

THE CONSTRUCTION OF THE PARANORMAL: NOTHING UNSCIENTIFIC IS HAPPENING*

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Introduction

RECENT studies have exposed the social components involved in the assessment of the validity of scientific findings, a process normally thought of as being in some way immune to social forces. This programme has developed from the work of Wittgenstein¹ through the 'Copernican Revolution' in the history and philosophy of science, associated with the writing of Kuhn, Lakatos, Feyerabend, and Lakatos and Musgrave,² to an explicitly relativistic sociology of science, as argued, for instance, by Barnes, and Collins and Cox.³ Detailed case-studies of the social processes involved in knowledge production have been carried out by the present authors. Collins⁴ has attempted to show the social component involved in the process of 'testing' findings by the replication of experiments in the case of the physical phenomena of gravity waves, and extended this analysis to the case of replication in parapsychology.⁵ Pinch,⁶ by considering the part played by a particular proof, has attempted to show the social components associated with the rejection of the work of the physicist, David Bohm, on the foundations of quantum theory.

In this paper we will be concerned with examining the processes involved in the attempt to establish the existence of a certain class of phenomena referred to as paranormal phenomena. In particular we intend to analyse the tactics used by parapsychologists in their efforts to gain scientific recognition for their discipline and its findings, and the tactics used by orthodox scientists to deny them this stamp of legitimacy. In analysing these tactics it is hoped that something will

* Our subtitle refers to the paper 'Nothing Unusual is Happening' in which Emerson discusses the interests and techniques of actors trying to establish that nothing, or something, unusual is happening in situations such as hold-ups and gynaecological examinations. The parapsychologists' tactic of metamorphosis, and their critics' attempts to prevent its success, can be seen in these terms.^{1a}

We are grateful to David Travis for his helpful discussion of the paper, and to the Social Science Research Council for supporting our fieldwork.

be learned about the social processes involved in introducing radically new or strange elements into an established culture, and indirectly about the social processes which maintain science as a part of an established culture.

Paranormal phenomena are defined by Beloff (a leading parapsychologist) as:

‘ . . . phenomena which in one or more respects conflict with accepted scientific opinion as to what is physically possible . . . ’⁷

The element of conflict within the parapsychologist's programme is rewarding in the context of a sociological study, for controversy highlights social processes with particular clarity. But it must be borne in mind that parapsychological and scientific man is not a ‘cultural dope’. Unfortunately, for the possibility of ever doing really neat quantitative fieldwork in this area, there are an embarrassing number of orthodox scientists actively experimenting with psi (see note 7); an embarrassing number who, if not actually engaged in such research, are at least prepared to entertain it; and an embarrassing number of critics of psi experimentation among the ranks of the experimenters themselves. What is more, a recent survey of *New Scientist* readers has shown that attitudes are becoming more open, for 70 per cent of the respondents claimed to be believers in the phenomena or the possibility of the phenomena.⁸ Furthermore, in our own discussions with physicists investigating Uri Geller-type metal bending, little open hostility from the physics community has been mentioned.⁹

But survey results do not make an area scientific, and even a cursory look at parapsychology produces concrete evidence of the conflict with orthodox science. For instance, articles have been rejected by orthodox journals, often with no reasons given, and sometimes referees' reports recommending publication have been overruled by journal editors.¹⁰ In fact there is evidence that the refereeing system has broken down for parapsychology papers. The editor of a well known science journal told us that there is little point in sending out parapsychology papers for refereeing because opinions are so polarised as to render the content of referees' reports entirely predictable. (This particular editor overcame the problem by evaluating nearly all parapsychology papers himself.) Again, our respondents often reported adverse effects on their careers due to their involvement with parapsychology, and any university academic is likely to suffer amused patronisation from his colleagues

