the observational cases. There is too close a connexion between the phenomena of psychical research and conjuring tricks for anyone without an expert knowledge of magic to feel comfortable. It is not necessary, either, to assume, as Eysenck has done, that if trickery were to be employed, it would require a large scale conspiracy. An experimenter can, even when part of a research group, still single-handedly tamper with the results, as is shown in the well-known cases of Soal and Levy.

Flew's second objection, that of the absence of repeatability of experiments, is probably the most common complaint to be advanced, certainly by the scientific community. The experiments have not, in general, shown themselves to be repeatable, regardless of whether the replications are carried out by different workers in different laboratories, or by the original workers themselves. Until a repeatable experiment is forthcoming, many scientists argue, parapsychology should be denied any scientific status. Such a demand can be misunderstood, for science in general does not depend on repeatability. There are certain disciplines, which, by their nature, cannot offer repeated instances of particular phenomena: geology and palaeontology are two obvious examples. And there are other sciences, such as astronomy and meteorology, where phenomena may recur, but only rarely, and certainly cannot be brought about on demand. The importance of repeatability is to be found
in the experimental sciences, where methods can be used to isolate and measure the different variables that are involved in the production of the phenomena under investigation.

Scriven has suggested (Colodny, 1964, p.97) that this demand for repeatability may be too strong:

If I make the claim that a certain kind of drug will cure diabetes, administered in a specific way, then I expect when I administer it in this way to find this result. That is not the kind of claim that is being made here. The claim that is being made [in parapsychological cases] is that some people have a certain capacity. The appropriate comparison is with claims about individuals who are alleged to be calculating prodigies or eidetic imagers.

But there are two important differences here. The calculating prodigies can produce their feats on request. Although, as Scriven says, their powers may wane over the years, they can calculate on demand in all manner of circumstances. So far as the parapsychology experiments suggest, even the handful of "star subjects" that have been discovered are not capable of producing good results in all conditions, especially when sceptics are present. Moreover, Scriven's analogy is not a true one. The calculating prodigies all do something which you or I could do, given the aid of a calculator or paper and pencil. It is just that they do it in their heads, and at remarkable speed. ESP is not like that; I cannot buy a machine that will help me to guess which card you are now looking at. I know what it is to do mental arithmetic, but not to perceive extrasensorily. Having carried out a calculation in my head, I can write down the steps in the process on paper, and survey my working out of the problem. ESP, however, remains as mysterious in its operation as ever.
For some psychical researchers, the absence of repeatability has been a cause for excitement rather than despair. Rhine certainly exhibits this tendency strongly, claiming that the very lack of regularity and consistency in his results is not only a challenge, but a vindication of his view that paranormal phenomena are non-mechanistic, and thus, he would argue, non-material.

But even if repeatability could be achieved, it would only be the beginning: repeatability is needed not simply to vindicate the phenomena but to set the new science of parapsychology on secure foundations. Once a repeatable phenomenon is found, systematic work could be carried out to isolate the significant factors that bring it about. We could not, however, assume that the present parapsychologists would be the most competent group of specialists to carry out this work (just as, for example, flying saucer enthusiasts might not be the best experts to investigate extra-terrestrial beings, were some to be discovered).

Flew's third objection is the absence of any serious theory to explain the results. There is no shortage of ideas in this area, but the essential feature has to be testability. As Walker has said, "There are at present a large number of 'theories', hypotheses, concerning the nature of psi. The number can grow essentially without limit, and avail nothing. Theories have a value only to the extent that they provide a consistent understanding of experimental data. In this regard, most theories have explained almost none of the detailed data developed by parapsychologists" (1984, p.326). McConnell agrees: "large-scale theorizing, not closely tied to the data, is not acceptable" (1982, p.22).
There seems, among parapsychologists, to be a naive empiricist view of the workings of science. After an extensive period of data-gathering, the scientist is supposed to sit down and seek out underlying regularities, and then go back and check to see if they actually hold. This view is very clearly expressed by Carrington. The scientist, he says, "wishes neither to prove nor to disprove anything. ... The facts are the most essential things of all. Theories can follow" (1932, pp. 14, 16). Rhine was also content to leave theory till later: it is "not assumed that ESP is continuous and regular in its function. Any assumption whatsoever about it is to be avoided at this point" (1940, p. 16). Parapsychology can lay claim, of course, to a vast body of what Carrington would call facts, that is, experimental results. But are these worth anything as they stand? One can imagine an enthusiastic amateur meteorologist carrying a thermometer around with him, and keeping a record of the atmospheric temperature every hour, regardless of where he happened to be. After many years of devoted collecting, he would have amassed an impressive array of such "facts", but to what purpose? Unless the observations can be shown to mesh in with other associated observations (of, say, atmospheric pressure), they remain a collection of unrelated numbers.

Theory is logically prior to experiment: we cannot begin to design an experiment without having certain theoretically determined expectations. To state the matter crudely, unless we know what we are looking for, we shall be in no position to say whether or not we have found it. We do not fumble about, trying different things "to see what happens". But neither should we attempt to rewrite science on a major scale unless we
have some incontrovertible results to offer: mere speculation is not enough.

As an example of the sometimes whimsical theorizing that can go on, consider this proposal made by Donald and Martin (Martin, 1983). Their "theory", which relies on the assumption of backwards causation, is called the "past directed negentropy theory". As Martin puts the matter, if psychic phenomena were real, "there would have to be a revaluation of several branches of physics". Now that is, to say the least, a large claim. Unfortunately, what Martin then does is not to show that such phenomena are real - that, it seems, is to be taken for granted. He simply suggests that the laws of physics should be rewritten (discarding the second law of thermodynamics in the process) to accommodate the results.

The results themselves, though curious, are by no means so clear-cut as parapsychologists like to claim. Psi is simply another name for statistically significant deviations from the mean chance expectation. The so-called psi hypothesis has no explanatory value - it tells us nothing of how things work in the world. Although Rhine sometimes urges the rejection of mechanistic models (which he identifies as outmoded relics of the nineteenth century), his own hypothesis takes us well beyond the limits of what can be legitimately asserted.

The problem of constructing viable theories admits of no easy solution. Thouless has argued (1972, pp.2-3) that we can expect no guidance from common sense, and, given the inherently queer nature of the phenomena, one can feel a certain sympathy. But robust common sense might urge that the results are nothing more than fraud
and error. Thouless disagrees: he is looking for a "scientific" solution, and he relies on an analogy with two physical phenomena. The behaviour of subatomic particles and the transmission of light waves through a gravitational field both run counter to our common sense expectations. If common sense can be mistaken about these, then it should not attempt to pronounce on parapsychology. But the analogy is a poor one: common sense does not comprehend such things as subatomic particles and light waves, and thus is not entitled to make assumptions about their behaviour. Common sense is firmly rooted in our everyday experience of the world, and in our ability to make rational inferences on the basis of that experience. It could be said that nobody encounters these phenomena as such, except for professional physicists (that is to say, they are uncommon cases to begin with). What is required are explanations, a commodity in which common sense rarely trades. Any fool knows that apples fall to the ground. Physics shows us how. The paraphysicist Elizabeth Rauscher (in a BBC Radio 4 interview on International Assignment, broadcast on 17th December 1984) pointed out, by way of demonstrating the limitations of physics, that although we know how gravity works, we do not know why. An answer which aimed to serve what Wittgenstein called "philosophical clarity" might be that that is just the way gravity is.

Important as the prevalence of fraud, the lack of repeatability, and the inadequacy of theory may be, there is another compelling reason why parapsychology should be denied scientific status. That is its tendency to "fudge". Suppose that we run a card-guessing experiment, E1, and obtain a significant score. A week later, we run a replication of that experiment, E2, in which all the circumstances are kept
as close as is humanly possible to those in E1. The only difference between the two is in the results. In E1, the score is high, in E2, it is in accord with the mean chance expectation. What can we say? The sceptic would probably aver that E1 was flawed in some way, or just a lucky chance, and that the results in E2 represented nothing more interesting than the familiar statistical tendency of regression to the mean. The parapsychologist, on the other hand, is likely to mark down E1 as a case of psi, but to propose that E2 was marred by some unconscious psychic blocking by the experimenter or the subject—the so-called experimenter effect, which, it should be noted, allows the parapsychologist to advance a psychic explanation even when there is no evidence for it ("heads I win, tails you lose..."). The parapsychologist, then, who would have been delighted to have a replication of E1, is not prepared to count E2 as a disproof.