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should they discover his involvement with psi—it is best kept secret as long as possible in most departments. The authors have both done experiments in areas of parapsychology and we have quickly discovered the importance of telling our sociologist colleagues that our interest was strictly that of the participant observer building up the background for good sociological fieldwork. Another survey compared the reported results of experiments published in psychology journals with those published in parapsychology journals.¹¹ The findings were that psychology journals preferred to publish negative results, when they published at all. Unfortunately this study lacked conviction, for only six parapsychological articles could be found in the psychological literature, though this is indicative in itself. Finally, parapsychology (at least in the U.K.) does not attract any government funding. Not surprisingly some parapsychologists were bemused by our sociological project, which is, ironically, probably the only government-funded research connected with parapsychology in the U.K.

1. Modes and Forums of Conflict Within Science

Sociologists have learned about conflict within science through one or two celebrated case studies, but this may have obscured the variability of the phenomenon. To put our discussion of parapsychology into perspective, it may be useful to point to the distinction between explicit and implicit conflict, and, to help with the analysis, a distinction between 'constitutive' and 'contingent' forums of debate will be drawn.

Implicit rejection operates when rival knowledge claims are ignored by orthodoxy, whilst explicit rejection is characterised by controversy where the objects of dispute are articulated by individual scientists or opposed groups of scientists. Implicit rejection has received very little discussion in the sociological literature, which is somewhat surprising, considering that it appears to occur rather often in modern science. For example, many new and challenging physical theories, even when they are published, disappear into the literature ignored rather than contested. In accord with this, there appear to have been comparatively few cases of explicit rejection with its associated controversy, in modern physics.

Both implicit and explicit rejection operate within two forums. On the one hand there is what we will call the 'constitutive' forum, which comprises scientific theorising and experiment and correspond-

ing publication and criticism in the learned journals and, perhaps, in the formal conference setting. On the other hand, there is the forum in which are set those actions which—according to old-fashioned philosophic orthodoxy—are not supposed to affect the constitution of ‘objective’ knowledge. We will call this the ‘contingent’ forum, and would expect to find there the content of popular and semi-popular journals, discussion and gossip, fund raising and publicity seeking, the setting up and joining of professional organisations, the corraling of student followers, and everything that scientists do in connection with their work, but which is not found in the constitutive forum. Within this classification, early work in the sociology of science can be seen as first discussing the explicit values of the constitutive forum,¹² and then showing the influence of the contingent forum upon it.¹³ Later work can be seen as showing that even actions properly within the constitutive forum do not have any specially privileged epistemological status, so that, within certain sociological perspectives, the separation of the two forums relates to no underlying distinction in the construction of ‘scientific’ knowledge *per se*.¹⁴

Nevertheless, despite this sociological subtlety, it is possible to generalise about the type of arguments and actions which may be legitimately expressed, in the normal way, within these forums. In the constitutive forum, actions should be *seen to be* based on universalisable non-contingent premisses, whereas, in the contingent forum, actions may be of any kind, but normally they will not *look as though* they are constitutive of scientific knowledge.¹⁵

Returning now to the question of the mechanisms of rejection of knowledge claims, we can construct a fourfold classification and give some examples of knowledge claims where the mode of rejection has been largely of one type.

		Forum	
		CONSTITUTIVE	CONTINGENT
Mode of Rejection	IMPLICIT	1	4
	EXPLICIT	2	3

Into category 1 for example, would fit the rejection of many rival interpretations of the quantum theory. These are (were) certainly presented in the professional journals, but have been allowed to fade quietly from the scene without any explicit rejection.¹⁶ Examples of category 2 include the rejection of the knowledge claims of Bohm¹⁷ and

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Barkla¹⁸ and of the phenomenon of high fluxes of gravity waves.¹⁹ Such knowledge claims all produced controversy but this was seen to be confined in the main to the constitutive forum—the professional journals and the conference presentation. The Velikovsky affair seems to fit into category 3, with his knowledge claim promoting a controversy, but a controversy which did not impinge much on the professional journals and which was conducted more over a popular front.²⁰ Finally, in category 4 are all the knowledge claims which are so radical as to produce no controversy within science, but are rejected out of hand, and without explicit mention. For example, the knowledge claims of scientologists or followers of the Reichian “orgone energy” concept would fall into this box.²¹