Parapsychology makes itself fireproof by fiat: a positive score is a confirming instance, a chance score is one where the subject's ESP is blocked by the experimenter's, a negative score is "psi missing". This is nothing more than fudging. The corollary of it is, however, that if disproof and falsification have no value to the parapsychologists, can they any longer speak meaningfully of proof or confirmation?

If this seems to be a harsh criticism, consider the matter of "chronological decline" in scoring. A subject, having begun with high scores in earlier tests, will, over a long series, find that decline sets in: practice, it seems, does not make perfect (see Rhine, 1940, pp.284, 314). Both ESP and PK are unstable and fugitive. Rhine's method of dealing with this difficulty is first to ignore it: variations in scoring "are outside the scope of concern under the problem as
formulated. Statistical significance does not require uniform performance" (1940, p.16). Of course, if performance were uniform, then the statistics would never have been required at all. His second move is to claim that this "decline effect" is an important aspect of the paranormal: "This decline evidence has all the merits of a confirmatory experiment" (1948, p.138). This decline in scoring, which may be nothing more than regression to the mean, has, in Rhine's hands, become another "paranormal effect".

Hyman has referred to this tendency as the "patchwork quilt fallacy" (Frazier, 1985, p.198). Anything that is found in an experiment "becomes part of your data". Thus scores below chance are cases of psi missing, scores hitting the previous or the next target are cases of displacement. Given such latitude in the post-experimental analysis of results, it is difficult to fail to find something to report.

Although many scientists have focused on the repeatability issue as being the central problem, it is actually a mere symptom of what is wrong. The lack of theory combined with the absence of regularities together constitute the main obstacle in the way of the acceptance of parapsychology as a science. Lack of repeatability is, of itself, disturbing, but it is not crucial. Until we have theory-based predictions, the extent to which experiments fall short of being actual replications cannot even be measured.

The factors that may be relevant in any one experiment are simply not known, and the theoretical framework of parapsychology is not adequate to the task of providing even a brief list of them. We do not know if it makes a difference to the outcome if the experiment is run on a
Tuesday rather than a Friday. We could run the same experiment over and over again, but even if we got the same scores on some occasions, we could not be sure that we had actually repeated the experiment in all its significant particulars. Parapsychology has established no coherent account of what might be going on in its experiments, nor any recipes for bringing psi about. Claiming that the experimenters themselves unconsciously use their own psi to enhance or inhibit the scoring - the so-called experimenter effect - begins to look more like an excuse for failure than a genuine scientific hypothesis.

Playfair (1984, p.3), drawing on the unpublished work of Batcheldor, suggests that recording events on "audio tape does not seem to inhibit psi phenomena, [whereas] video certainly does ... there has to be a 'loophole' to allow for differing interpretations of a given event, if there is a mind involved that requires such a loophole". Unfortunately, to the sceptic, such a suggestion sounds very much like a variant of the parental admonition that children who don't believe in fairies will never be able to see them.
CHAPTER FOURTEEN

EXPERIMENTAL CASES:

III. THE QUEST FOR SCIENTIFIC STATUS

Given that parapsychology has amassed a great quantity of data, but still has no serious theories, how does its claim to scientific status stand? In this chapter, I shall argue that it has failed. I shall pay particular attention to the inability of parapsychology to make testable distinctions between different phenomena that it investigates. I shall go on to discuss its claim in the context of the work of three recent philosophers of science, and show that in each case, parapsychology falls short of their requirements. Finally, I shall consider the kind of stance we should take towards the experimental work.

The crucial task for parapsychology in interpreting its experimental results is to decide on the closely allied matters of differentiation and reduction of phenomena. It must ask if it is possible to differentiate between the various categories of psi phenomena to a meaningful degree, and also if it is desirable to reduce these categories to a number of more basic types. We need to know not just that something odd is going on, but that it is going on in such and such a way, and for so and so reasons.

The problem of differentiation has recurred throughout the history of psychical research (I have already discussed it in the context of the survival question,
and followed Dodd's argument that ESP must be considered at least as plausible a paranormal explanation as survival for the mediumship cases). As early as 1897, Podmore, in the course of a discussion of Reichenbach's experiments, was led to ponder the matter (p.92). He begins by taking a sceptical position towards the phenomena, but then proposes that he might be prepared to accept them, at least for the sake of discussion. What, then, might be going on? He doubts the wisdom of accepting the proffered explanation for the results (couched in terms of "luminous emanations" from magnets - an unlikely idea, on the face of it) and declares that they could equally well be glossed as "thought transference from the minds of the experimenters". In making this observation, Podmore may well have been succumbing to a feeling that, since the idea of thought transference made more sense to him than did luminous magnets, then the former explanation was the more plausible (Podmore was, of course, a leading member of the SPR in its early days, and played a prominent role in its early telepathy experiments). But he may have been groping to the more radical position of accepting that, given the evidence, and the framework within which we have to interpret that evidence, we just cannot say one way or the other. Knowing Podmore's capacity for scepticism, I should suspect the latter.

Whichever interpretation might be the case, Podmore's problem highlights the difficulties involved in differentiation. Rhine suffered similar problems. In 1930, he noticed that the distinction that had hitherto been glibly made between telepathy and clairvoyance was to say the least problematic. He was obliged to subsume both under the common label GESP (general extrasensory perception). He then went on, quite illegitimately, to
assume that they were "essentially the same ability" (1948, p. 44), but it may have been the paucity of his conceptual framework, or the inadequacy of his experimental designs, rather than some inherent quality in the phenomena themselves, that led to his taking this position. The experimental methods adopted and the assumptions underlying them guaranteed from the outset that no distinction could be drawn at all. H.H. Price notes (Wheatley, 1976, p. 121) that it is "not at all easy in practice to distinguish clairvoyance from telepathy". Because of the way things have been set up, it is actually impossible. As Stauffer notes: "the minute the clairvoyant effect is verified", an interpretation in terms of precognitive telepathy can be substituted (1977, p. 84).

Even ESP and PK cannot be clearly distinguished. Take a case of "dice influencing". Here the experimenter sets the targets, and the subject is supposed to influence the fall of the dice so that a preponderance of the target is cast. The obvious interpretation of statistically significant scores would be that the subject was offering up energy to the dice, and thus causing the deviant results. But it could just as plausibly be argued that precognition was the vital factor: the experimenter, unconsciously precognizing the scoresheets that would have been compiled during the experiments, was in a position to select just those targets that the dice would favour in that run. For all the "willing" on the part of the so-called subject of the experiment, she made no actual contribution to the paranormal effect.

Once we admit the very possibility of the paranormal, it seems that our conceptual framework begins to crack, and "explanations" can be multiplied without limit.
Thouless (1972, p.192) suggests that procedures "independent of human activity" (such as temperature changes recorded in daily weather reports) might be used for preparing random targets. In this way, he feels, the plausibility of the argument from experimenter contamination is reduced, though he admits that there must still be a lurking doubt (one can imagine PK being used to change the level of the mercury in the thermometers). He suggests, though, that in certain cases it is legitimate to prefer one interpretation over another. "Yet obviously", he says, "the PK explanation of success ... would be very much the more probable since the alternative precognition explanation would require that precognition was working with an efficiency such as has never been observed in an experiment designed to measure precognition".

This is nothing but question-begging: the experiments that were designed to measure precognition do not necessarily measure anything of the kind. There is, for example, the possibility that the future event may have retroactively influenced the brain of the so-called precognizer by PK - a case of retroactive PK rather than precognition. Thouless, in his attempt to solve this problem of alternative explanations, cannot simply extract one thread - precognition - from the others and take it as his fixed point of reference.

In recent years, the parapsychologists have made no progress with the problem. Consider the following extract from an interview with Richard Broughton by Pat Spivey (1983, p.4):

R.B. - I devised my first real computer psi game, back in '77.
P.S. - Was it a PK game?
R.B. - That was a psychokinesis game. Well, it is hard to say what it was actually. It was probably psychokinesis, but it could have been precognition.

Attempts to draw such distinctions have failed. One of the earliest was carried out by McMahan (1946). She believed that her experiment would be one in pure telepathy. The targets would be written down in coded form, the code being known only to herself and her assistant. In this way, she assumed, the list of targets would not be available for "clairvoyant inspection" by her experimental subjects. What McMahan could not rule out, however, was the possibility that the subjects might, unconsciously, make use of a two-pronged attack to uncover the coded targets by clairvoyance, and the code itself by telepathy. They might even have used precognitive clairvoyance to check the list of decoded targets that McMahan would prepare (for the purposes of checking) after the experiment itself had been carried out. As it stands, her design was far less watertight than she imagined.

Schmidt, in one of his mechanized experiments (1972), has used a specially constructed apparatus which can, he assumes, test for either PK or precognition. In the experiment, the machine is, unknown to the subject, occasionally switched from one mode to the other. Differences in scoring patterns reveal which mode the subject has been using. The apparatus (which is a random event generator) should be checked regularly to ensure that its output of targets is truly random. Such tests, however, depend upon a crucial assumption. The machine is simply left running in order to generate a series of results. If this is found to be random, then the machine is taken to be performing correctly. Psi, we have to assume, only enters the picture when a
subject is seated before the machine for the purpose of making an experiment. But Schmidt's device might be consistently malfunctioning, and producing non-random sequences of the kind reported in the experiments. Only when the machine is given a dry run, for the purpose of testing, is this malfunction depressed, by the action of Schmidt's unconscious psychokinesis, hence producing, for the first time, a random sequence.

Braude (1979, p.32) allows that it is "difficult to distinguish experimentally between real-time and precognitive modes of psi". This "difficulty" arises ex hypothesi, and it is no answer to say that a particular experiment proves that psi occurs, but cannot (as yet) put forward anything more about it: parapsychology, on such an account, would be a barren little science. If the completely pure experiment is a "theoretical impossibility" (p.30), is not the theory itself in great need of rethinking? It is clear that attempts at differentiation cannot be regarded as safe.

These problems of differentiation are not confined to the experimental cases, but can occur in spontaneous cases too. Take, as an example, the sinking of the "Titanic" (a favourite with psychics). Some claims to have precognized the disaster have been made, and, assuming that we do not wish to invoke coincidence as the most likely explanation, precognition will be regarded as the obvious candidate. But that is not the only possible paranormal explanation that we could put forward. Consider the following: the course that the "Titanic" was to take on its maiden voyage was determined well in advance on its sailing, and would be known to numerous people. The presence of large icebergs and the seasonal patterns of winds and currents in that part of the ocean was also known to
various specialists in meteorology, seamanship, and oceanography. Had this knowledge been pooled before the "Titanic" sailed, then it might have been realized that there was considerable danger in sailing by that particular route.

As I have said, someone predicting disaster on the eve of sailing would be assumed to be using precognition. But there is nothing to prevent us putting forward an explanation in terms of multiple simultaneous telepathy, in that the predictor obtained her information from the several experts who were in possession of it. She herself cannot say whence it came, since the unconscious nature of psi precludes any firm evidence on this point. I do not say that either explanation is satisfactory, or that one is more preferable than the other; I do ask how parapsychologists propose to judge between them.

To his credit, J.B. Rhine came to realize, at the end of a long career, that there was something radically wrong with the "science" that he had done so much to develop. In a paper entitled (significantly) Telepathy and other untestable hypotheses (1974), he admits that telepathy "remains only an interesting speculation, one that cannot be confirmed by any known method" (p.145). But why, one might wonder, did it take Rhine so long to discover the problem? He seems to feel that the difficulties posed are of a purely practical nature - a "severe challenge", which, "in the interest of progress", is unavoidable (p.138). But the issue is raised by the experimental programme itself. The use of behaviouristic techniques and the assumption of the principle of unconsciousness both undermine any attempts to differentiate.
The problem has been taken up by Meehl (1978a). He describes a simple test which will give us grounds for preferring either PK or precognition in the interpretation of particular experiments, though he admits that it does not settle the matter conclusively. If we have a random event generator (REG) controlled by a radioactive source, we should expect it to produce a series of targets that is genuinely random. Let us assume that it is programmed to generate two-valued targets. The experimental subject is asked to guess what the next target is going to be. This would seem to be an experiment in precognition, but it could also be taken as an attempt on the part of the subject to influence the working of the REG by PK. On the face of it, we cannot be certain which description is to be preferred. However, Meehl argues that we can arrive at a solution, which, while not absolutely certain, is more than merely persuasive.

Let us assume that the subject scores a statistically significant number of hits over an extensive series. In order to differentiate between the two interpretations, we have only to examine the list of targets. Meehl points out that if the list follows a purely random sequence (as we should expect, if our knowledge of the physics of radioactivity is accurate), then we should assume that the subject was using precognitive clairvoyance. If, on the other hand, the target list shows significant deviations from the random pattern, then we should assume that the subject was using PK to alter the sequence of physical events.

However, Meehl's test is not without certain difficulties. Although the reasoning behind it sounds very reasonable and straightforward, it ultimately fails to provide an adequate method for
differentiation. The procedure depends on the assessment of randomness in a long run of trials (this is necessary because we cannot take a single target in isolation and say whether it is random or not). Meehl's test can only have application for whole series. We can say, following Meehl, that a series seems, on the whole, to be a case of PK, but there can be no guarantee that all the correct individual calls within that series are PK. There may be a few stray examples of precognition lurking that will serve to defeat the analysis. Meehl's test is bedevilled by a similar difficulty to that found in the well-known problem of differentiating between an ordinary guess and a paranormal one. At the level of individual calls, there is no way of doing so.

Meehl assumes that the principle of economy may be invoked to settle the matter: there must be just one explanation that is tenable – the correct calls must be due to either PK or clairvoyance. Further, there is assumed to be only one "subject" involved in the experiment. Both assumptions need not be the case. One could imagine that someone who is remote from the experiment is unconsciously interfering with the REG by PK, and that the "subject" then precognizes the altered targets. On Meehl's criterion, we should simply judge the subject to be using PK.

If the models of precognition and PK that are subscribed to by most psychical researchers had any cash value, then we could settle the question of differentiation very quickly. For in PK, we are supposed to do something, and in clairvoyance, we are supposed to think something. The difference would seem to be clear-cut. Unfortunately, the unconsciousness
principle renders any attempt at finding out what is going on impossible.

The application of the principle of economy - Ockham’s razor - is frequently recommended in psychical research, both by sceptics and, occasionally, by believers such as Meehl, as though it would solve all problems. It will not. Ockham’s razor is a device for judging the efficiency of competing explanations; of itself, it can settle nothing. Given two or more explanations, we can cut away the inefficient ones, leaving the one which is simplest. That does not mean that this explanation is right and the others wrong: it indicates that we should examine it before the others, and design our experiments on the basis of it. Should it fail, we look to the other candidates, which will, because of their greater complexity, require more sophisticated experimental design. In the context of experimental sciences, Ockham’s razor can suggest a research strategy, but it cannot, in advance of the results, settle the matter one way or the other. Those psychical researchers who, like Meehl, use it as an aid to understanding cannot rely on it to any extent, and even the sceptic’s use (intended to nudge us in the direction of an explanation couched in terms of fraud or scientific ineptitude) while it is telling, should not be regarded as conclusive.

Prompted by the difficulties involved in making sound, testable distinctions between the various kinds of phenomena that are assumed to occur, some psychical researchers have asked if it might not be possible to reduce the number of possible candidates. In this way, cases might be reinterpreted as, say, types of PK in its various forms. But as Scriven has said: "we have not achieved much success with the reductionist path:
although possible in principle, independent confirmation has been lacking" (Thakur, 1976, p.188). I am not sure that Scriven is right in asserting that "independent confirmation" of any particular form of reduction is actually possible, and I should welcome some examples. Given that psi operates without any conscious awareness, the possibility that we might find just one mode of operation that resists reinterpretation in terms of another mode seems doubtful. This doubt arises not because, as Scriven suggests, there has been a lack of empirical success, which, with a little extra work, we shall be in a position to reverse. The reason for this failure lies in the way that the experimental research programme was set up. In parapsychology, we have models of what is taking place, and we have experimental methods. But models and methods fail to mesh in one with another. Experiment in parapsychology is incapable of gaining a purchase on the very questions that it was designed to answer.

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Can parapsychology lay any satisfactory claim to scientific status? How does it measure up when compared with the stipulations of some influential philosophers of science? I shall consider its position with respect to the theories of three major figures: Popper, Lakatos, and Kuhn.

The Popperian view (1959) makes the experimental falsification of theories the touchstone of the scientific enterprise. Bold conjectures, being more amenable to falsification than modest proposals, are to be welcomed as examples of confident risk-taking. Once a proposal has been tested and found wanting, however,
ad hoc shifts must not be made in the hope of salvaging the falsified theory (the introduction of epicycles into Ptolemaic theory is an historical case in point here).

Now psi, it might be said, is a very bold conjecture indeed. It will not, however, meet Popper's criterion, for while boldness is a prerequisite, so also is falsifiability. To show that they have scientific content, conjectures must make themselves vulnerable to falsification. In experimental parapsychology, not only are the proofs doubtful, there seems to be no sort of disproof either. A negative result in an experiment is not taken as showing that psi does not take place, but dismissed as an occasion when negative vibrations or the experimenter effect have blocked out the "real" psi that was, we are asked to assume, going on all the time.