Our discussion here is of explicit mechanisms only, and we will consider both forums in turn, pointing out especially those illuminating cases where anomalous activity is found within a forum, that is when the normal boundaries are seen to be crossed, and are allowed to be crossed. However, it is important to bear in mind that the paper as a whole is informed by ‘recent work in the sociology of science’ which accepts no epistemological distinction between the two forums. That is, for the authors, contingent actions do constitute scientific knowledge, and the constitutive actions are as much a social construct as anything else. Only in this light can one of the parapsychologists’ main strategies for gaining scientific legitimacy be understood, this strategy being one of physical metamorphosis—of changing themselves into scientists. In getting chairs, students, university posts, funding from legitimate sources and publication in recognised journals, and in looking like ordinary quiet members of the scientific community, parapsychologists may construct scientifically acceptable telepathy, clairvoyance, and psychokinesis.²² If this were not the case, then the appropriate strategy would surely, be simply to perform and report the ‘definitive’ experiment!

2. Tactics of Legitimation and Rejection in the Constitutive Forum

In nearly every classificatory scheme such as this, certain items will be found which do not fit unambiguously into either category. Our policy is as follows. Correspondence to constitutive journals such as *Nature* or *Science* has been treated as belonging to the constitutive forum, as have books which have been reviewed in constitutive journals. Arguments which have been used in both forums are fully

discussed when they appear under the constitutive heading so that they do not need to be redescribed under the other heading.²³ A list of sources of constitutive-and contingent-forum material is given in the appendix.

Before looking at the detailed tactics of the parapsychologists and their critics we must explain that our notion of tactics is a way of 'making sense' of individual actions, though not all individual actors (not cultural dopes) need be consciously committed to what we see as an overall strategy. Other parapsychologists (not discussed here) are consciously committed to a quite different strategy. They prefer to work within the institutions of parapsychology external to the main body of science and are happy, for example, to publish only in parapsychology journals. The basis of another route to legitimacy can be seen in this attitude. The internal institutions of parapsychology themselves provide a cognitive and social framework within which credibility can be sought. Such a path will not directly concern us here.²⁴

The tactics of the parapsychologists

(i) Using Symbolic and Technical Hardware of Science

Parapsychology presented its first important knowledge claim to science in the 1930s with the work of J. B. Rhine. His experiments had many of the characteristics of legitimate scientific experiments; they involved meticulous observations done over a period of years within a university laboratory, and presented in a similar way to the reports of orthodox psychology experiments (Rhine was trained as a botanist.)

A typical experiment within the field might consist of a subject making guesses at the order of a randomly ordered pack of cards while the subject was isolated from any possible sensory channel of communication with the cards, or anyone knowing the order of the cards. The subject's guesses would be compared with the actual order of the cards, at the termination of the experiment, and usually these guesses would deviate only slightly from chance expectation. But if the subject's guesses were consistently better than chance over a long period, the total effect might be highly statistically significant, and in these cases, the subject's success could be attributed to some sort of E.S.P.

Rhine's early experiments of this type were open to a number of

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criticisms such as the possibility of unnoticed sensory cueing, or design faults at the statistical-analysis stage. This early debate will not concern us here and several reviewers agree that by the late 1930s parapsychology experiments were no longer open to obvious criticisms of procedure or statistical analysis.²⁵ The statistical debate was ended for most parapsychologists (although not for some orthodox scientists—see the arguments of Brown and Bridgman below) by a statement from the Institute of Mathematical Statistics in 1937 endorsing the parapsychologists' procedures. Thus by the 1940s the parapsychologists had obtained one of the hall-marks of scientific legitimacy, endorsement of their statistical methods by one of the most authoritative groups of scientists, the mathematicians.²⁶