Lakatos (1978) is more accommodating to ad hoc shifts. He takes the view that there is a "protective belt" that surrounds scientific theories, rendering them partially immune from the rigorous falsificationism advocated by Popper. However, Lakatos explains how this protective belt functions by reference to examples from a developed theory (that of orbital planetary motion and perturbations). The shifts that he describes are not truly ad hoc, rather, they are special conditions which are allowed for by the theory, and which can be worked out in its context. To allow such moves is not to overthrow the theory (as Popper suggests) but to use it creatively to allow for special cases.

Lakatos makes an important distinction between "progressive" and "degenerating" research programmes, based on the extent to which a programme's theoretical
growth anticipates its empirical findings. Roughly, a programme where experiment is theory-led can be said to be progressive, and one where results pile up without theoretical foundations is degenerating. Parapsychology measures up poorly against such a yardstick. It fails to predict "novel facts with some success" (Lakatos, 1978, p.112), and its theoretical growth is by general consent almost non-existent.

Not only does theory fail to explain experimental results, the results themselves seem to be subject to periodic downwards revision. Stokes (1984), in reviewing a collection of essays by Martin Gardner (1983), protests at his highlighting of Rhine's early experiments \(^*\) (including the Pearce-Pratt series, earlier called into question by Hansel), and complains that Gardner is sedulously flogging a "dead (and decaying) horse" (1984, p.89). Such a view is disturbing. Not so very long ago, these experiments were cried up by parapsychologists as providing much more than merely persuasive evidence for psi. Only isolated sceptics voiced informed doubts (and were sometimes criticized for doing so). Eventually, after years of attrition, the parapsychologists themselves seem to have accepted that there might be something unsatisfactory about this work (and also, indeed, about Soal's experiments).

Now it must be said that the early experimental work of Rhine and Soal constitutes a significant part of the very foundations upon which the edifice of parapsychology has been built, and parapsychologists, at the time, seemed to be quite happy with the results. To dismiss these experiments, half a century on, as a "dead horse" could be taken as suggesting that, before too long, more recent experiments might be subject to a similar process of rejection. The point is that Rhine...
and Soal conducted their experiments using essentially the same underlying assumptions that are made today. To be sure, the new techniques may rather more reliable, but the original card guessing work is of a piece with the technologically sophisticated experiments using random number generators. If we are to accept that Rhine's early work is a "dead horse"; then we are forced to the unpalatable conclusion that his results were due to either fraud or incompetence. This allegation cannot be shrugged off lightly.

Kuhn's seminal idea of the scientific paradigm (1970) has been adopted by a number of psychical researchers. Its use may, however, give rise to more problems than it solves. There is much loose talk of paradigms in contexts where it is quite inappropriate. Consider Inglis (1977, pp.12-13): "Kuhn's theme offered a way out of the dilemma posed by the rejection of the supernatural. What I have done is work within a hypothetical paradigm, writing on an 'as if' assumption - the events being related as if they may have occurred". This is a misapplication of Kuhn's idea. Inglis helps himself to the concept of a paradigm for the purpose of retailing a considerable body of purely anecdotal cases, which fall by definition outside the range of scientific discourse. If such things as Inglis relates actually took place, then, should he wish to furnish a scientific explanation of them, he would still have to offer a truly Kuhnian paradigm within which they could be comprehended. Playing a game of "let's pretend..." is not enough. His "hypothetical paradigm" is actually a piece of blatant question-begging, and is quite the wrong assumption to make at the outset of such a work.
Can we say that parapsychology has anything approaching a real paradigm? There seems to be no consensus of opinion here. At the 1976 Parapsychology Foundation conference (Shapin, 1977), which took as its theme "the philosophy of parapsychology", no less than three papers discussed paradigms in some detail. Stanford (p.1) is prepared to allow that there is a "paradigm (perhaps, in almost a Kuhnian sense)" in parapsychology. Edge, however, calls for a new paradigm in physics, which will encompass paranormal phenomena, and Thakur notes the "'theoretical anarchy' within contemporary parapsychology", which suggests that research is not yet paradigm-based (p.199). The divergence of opinions here is a strong indication that there is so far no clearly developed Kuhnian paradigm.

Parapsychology has so far failed to provide satisfactory testable predictions and recipes. All it can offer is a collection of unexplained (and possibly unrelated) data. One searches in vain for something in the nature of mechanisms or causal links. For some, this is taken to be a profound and exciting indicator of the revolutionary nature of parapsychology. Both Rhine and Koestler attack Newtonian, mechanistic thinking as crude and outmoded. They forget, though, that Newton, in his lifetime, was himself attacked because his proposed mechanism, which implied action at a distance, was occult (Hesse, 1961). The significant difference is that Newton's theories generated testable predictions. Parapsychology seems to offer us little more than oddities.

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So what stance should we take towards the reports that are made by the parapsychologists? Beloff (Ludwig,
1978, pp.353-370) has proposed that there are five basic explanatory approaches to the experimental data: (1) Outright scepticism, whereby the results are explained away; (2) Flewism, which accepts the results, but denies that anything particularly interesting can be said about them; (3) Normalization, in which parapsychology is eventually integrated within the mainstream of science; (4) The acceptance that parapsychology must be regarded as lying outside the province of science; (5) Some form of synchronicity, as outlined by Jung.

As I said in the opening chapter, Option (1) - outright scepticism - may well be equal to explaining away most, if not all, the data, but it signally fails to impress the psychical researchers themselves. As long as the controversy continues, parapsychology has no hope of being accepted by the majority of scientists.

Option (5), that of synchronicity, has, since Beloff originally wrote his article, been decisively disposed of by Braude (1979, pp.217-241). Of the proposals offered by Beloff, that leaves only (2), (3), and (4).

It should be noted that of these three, indeed of all the five, options, only (3) makes any pretensions of offering us anything like a science of psychical research. But so many of the phenomena that are claimed to occur are so strikingly bizarre that they seem to demand (if we take them seriously) an explanation that lies beyond what mainstream science is likely to be able to offer. The sort of anomalies with which psychical research deals are very different from those which Kuhn suggests are to be found at the fringes of an obsolete paradigm. It is not that such things do not
quite fit the current paradigm of, say, physics - they lie completely outside it. Physics does not deny them, because physics has nothing to say about them. Carrington (1932, p.61) claims that "ordinary science ... contends that there are no haunted houses at all". But this is not the case: science attempts to explain the world without recourse to such "explanatory devices" as haunted houses, and does so remarkably successfully. On the subject of haunted houses, it has no fixed opinion (individual scientists may have, of course, just as they may have views on the existence of God). If we look up "haunted houses" in the indexes to scientific textbooks, we will find no references to support Carrington's contention, because such matters are simply not comprehended in such books. To try to redraw the boundaries of science so as to include them would not be to generate a broader-based discipline, a "science with added paranormality". Rather, it would be to destroy the coherence of science, perhaps so drastically that there would no longer be any scientific framework left into which the paranormal could be fitted.

Option (3) takes the view that paranormal phenomena are (at bottom) law-governed. As such, given a concerted effort, their secret cannot long remain hidden. But where are the laws, or even rough approximations to them? Because of the unconscious nature of psi, parapsychology is estopped from investigating the regularities underlying its phenomena. The use of devices such as Mill's canons is ruled out - such techniques as concomitant variation, in which the experiment is repeated while different factors are varied in turn - are not only not used in parapsychology, they cannot be used. The reason for this is not that the lack of repeatability makes it
difficult; we simply do not know what factors may be relevant, and we have no way of finding out.

But if we were to subscribe to option (4), and admit that parapsychology deals in nothing more than the supernatural, the anomalous, then what attitude should it take to the laws of science and, more generally, the laws of nature? If paranormal phenomena contravene these laws, then there are two possibilities; either the laws are themselves drawn too tightly, or these laws are subject to occasional suspension, allowing the paranormal to break through.

Parapsychology has to clarify how it stands with respect to these different possibilities. Does it take the "nomological" line of (3), or does it prefer the "supernatural" view of (4) - and if so, what is its attitude to laws of nature? Can it redraw these, giving them broader applicability, or does it opt for what we might term "suspensionism"?

Of all Beloff's five categories, I am inclined to opt for Flewism as being the one that creates the least intellectual discomfort. If standard scepticism is the atheist response to psychical research, then the form of Flewism that I espouse is the agnostic version (in T.H. Huxley's original sense: it is not that the evidence is so far incomplete, it is that, given the nature of the evidence, we just cannot say whether there is anything serious in it or not). In the next chapter, I shall explore the reasons for my taking this sceptical position.
PART FOUR

CONCLUSIONS
We have seen that lack of repeatability and the evanescence of paranormal phenomena have been traditional targets for criticism by the sceptic. These are, however, merely symptoms of the problem: it is the unconsciousness principle that undermines the scientific pretensions of parapsychology. If we were to follow the psychical researchers, we would have to allow that the results of their experiments point to the conclusions that human beings can obtain information from one another by mind-to-mind contact without perceptual mediation, discover information about material objects without employing their sensory organs, and move objects (without touching them) by means of sheer willpower. But if we consider the implications of the unconsciousness principle, we can see that psychical research can actually give us no such assurances.

Psychical research is prepared to allow that the unconsciousness principle may be admitted, but only in a very attenuated form. The name usually given to it is "the experimenter effect". While it can offer no recipes for bringing about its results, no necessary conditions for paranormal events, and is unable to isolate any significant variables, psychical research is prepared to say that its evanescent, unrepeatable phenomena may be affected by some kind of experimenter effect.
What is an experimenter effect? Social scientists are aware that the outcome of an experiment can be affected by the attitudes and preconceptions of the experimenter. Self-fulfilling prophecies, the Pygmalion effect, and the Rosenthal effect are well-known cases of the desired result being brought about by the influence of the experimenter. Braude, in a discussion of this matter (1979, pp.32-41), concentrates on such difficulties, which are not unimportant, but of little special interest for parapsychology. He virtually ignores a second form of experimenter effect, however; one that is crucial to parapsychology.

The classic study which made this problem clearly apparent is that reported in 1953 by West and Fisk. West and Fisk alternated as experimenters in a card guessing series. Their subjects themselves were not made aware of this alternation (the experiment, using sealed packs of target cards, was carried out by post). Generally speaking, West's results showed a negative deviation from chance, but not one that was statistically significant, whereas those of Fisk were both positive and significant. They argue that "This suggests that D.J.W. [West] is a jinx and gets only null results, for the scores of his section have only diluted the better results obtained when G.W.F. [Fisk] was experimenter" (1953, p.186).

The West/Fisk experiment thus suggests that some experimenters may have an inhibiting effect on the psi of their experimental subjects. This, it is thought, might account for some consistent failures to score. For instance, Beloff, a competent and careful experimenter and a believer in the paranormal, consistently fails to secure positive results. The question of why some fail rather than why some succeed
is, perhaps not surprisingly, considered with greater enthusiasm in certain quarters. Thus Haynes (1982, p. 11) takes it for granted that the "experimenter effect is to be interpreted as being one of inhibition by the experimenter: an attitude of caution is presumed to be non-conducive of psi in the subject". Here, the suggestion is that the subject can be put off by too stringent safeguards and a coldly scientific atmosphere during the experiment. Psi, it is implied, is some kind of ability or performance, and one that can readily be inhibited.

Now we know how to apply the concept of inhibition in "normal" cases, and are aware of the signs that mark its presence: the subject may fidget nervously, speak incoherently, avoid our gaze. In the experiments, however, none of these typical indicators need be present. All that happens is that the subject does not get quite as many guesses right as we might have hoped. Now if the inhibition that is talked about were anything like the inhibition that we are familiar with, it should be possible to overcome it by means of habituation. Sheer practice and familiarity ought to banish any inhibition. However, this does not seem to occur, and Haynes's use of the term has really little more than metaphorical significance.

Other parapsychologists have posited the more radical view that some experimenters might themselves unconsciously block out the psi signals with their own psi, presumably in a similar way to a radio station "jamming" another by broadcasting noise on the same frequency. Sargent, for example, explicitly canvasses such a possibility in a paper read to the SPR (Inglis, 1981). The idea cannot be discounted, though it must be
said that it creates nice problems of experimental design.

The difficulty, and a proposed solution to it, have been framed as follows by White: "if psi is a reality, it would be impossible to rule out the experimenter in the results of any investigation. Therefore, we must do the next best thing: delineate in as much detail as possible exactly what the experimenter's influence is and how far it can be expressed. ... We cannot obtain reliable information about the subjects in psi experiments ... until we learn how much of the subject's response is attributable to the experimenter" (Wolman, 1977, p.298). But this position cannot be sustained. Because of the unconsciousness principle and the behaviouristic nature of the experiments, we are precluded from discovering the extent to which an experimenter may influence the outcome of an experiment. We may feel that common sense bids us make assumptions, but there is no method of identifying with any certainty who (or, indeed, what) is "participating" in a parapsychology experiment.

What has happened here is that the picture of an "agent" and a "patient", controlled by an experimenter, has been unquestioningly accepted. The agent transmits, the patient receives, and the experimenter records and interprets. But then repeatable results are not obtained. The experimenter, being the only member of the trio not so far implicated in the paranormal transaction, is then included, as an ad hoc explanatory device, to save the appearances.

Although the experimenter effect is superficially attractive to psychical researchers, there is, as Girden has said, nothing to support it: "whatever the
views of the possible role of the experimenter in psi tests, they are pure speculation. There has been no experimental evidence that an experimenter has ever had such an effect in ... tests (Carterette, 1978, p.405).

But what might such evidence be like? If psi is unconscious, then how are we to establish that the experimenter is having any influence at all? To ask him to stay away from the building during the experiment is no answer, since psi appears to operate without regard for spatial distance. To obtain from him a sworn affidavit that at no time did he influence the experiment by means of his own psi will not help either, since, if psi is unconscious, how could he know? If psychical researchers have failed to find a reliable method for bringing psi about, they can hardly be in a position to exclude it.

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Broad once suggested that, if only clairvoyance were a conscious faculty, then those who possessed it would be "invaluable helpers in physical laboratories" (1953, p.45). It is not, and the problems for science of that fact are potentially (but only potentially) devastating.

Inglis (Roll, 1981, pp.148-149) goes so far as to suggest that some experiments in conventional science may have been contaminated by experimenter psi. Citing Neal Miller's research into biofeedback (which had subsequently proved impossible to replicate), Inglis speculates that perhaps Miller, or one of his assistants, might (unconsciously) have brought about positive results by psi.
Inglis fails to allow for psi contamination from a quarter remote in both space and time from Miller's laboratory. Like White, he would rather take the easy course, and assume that only Miller or his associates could have any kind of psi influence on the results. (It should be said that the invocation of psi in such experiments, especially in a field of research that is notoriously beset by pitfalls, is little more than speculation at the best; while it cannot be ruled out, it need not be taken too seriously).

The problem of psi contamination by an experimenter in orthodox science has also been discussed by Mundle (1973), with reference to Mendel's celebrated work in plant genetics. He reminds us that Fisher was able to demonstrate that Mendel's results are suspect, because they are just too neat. It is possible that Mendel tidied up his figures, or, as Hardy suggests, that Mendel's gardeners gave him the results that he had already told them that he was expecting (see Koestler, 1971, p.56). Mundle goes further, and - tentatively - suggests that Mendel might have used psychokinesis to influence the growth of his seedlings in the desired ratio. This is certainly a radical proposal: other considerations apart, one might ask what had prompted Mendel to prefer that particular ratio to such an extent that his unconscious PK affected his seedlings, and why they then grew precisely in accordance with that ratio. A great deal is being taken for granted here.

Such extravagant proposals are not rare in psychical research: Pratt, in his response to Markwick's critique of Soal's card guessing work, displays a similar tendency to prefer a paranormal account even when a more straightforward version could readily be offered.
(Pratt, 1978). Rather than accept that Soal (consciously or unconsciously) tampered with the target series, Pratt proposes that Soal was his own experimental subject, and in setting his targets was precognizing what Shackleton was going to guess.

Schmeidler (Wolman, 1977, p.148), discussing Spencer Brown's work with random number tables, remarks that "he found significant correspondences, followed by declining correspondences. They supported his argument but alternatively could be interpreted as showing his successful use of ESP (or PK) in his first choices of the 'right' places to enter the table". Schmeidler derides Spencer Brown's method of using the tables as "naive", but this is to miss the point. Spencer Brown is arguing that psi does not have a separate existence, independent of the statistics (as Girden has said, "operationally, psi is a statistic" (Carterette, 1978, p.406)). In her too ready assumption of the reality of psi, it is Schmeidler herself who is being naive.