Subsequently, and to an increasing extent, the parapsychologists (and one may see this as their constitutive strategy) have incorporated into their work the complex experimental techniques available to physicists, biologists or psychologists. For instance, in the modern parapsychology experiment, E.S.P. data are automatically recorded and processed, often by means of a direct computer link-up, thus evading the possibility of unconscious bias at one stage of the experiment at least. The generation of random targets may now be based on, for instance, quantum transitions processed by complex electronic equipment. This kind of target generator can be tested for randomness over millions of trials, and, being capable of producing fresh numbers at almost unlimited speed, can be incorporated with ease for instance, into experiments which may try to measure complex parameters such as speed of psi action, or can be used to test for psi 'learning'.²⁷ At the same time, there have been a growing number of attempts to discover personality variables related to success at psi tasks, and to develop ideas such as a 'decline effect', responsible for the lack of consistency in subjects' performances over extended periods.²⁸ Other conceptual tools such as information theory—a powerful heuristic in terms of normal communication systems—are increasingly being incorporated into the analysis of extrasensory communication systems.²⁹

All this has been accompanied by an increasing sophistication with statistical analysis and experimental techniques such as 'double blind' judging of results, and the use of independent observers. It seems likely that the best of modern parapsychology comprises some of the most rigorously controlled and methodologically sophisticated

work in the sciences.

Since Rhine's work there have been many accounts of good experiments to be found in the literature, and these may appear completely convincing. Indeed many of the most 'hard headed' parapsychological researchers have come into the field as a result of reading this literature though they themselves may never have seen or experienced any type of paranormal effect whatsoever. (The reader is recommended to read Schmidt³⁰ for what the authors and many parapsychologists consider to be a paradigmatically convincing account). It might be thought that a number of such carefully conducted experiments, competently reported and presented in the constitutive forum, would be sufficient to establish the existence of psi phenomena. (At least one physicist turned parapsychologist to whom we have spoken supposed that this would be the case once he had published his definitive article. Subsequent events proved him wrong.) The situation is, however, far more complicated than this.

The tactics of the critics

(ii) *Blank Refusal to Believe*

Surprisingly it is not uncommon in this field to find prejudice (in a literal sense) given as the main reason for rejecting parapsychology even in the 'objective' literature of the constitutive forum. For example, the psychologist D. O. Hebb has remarked:

'Why do we not accept E.S.P. as a psychological fact? Rhine has offered us enough evidence to have convinced us on almost any other issue . . . I cannot see what other basis my colleagues have for rejecting it . . . My own rejection of [Rhine's] views is in a literal sense prejudice.'³¹

That it should be permissible to make such statements in this forum is a startling breakdown of the normal boundaries.

Such criticisms are not limited to the constitutive forum. For example, in an address to the American Association for the Advancement of Science (A.A.A.S.), delivered in 1958, the Vice President recalled the earlier remarks of the physicist Helmholtz, who said:

'I cannot believe it. Neither the testimony of all the Fellows of the Royal Society, nor even the evidence of my own senses would lead me to believe in the transmission of thought from one person to another independently of the recognised channels of sensation. It is clearly impossible.'³²

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(iii) *Using the Symbolic Hardware of Philosophy*

Some critics have hidden their beliefs behind semi-philosophical rhetoric, thus superficially maintaining the boundary between what may, and may not be said in the literature. For instance the psychologist T. R. Willis comments:

'The conclusions of modern science are reached by strict logical proof, based on the cumulative results of numerous ad hoc observations and experiments reported in reputable scientific journals and confirmed by other scientific investigators: then and only then, can they be regarded as certain and decisively demonstrated. Once they have been finally established, any conjecture that conflicts with them, as all forms of so-called "extra-sensory perception" plainly must, can be confidently dismissed without more ado.'³⁵