Once such a position has been reached, it is hard not to see the potential for psi everywhere. The difficulty with such a posture, apart from its wilful credulity, is that it relies not only on the acceptance of psi, but also on the evidence of the experiments that are supposed to have established its reality and its efficacy. But then, how do we know if these very experiments have not been contaminated by the unconscious psi of others? Once we accept the possibility of psi, on its own terms, we are estopped from saying anything useful about it, and the essential purpose of our experiments is defeated. Finding out how psi operates is rendered impossible.
Not only is parapsychology impossible; orthodox science itself must succumb. The claim that psychical research might actually overturn science can be read in two ways. First, it might mean that science will need to carry out considerable re-thinking in order to accommodate the findings, and that the resulting science will be very different from that which exists at present. Secondly, and much more strongly, it could mean that the operation of psi is so insidious that conventional science becomes impossible. The unconsciousness principle, which allows that psi is ex hypothesi undetectable and can operate from remote times and places, means that the operation of normal scientific procedures becomes impossible. If followed to its conclusion, such a position would lay waste the principles on which experimental science is based and invalidate, with the rigour of a Cartesian demon, every single experiment that had ever been conducted. Science would be impossible. This is, to say the least, a disturbing conclusion.

It is, however, relished by Inglis: "psi experimenter effect, if it can be demonstrated ... will hand you that sharp instrument, Occam's razor - for so long used against you, but henceforth a weapon in your own hands, with which the skeptics can be effectively castrated" (Roll, 1981, p.148). Inglis, in thinking that his prescription would undermine only those "orthodox psychologists" who criticize parapsychology (p.149) is understating his case. The image of castration is entirely appropriate: science becomes impossible if we accept Inglis's assumptions. There is just one problem: the experimenter effect cannot be demonstrated. In calling for an abandonment of "protocolitis", and a return to informal methods of research, Inglis misses the central problem. What needs to be dismantled is not
the experimental outlook, but the set of assumptions on which the experiments are based. His proposal, if followed, would create a form of psychical research amounting to little more than the unsystematic collection of bizarre stories.

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Because psi is unconscious, psychical research cannot gain a grip on it. The use of control groups in experiments, and the isolation of different variables are in principle impossible. To take any other view amounts to idleness or wishful thinking. Nonetheless, it is sometimes suggested that controlled experiments have been carried out.

A controlled experiment aims to isolate a particular factor in a causal process. Thus if, for example, a new drug to cure influenza is discovered, its efficacy might be tested by monitoring the progress of a group of sick patients, half of whom had been given the drug, and half of whom had not (the latter being termed the "control group"). By comparing the progress of the two groups, physicians would be in a position to assess the efficacy of the new drug. (It should be said that in parapsychology, the term "controls" can also be used to mean the precautions that are taken to exclude normal sensory perception of targets. This is not the sense that I shall discuss here).

Sargent finds a number of controlled experiments: "if you wanted to look for literature which has quite a range of control groups it would be the hypnosis literature where control groups have been much more frequently used. I think there are something like 23 comparisons of hypnosis and control groups in the
literature" (Shapin, 1983, p. 115). Against this rather economical view, Thouless says: "it would be tedious to list all the experiments that have been done with controls. They are very numerous" (1963, p. 112). But could any of these experiments have had controls at all?

Thouless suggests that they did: "the results of an ESP guessing experiment can for example, be compared with the results of observations which are like the experiment in every respect, except that the score obtained must be a chance one since it is obtained by a process in which ESP cannot enter" (1963, p. 110). Thus one might shuffle two Zener packs, and compare them card for card, counting each agreement between the packs as a hit. Then the hits could be compared with the hits scored in an experiment. It is hard to see what this would prove. In the first place, the card-matching ritual is not "like the experiment in every respect". It would (assuming it were carried on for long enough) result in a series of chance scores, as Thouless requires, though one wonders what exactly is the point of doing it, for the odds against chance can be calculated beforehand. And although Thouless demands that the process be one "in which ESP cannot enter", he has no way of excluding it. For all he knows, the cards may have been influenced by the "psychic shuffle", which is defined by Rhine (1948, p. 61) as being when ESP itself "is used in the act of shuffling, aiding in placing the cards so as to make them match" the targets.

How can Thouless tell that ESP cannot enter into the procedure? Psi is not significantly attenuated by distance: the inverse square law does not hold. Psi occurs without conscious awareness on the part of the
subject. And when we introduce temporal displacements, the prospects grow even more dismal. Let us imagine an experiment, apparently in simultaneous telepathy: subject S1 concentrates on individual Zener cards, and subject S2 writes down his guesses. They sit in two separate rooms in a building in London in 1980. The scores, over a long series, are interpreted as being indicative of psi hitting directly on the target.

On the face of it, this would seem to be a clear case of simultaneous telepathy. But is it? For let us further imagine that someone, call her X, is sitting in a building in New York in 1990. She is able, through retrocognitive clairvoyance, to have access to the sequence of cards that were the targets in the London experiment ten years before. She obtains this quite unconsciously, and is not even aware of it, so we need not think of her as "possessing information", still less of "having knowledge". She might spend her spare moments doodling with pen and paper, in a reverie - the phenomenon that is known as automatic writing. S2 is able, through precognitive clairvoyance, to obtain (again unconsciously) the results of X's doodlings, and so to record on his scoresheet sufficient correct hits in the series to convince his experimenter that a paranormal interpretation is called for. Psychical research cannot deny such a possibility, but neither can it confirm it. On its tenets, both these interpretations (and many others) are equally valid; one "explanation" is potentially as useful as any other.

It will doubtless be said that, if we are to entertain such lush proposals as these, the operation of particular kinds of psi would have to be very much more efficient in their working than experiments have shown
them to be (see, for example, Thouless, 1963, p. 85). This is mere question-begging: the experimental results that we have, and which we might think we have interpreted properly, cannot be used as a yardstick to measure other interpretations by. For we have no canons of interpretation in this area, no standards laid down that will license us to prefer one version of events over another: psi's modus operandi might be as simple or as complicated as we might care to imagine, but parapsychology is ex hypothesi incapable of arriving at any definitive solution.

In the everyday mental life of all human beings, there are many curious intrusions. As I walk down the street, I may imagine a sound or a smell which can be captured with a Proustian vividness. An idea, quite unbidden, may "pop into my head". These are common occurrences, and surprise no one. But could their origin be paranormal? Could it be that, just as I am locking my office door in the evening, and, for no obvious reason, begin humming to myself the trio from Act Three of "Der Rosenkavalier", someone is, somewhere, rehearsing it, or playing a record, and telepathically transmitting the sounds to me so that I begin to hum them? I may have thought that I was acting quite spontaneously, but perhaps I was the victim of ESP. "Why not?" some might say, but "how?" is the proper question.

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So what can psychical research do to alleviate the difficulties that stand in the way of its developing scientific hypotheses? Scriven (1956) suggests that, as we become more familiar with certain paranormal phenomena, we shall come to accept them more readily: any oddity that at first may have struck us will be
diminished. Hypnosis, for example, no longer seems supernatural to us, even though we know very little about it; familiarity has blunted the edge of its strangeness. In much the same way, Scriven suggests, as we become more familiar with the "supernatural", then we shall find it less disconcerting. This cautious stance has much to commend it: perhaps paranormal phenomena (if there be such) are beyond our present capacity for understanding, and the most we can hope to do is to record instances as and when we come across them. If so, there is something to be said for acknowledging as much, and dismantling the edifice of "parapsychological science".

Scriven's notion of "familiarity" is, however, not without its problems. He seems to be suggesting that if sufficient work is done, regardless of whether any understanding accrues as a result, then we shall have gained familiarity. But after more than thirty years, although the journals have continued to detail new research, familiarity seems as elusive as ever. "Familiar" can, however, be glossed in two quite different ways. We can claim to be familiar with the re-appearance of Hailey's comet, because it has been well-documented over many centuries and its orbit has been computed. But that does not make the re-appearance itself a familiar phenomenon at all - it happens far too rarely for that. It is familiar in the former sense because we have discovered the underlying regularities and causes, and we know what to expect. It would have to re-appear very much more frequently before we could claim any familiarity with it in the latter sense. And it appears that it is this latter sense that Scriven is asking for, for he explicitly states that understanding of the phenomenon is not essential. All Scriven offers is the vague promise that by carrying out many more
experiments, which yield roughly similar conclusions, we might feel less intellectual discomfort.