This type of criticism although referring to some variant of the 'scientific method', draws its weight from the 'ethnocentricism of now'³⁴—nothing is true which conflicts with what is now known. The criticism can be dressed up as the technical 'a priori argument', 'Hume's argument concerning miracles', or 'Occam's Razor'. An example of the a priori argument comes from Hansel, who writes:

'... the a priori argument ... may even save time and effort in scrutinizing the [E.S.P.] experiments ... In view of the a priori argument against it we know in advance that telepathy, etc., cannot occur.'³⁵

Hume's argument is expressed by Price³⁶ when he writes:

'Now it happens that I myself believed in E.S.P. about fifteen years ago, after reading *Extra-Sensory Perception After Sixty Years*, [a book by Rhine] 'but I changed my mind when I became acquainted with the arguments presented by David Hume in his chapter "Of Miracles" in *An Enquiry concerning Human Understanding*.

'Hume's argument runs as follows: "A miracle is a violation of the laws of nature; and as a firm and unalterable experience has established these laws, the proof against a miracle, from the very nature of the fact, is as entire as any argument from experience can possibly be imagined ..."

Such arguments too have appeared in the contingent arena. Occam's Razor, for example, is used by Hanlon in his article in the semi-popular journal, *New Scientist*:

'By Occam's Razor it is only necessary to show that plausible normal explanations have not been excluded in order to prefer such explanations ... And their experiments fail the Occam's Razor test ...' (The experiments referred to are those of the Stanford Research Institute team who investigated Uri Geller and others).³⁷

(iv) *Association with Unscientific Beliefs*

Many of the criticisms of parapsychology even in the constitutive forum, rest upon its external basis in spiritualism and its occult associations. For instance, the psychologist Boring has written:

'It is quite clear that interest in parapsychology has been maintained by faith. People want to believe in an occult something.'³⁸

The commentator Rawcliffe claims:

'To view the modern E.S.P. movement in perspective, one must realise that it is basically a cult—a cult of the supernatural in technical dress. The perpetuation of all such cults depends ultimately on irrational beliefs and the ignoring or "explaining away" of rational criticism.'³⁹

Szasz also makes the point that the identification of E.S.P. with the occult:

'... is responsible for the obscurantism which pervades this area of inquiry and which makes its companionship unwarranted in the larger field of scientific disciplines.'⁴⁰

That parapsychology's popular associations may be cited in criticism in the constitutive forum displays the flexibility of boundaries that we have referred to.

In the contingent forum this type of criticism has appeared although here the pretence of 'objectivity' has often been dropped. An extreme form has been presented by the psychoanalyst, H. Hitschmann. He described interest in psi as:

'... the narcissistic tendency to ascribe to oneself "supernormal" faculties [corresponding] to the infantile craving to possess omnipotence of thought, a faculty of acting at a distance, and the like, as found ... in the child, savage and the insane.'⁴¹

Other critics have associated parapsychology directly with spiritualism and have attempted to imply that the subject matter of parapsychology has been 'overtaken' by science. A case for the historical illegitimacy of psi has been made by the well-known sceptic, Chris Evans.⁴² In an article in *New Scientist*, aimed at finally laying parapsychology to rest, Evans wrote:

'Most striking, of course, is the way in which, to all but the totally committed, psychical research and its subject matter seems so hopelessly, woefully out of date.'⁴³

Other critics acknowledge that parapsychology does bear some of the hallmarks of a modern science, but do not consider that this is enough. As Price puts it:

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'In short, parapsychology, although well camouflaged with some of the paraphernalia of science, still bears in abundance the markings of magic.'⁴⁴

The dangers of parapsychology's association with the occult have not gone unnoticed by parapsychologists. For example, Beloff writes:

'Parapsychology has, all through its history suffered from its fatal attraction for persons of unbalanced mind who seek in it their personal salvation . . .'⁴⁵

(v) *Accusations of Triviality*

In contrast to the arguments of those who feel that psi phenomena must be rejected because of their occult associations, are the arguments of those critics who feel that psi phenomena are simply uninteresting. For example, the psychologist Boring⁴⁶ has stated that E.S.P. data represent 'an empty correlation' and Stevens⁴⁷ claims that 'the signal-to-noise ratio for E.S.P. is simply too low to be interesting'. In an editorial of *Science*, in the context of a discussion of E.S.P., it is claimed that 'unexplained cases are simply unexplained. They can never constitute evidence for any hypothesis'.⁴⁸ Similarly the distinguished physicist turned philosopher of science, P. W. Bridgman, has written in an article in *Science* that:

✓ 'I am unwilling to accept the genuineness of any phenomenon that leans as heavily as does E.S.P. on probability arguments.'⁴⁹

A. J. Ayer has written in the contingent forum:

'The only thing that is remarkable about the subject who is credited with extra-sensory perception is that he is consistently rather better at guessing cards than the ordinary run of people have shown themselves to be. The fact that he also does "better than chance" proves nothing in itself.'⁵⁰

(vi) *Attacks on Methodological Precepts*

A more startling statistical criticism made against parapsychologists came in the 1950s from the mathematician, Spencer-Brown. Brown⁵¹ accepted the results of the parapsychologists, not as evidence of psi, but as 'the most prominent empirical reason for beginning to doubt the universal applicability of classical frequency probability'. Brown's position is clearly stated in an article in *Nature*; he wrote:

'Though not doubting the validity of some of the experimental work by the accepted standards, I was led to question the validity of one of the accepted standards.'⁵²

In other words, if experiments plus statistics equal psi phenomena, Brown was prepared to abandon statistical inference to avoid the necessity of accepting psi. Brown tried to show that statistically significant correlations could be generated between different parts of the tables of random numbers that parapsychologists were using for their controls, so that their results might have no more significance than that of one random series compared to another. The statistical criticisms of Bridgman, and Brown's criticisms in particular show the length to which sceptics are prepared to go, for, were their arguments to be accepted, normal scientific statistical procedures would have to be abandoned over a wide area.

The parapsychologists responded to Brown's arguments by conducting their own experiments with random number tables. For example one investigation, carried out by A. T. Oram⁵³ claimed to give 'a simple factual reminder that our statistical methods, when tried out in the absence of any possible influence from psi phenomena, do give reliable "chance" results'.

Interestingly, Brown's arguments, as well as being directed towards parapsychologists, can be interpreted as putting forward a hypothesis which is as much a challenge to science as is parapsychology itself. The concept of synchronicity, explicitly put forward by Jung, and implicitly by Brown,⁵⁴ is currently in vogue among some parapsychologists, perhaps due to the recent emphasis on such ideas by Koestler.⁵⁵ In the light of this current sympathy the initial hasty scepticism towards Brown may seem in need of explanation. One may speculate that, at the time, Brown's work seemed to threaten, not just to withdraw the hallmark of statistical competence from the parapsychologists, but to abolish it altogether.

(vii) *Unfavourable Comparisons with Canonical Versions of Scientific Method*

Another prerequisite of scientific legitimacy which critics claim that parapsychology has failed to satisfy, is the need for a theoretical explanation of the phenomena. This criticism seems to have been levelled in particular in the late 1950s and early 1960s. It was made, for example, by Thomas Szasz when he wrote:

'... in the realm of psychological research, in spite of widespread interest and intensive effort over more than half a century, there is still nothing that would deserve to be called a theory, even by the most enthusiastic

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proponents of this work. This, in the writer's opinion, constitutes the most decisive factor which casts doubt upon the "reality" of the entire structure of parapsychology.⁵⁶