There are possibilities in the fields of physiology and psychology. Suppose that there were some neurophysiological event or process which was found in both the agent and the patient, and was constantly correlated with correct guesses. This would certainly be strongly suggestive, and would provide an exciting topic for further research. But it would only be a beginning. We should also have to show that the brains were not merely acting in temporal coincidence, but in a causally related way. In the absence of any conscious experience, this would be impossible. To show that no other human being was acting as an unconscious intermediary between the agent and the patient, we should have to test every single person, simultaneously with the experiment, to ascertain that none of them was also registering a similar brain event. Even this would fail to provide the requisite assurance, for we must also allow for the possibility of both retrocognitive and precognitive telepathy. In order to exclude the possibility that someone is unconsciously influencing the experiment from some remote epoch, we should have to run the brain test on everyone that ever lived or ever shall live, for every second of their lives.

If, however, psi were conscious - if there were some definite sensation or experience to mark out its occurrence - then it would long ago have been incorporated into the mainstream of science. One can imagine experiments being conducted, and arriving at some definite conclusions. After an experimental session, dialogue of the following sort might ensue:
Experimenter: Well, that was most satisfactory. You've done twenty complete runs this afternoon, and your scoring is quite impressive. You clearly have some degree of non-mediated perception, or NMP as we call it.

Subject: You should find around four correct calls per run, over and above the five that I should have got right by chance. On the last run we did, I think that calls 3, 5, 7, and 9 are the ones to look out for; I definitely felt the sensation then, a sort of tingling in the centre of my forehead. It's quite an odd feeling - one you can't mistake, really.

Experimenter: Good. In fact, you scored correctly on calls 2 to 8, and on 10 and 12. Numbers 2, 4, 6, 8, 10, and 12 were just lucky guesses, although you did get one more right than we should expect by chance. Anyway, there's no NMP experience there, so we can write them off. 3, 5, and 7 you got by NMP, but 9 you were wrong about, in spite of the experience. When we've done some neurological tests, we may find an answer to that problem.

The purpose of this little fantasy is to emphasize the fact parapsychology would have been a very different kind of science if there had been some psychological and neurological aspects to psi. Indirect inference seems, however, to be the only method of bringing it to light. By conducting its experiments at the macro-level, concentrating on molar events in human behaviour (such as the guessing of cards), parapsychology bids to maintain its scientific pretensions. But when we turn our attention to the micro-level (the introspection of psychological events, or the monitoring of neurophysiological processes), we find nothing consistent at all.
Parapsychology fails to come to grips with the very phenomena it has set out to study. The phenomena are simply left dangling, with neither theory nor model to explain them. The phenomenon of psi must be taken to be, at best, statistical. Although parapsychology might continue to carry out experiments, and infer that "something paranormal must have happened" whenever extra-chance scores are obtained, it will still fail to provide any proper understanding.
CHAPTER SIXTEEN

THE IMPOTENCE OF PSYCHICAL RESEARCH

Is psychical research a science? That is the central issue to which I have addressed myself. Traditional scepticism has often focused its attack on the phenomena themselves, and on their inherent improbability. As I have said in Chapter One, such charges fail to impress the psychical researchers themselves, and are often written off as mere prejudice. The right question here is not: do such things happen? We should rather ask: if they do happen, can we say anything significant about them? Only if we can say such things is there any hope for a science of psychical research.

As I have indicated, the failings of psychical research are to be found at a more fundamental level than mere experimental detail. If the metaphysical assumptions that lie behind an experiment are faulty, then whether someone might have cheated is not immediately relevant to our verdict. Bad metaphysics can bring forth only bad science.

The quality of science depends not only on sound foundations and the absence of fraud or sloppy procedures. Interpretations, as well as "raw facts", are involved here, and if those interpretations are tendentious, unwarranted, or at fault in some other way, then the experiments will not prove nearly as much as some people would like us to believe.
The "two person" dualist account of human nature is discredited, and it is surprising to see that it still survives, indeed flourishes, in psychical research. Because parapsychologists such as Rhine accept dualism, they are able (in behaviouristic experiments) to study just one half of the partnership, and then make all sorts of unwarranted assumptions about "the mind". This is seen most clearly in Rhine's naive and tendentious talk of "the soul hypothesis" (1948, pp.165-166).

We have to accept that psi is the name given to some statistical anomalies; ESP is not a form of sensation or perception, nor we can lay any claim to derive knowledge of the world from its operation. But these statistical quirks have been reified: an event or process is supposed to bring them about. Psi is the cause that they are all supposed to have in common.

But it is clear that, though many psychical researchers like to think of these things as being caused, they are unable to provide any satisfactory causal account of their operations. Psi is apparently uncaused and meaningless. Some parapsychologists appear to find this exciting. Talk of backwards causation must be regarded as premature. Indeed, the introduction of such a topic into the debate only serves as a smokescreen to conceal the absence of any seriously thought out causal connexions.

Spontaneous cases, which are prone to both overdescription and underdescription, are no sound basis for a scientific study. Nor can the observational cases be taken as reliable. There has been too much fraud, and too little serious theorizing to allow us to accept them as completely reliable. It could not be denied, though, that future discoveries might lead to a
revision of theory, in a way that would accommodate the prodigies of such as Daniel Home. In the meantime, however, there is nothing to be gained by disinterring long-forgotten accounts.

The metaphysical bases on which psychical research has been constructed are thus fatally flawed. So much the worse, the psychical researchers might say, for metaphysics. But the scientific techniques that are used are also flawed, in such a way as to make the scientific pursuit of the paranormal a self-defeating exercise.

Because there are neither psychological nor physiological changes associated with correct guessing of cards or with the willing of dice, and because psi apparently operates regardless of the limits of space and time, there is no hope of science gaining a purchase on the results. The terminology suggests pictures for which there are no tests. The models fail to mesh in with the methods. In "explaining" the results, we can substitute one picture for another, with no apparent gain or loss. Telepathy and psi are not even explanatory fictions: they offer no explanations.

If we accept the basic assumptions of psychical research, and allow that the alleged phenomena occur as described, then this "science" is seen to have an uncertainty principle built into it. For all the talk of theory, psi turns out to be only a statistical oddity: a literally anomalous occurrence. If we accept this view, then the problems that beset us disappear: the failure of parapsychology to explain its data can be understood.
Psychical research may continue to catalogue anomalies, and perhaps make attempts to classify them. This might be an interesting exercise, but its scientific worth is questionable. To say "we are investigating the unexplained, and here are our results: a miscellaneous rag-bag of curiosities" is hardly a statement of a viable research programme. To use the techniques of science, without a sound theoretical basis, to "see what happens", is a recipe for frustration.

If paranormal phenomena occur, then either they are law-governed or they are not. If they were law-governed, then we could investigate them systematically, and experimental parapsychology might become integrated within the mainstream of science. There are good reasons for thinking that not only will this not happen, but that it cannot happen. So what is left for psychical research? If there are no laws of the paranormal, then, should psychical research wish to continue its work, it must make a choice between two alternatives. If a paranormal phenomenon is taken to be one that transgresses a law of nature, then either the law of nature has just been too tightly drawn, and will need relaxing slightly, to allow for odd contingencies; or, more radically, the laws of nature must be assumed to be subject to occasional temporary suspension, allowing the paranormal to break through. I do not say I feel any particular attraction for either of these ideas. What I say most emphatically is that psychical research, if it is to be taken seriously, has got to make up its mind about this question.

But perhaps there is no transgression at all: perhaps all we have are, as I have suggested, a few quirks. We need not assume that science should explain everything. If I score seven out of twenty five, and the person who
concentrated on the cards says that I must have been in
direct, unmediated mind-to-mind communication with her
in order to do it, I shall probably feel that a law has
been broken in some way. If she says that I have shown
some anomalies in guessing, I will probably shrug, and
say that I was lucky.

What psychical research must have, if it is to make any
progress at all, is some definite psychological and
physiological data to examine. Then, by rigorous
application of such techniques as concomitant
variation, it might arrive at some proposals worthy of
our consideration. If, however, it persists in the
remorseless cataloguing of queer events, without
providing any satisfying explanatory structure within
which they can be comprehended, then it can be seen to
rest on a series of mistakes.
APPENDIX

A sense of the ridiculous

Skeptical Inquirer, 5(3), 51-56, 1980
A Sense of the Ridiculous

Where is it when we need it?

John A. Lord

In studies of the paranormal, we are frequently urged to maintain a due skepticism. I say “due,” because, of course, a total skepticism in the face of overwhelming evidence is unscientific and self-defeating. But the Committee for the Scientific Investigation of Claims of the Paranormal insists that paranormal claims not be rejected a priori, antecedent to inquiry. I want to suggest that this position is too liberal, because it places the onus of proof on the skeptic.

Part of the problem for most of us is that proper inquiry usually takes up a great deal of time, often costs money, requires considerable specialized knowledge and expertise, and may well induce a distinct feeling of sterility and wasted effort, especially when that effort could have been devoted to more obviously profitable studies. (Carl Sagan, for example, comments ruefully that the writing of his critique of Velikovsky diverted him from his mainstream research.1)

I am sure, though, that the scientists and magicians who carry out such work would justify it in two ways:

1. It fulfills a useful function by educating an apparently incredibly credulous public, showing them the errors to which so much paranormal research is prone (and at the same time countering the popular accusation that scientists are by nature aloof toward and

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biased against the "new paradigm").

2. It gives the critical investigator an opportunity to be involved in some possible (though rather improbable) major developments in our concepts of man and the universe. Marks and Kammann mention this a few times in the course of their recent book, and I am quite sure that James Randi would not begrudge a single penny of his $10,000 prize if, by awarding it, he could be the first to publish a cast-iron, replicable paranormal finding in the columns of, say, *Nature*: certainly it would guarantee him immortality in the history of science.

But what of the rest of us? How can those skeptics who are (like myself) neither trained scientists nor professional magicians hope to assess the claims that are constantly being made? Do we have a weapon in our armory that we can use to combat at least some of the wild claims that are made, not only by unscrupulous journalists but also by "hard-headed" scientists who work in laboratories?

I suggest that such a weapon is available: I suggest that most (if not all) skeptics possess a quality that is singularly lacking in many paranormalists and their followers. It is not simply that we are rather more, and they rather less, skeptical or critical. It is rather that we possess a sense of the ridiculous, whereas they do not. Anyone having this quality is less likely to be taken in by insubstantial claims. Of course it is not sufficient in all cases: there will always be some claims that will demand minute critical scrutiny, but it does at least rule out, at a stroke, the grosser manifestations of the paranormalists.

By "a sense of the ridiculous," I do not simply mean a sense of humor: too many critical writers have an unfortunate habit of lapsing into facetiousness when discussing the evidence for the paranormal, making feeble jokes at the expense of "ideas" that deserve more robust treatment.

Now it is true, as I have said, that many paranormal claims are not, on the face of it, a priori ridiculous: biorhythms would be a good example. It is an undisputed fact that certain organisms display periodicity, and given the existence of, say, the human menstrual cycle and of some circadian rhythms, it would be foolish (without further investigation) to laugh at the proposition that there are 23/28/33-day cycles in all human beings. Of course the suggestion that such cycles are constant and unchanging has a suspicious ring: it sounds altogether too procrustean. But the claim is not so ludicrous that it might not be true, and so it demands (and has been given) proper scientific investigation. Much the same might be said of the Gauquelins' claims for astrobiology.

What I have in mind when I speak of using a sense of the ridiculous are those cases that are so patently silly that to give them serious attention...
presumes a worth in them that they clearly do not possess. They are so
manifestly bankrupt that to give them critical consideration would be to
insult the readers of such criticism—and it would insult them by assuming
that, among other things, they did not have a proper sense of the
ridiculous.

Take Uri Geller, for example. I never saw his performances on British
television in the early seventies, but I subsequently gathered from the
newspapers that something very peculiar had taken place. Did I believe it
all? (I was fairly credulous at the time.) I can't actually recall caring very
much one way or the other. Of course Geller was subsequently exposed by
various investigators; but before I ever read their versions of what had
probably taken place, I came across a statement made by Dr. Puharich,
Geller's manager. He said that Uri derived his powers from computers
stowed in flying saucers from the planet Hoova! Further investigation was
now unnecessary. Uri was bending metal by either normal or paranormal
means: if normal, then it was a conjuring trick; if paranormal, then it was
due to some distinct and measurable physical forces and not, emphatically
not, to psychic vibrations from "Hoova" or anywhere else.

Another good case is that of Harry Price, the doyen of British ghost-
hunters, who founded his own "National Laboratory of Psychical Re-
search" and managed to create the impression that he was a critical,
skeptical practitioner. His greatest triumph was the investigation of Borley
Rectory (the "most haunted house in England"), about which he published
two best-selling books. The claims he made were backed by eloquent
testimony from responsible and respectable people, and for many years it
seemed as if Price had succeeded in producing a genuine record of well-
authenticated phenomena. There was a definite case to answer, and it was
answered decisively in two books that appeared after his death. From
these thorough, soundly researched books, Price emerges as a rogue and a
charlatan, a bare-faced liar and a falsifier of evidence.

The story badly needed telling, and the authors tell it superbly. But the
real problem is, should it have needed telling? The public was taken in by
Price's Borley books, and massively taken in at that. But should people
have been taken in at all? Not if they had any sense of the ridiculous,
coupled with a knowledge of Price's other activities. For at the time he was
engaged on his Borley investigation, Price published (in collaboration with

This book purported to be a serious inquiry into the case of Gef, an 84-
year-old talking mongoose from the Isle of Man. I should point out that,
not only have I never read this book, but I have no desire to do so (there
are, I think, better things I can do with my time). Now if Price were (as he
claimed to be) a serious critical investigator, how could he bring himself to
waste his talents on such palpable twaddle? And, if he didn't actually believe it for a moment, why take up the public's time with this nonsense? On the first assumption, Price would be reckoned a fool; on the second, a knave, and clearly not worth further consideration by any sane person with a sense of the ridiculous.

Turning our attention from talking mongooses to talking horses, we encounter Lady, who was investigated (and found genuine) by no less a pioneer than J. B. Rhine. So far, I am unimpressed, if slightly amused. Of course apologists might urge that this "research" was carried out early in Rhine's career—are we not all entitled to a few mistakes? On the other hand, we are always reminded that, when Rhine embarked upon his career as a parapsychologist, he was already a trained scientist (a botanist, in fact). In any case, Rhine continued to have no doubts about Lady. Nearly thirty years later, he wrote, with sublime innocence, "Experimental work . . . has already produced evidence of good exploratory character that at least one horse . . . has shown behavior consistent only with the psi hypothesis."

Rhine's major work has now been placed in a far less positive light by Hansel; but even so, we must ask how anyone could be deceived by such an obvious music-hall act as a talking horse. It is not simply that Rhine must have been an incompetent investigator, as Milbourne Christopher hints. Anyone who is taken in by such ridiculous pantomime games is clearly lacking a sense of the ridiculous, and how such a person could ever come to be regarded as a leader in his chosen field is beyond my comprehension.

Examples of such nonsense could be multiplied ad nauseam. I shall add just one more. When Betty Markwick finally demolished the already shaky experiments of S. G. Soal, J. G. Pratt made a reply. Markwick pointed out that the targets used by Soal were not compiled from random number tables as had been claimed. Instead, Soal, either consciously or unconsciously, had prepared targets that were anything but random. In his rejoinder, Pratt states:

I do not mind revealing that I am the person who suggested that Soal might have become his own subject on some occasions when preparing the list of random numbers on the record sheets before the sittings were held. This "explanation" [my scare quotes] would require that he used precognition when inserting digits into the columns of numbers he was copying down, unconsciously choosing numbers that would score hits on the calls the subject would make later. For me, this "experimenter psi" explanation makes more sense, psychologically, than saying that Soal consciously falsified his own records, but I do not argue that it should be accepted by others as the likely interpretation.
Faced with such willful credulity as this, what can one possibly say? The case is obviously more subtle than the others I have quoted, for Pratt is trespassing against more than our sense of the ridiculous—he is betraying his fundamental ignorance of a significant principle of the philosophy of science: ad hoc shifts should play no part in serious scientific discourse. If our theory does not account for observed events, we salvage what we can and abandon the rest. We do not engage in attempts to patch it up and claim that it still works but in a different way. I can only agree with E. J. Dingwall when he says that “perhaps Andrew Lang . . . was right, when he was reported as saying that sometimes it looked as if psychical research does somehow change and pervert the logical faculty of scientific minds.”

In the face of Pratt’s folly, I can only laugh: are not, after all, such reckless attempts to save a doomed experiment totally ridiculous?

But I do not expect that this will be appreciated by the believers, for who cares to be laughed at? Am I not myself, they may ask, being ridiculous: over-incredulous, and unscientific into the bargain? I can only reply that a critical treatment of claims as absurd as those I have instanced would be a totally inappropriate response. If someone wants to tell me that dice experiments show that some people are luckier than others, or that card-guessing experiments demonstrate that some people are better at guessing than they ought to be, then that is fine: at least it gives us something solid to bite on, something that might be worthy of critical investigation. But flying saucers from Hoova and talking mongooses? No, the only proper response to such ridiculous claims is to laugh them out of court. Serious argument would be a waste of time not only for the skeptic but (dare I say it?) for any sane person.

Notes


11. Ibid., pp. 279-281.

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