Tart, Schmeidler and Beloff,⁵⁷ (all parapsychologists) have claimed this to be one of the most important reasons for the rejection of psi by orthodox scientists. Of course, the objection begs the question of what is to comprise a 'theory' in science. Certainly the criticism does not seem to apply to contemporary parapsychology. Most of our present respondents are working with what they regard as 'theories' of parapsychology. Indeed one of the central foci of interest in parapsychology at the moment is the debate between those adhering to the various electromagnetic and quantum-mechanical explanations of psi. In any case, parapsychologists have pointed out that, even if researchers are not committed to a particular theory, 'black boxism' is a legitimate research technique in orthodox science.⁵⁸

Another argument levelled by the critics is that parapsychology has not produced anything like 'a repeatable experiment'. We will not deal in detail with the complexities inherent in this objection for one of us has already considered it at length,⁵⁹ but further examples of repeatability cited as the touchstone of scientific legitimacy can be found in the writings of Crumbaugh and Cohen.⁶⁰ Crumbaugh, beginning a considered discussion of the problem, writes that repeatability is:

'... crucial and must be met before the great bulk of scientists will swing over to accept the E.S.P. hypothesis.'

Such criticisms have also filtered through to the contingent forum, but in a less subtle form. For instance, Cohen, writing in *The Nation* claims:

'Obviously successful E.S.P. experiments are not repeatable and thus do not meet a basic requirement of all scientific experiments.'⁶¹

What needs to be carefully argued in the constitutive forum is here 'obvious' in the contingent forum. The authoritative voice of the constitutive forum is borrowed without the caution engendered through bearing ultimate responsibility for the point at issue.

Some of the above criticisms, which comprise the constitutive front of the attack on parapsychology, seem visibly influenced more by the desire to reject psi in particular, than by considerations of universal standards. Thus many of the criticisms would have a devastating effect if turned against parts of orthodox science. Also different critics

regard different criteria as crucial, and some criteria are seemingly inconsistent. For example, on the face of it, parapsychology cannot be ruled out both because its results conflict with science, and because they are uninteresting and statistically insignificant.

The fact is that although a group of scientists have dedicated a considerable portion of their lives to the attempt to discredit psi (an interesting phenomenon in itself), they have not succeeded in revealing any universally acceptable criteria to distinguish parapsychology from science. As a result, critics have been forced into bringing into the constitutive forum, arguments usually considered more appropriate in the contingent forum. One or two cases of this process have been discussed above, but the most remarkable critique of parapsychology to enter the 'scientific' literature is the accusation of widespread fraud.

(viii) *The Fraud Hypothesis*

Accusations of fraud against individuals have occurred in other sciences, but this particular criticism is levelled against a whole discipline. Price in his notorious article in *Science* gave a rationale for this:

'... we must recognize that we usually make a certain gross statistical error. 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 likely would not have heard of Professor X at all except for his psychic findings. Accordingly, the probability of interest to us is, the probability of there having been anywhere in the world, among its more than 2 billion inhabitants, a few people with the desire and the ability artfully to produce false evidence for the supernatural.'⁶²

Price then proceeded to show ways in which fraud could have been used in a particular famous experiment. (Later Price⁶³ retracted his accusation of fraud against Soal and Rhine in a celebrated letter to *Science*.)

This tradition of giving credibility and persuasiveness to the fraud hypothesis by the demonstration of its *possibility* has become a standard procedure in the critique of parapsychological experiments. Professor C. E. M. Hansel⁶⁴ made extensive use of this technique in his book *E.S.P. A Scientific Evaluation*. He actually repeated several of the well known experiments obtaining similar results through 'normal' mechanisms, the use of one sort of technical trick or another. More recently, Joseph Hanlon has attempted to discredit the

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Stanford experiments on Uri Geller by the invention of a scenario where normal sensory communication was managed through hidden radio transmitters and a receiver embedded in Geller's tooth, in such a way as to deceive the experimenters into thinking they were witnessing E.S.P.⁶⁵

Hansel and Hanlon both make it clear that they are not suggesting that the fraud did actually occur by the mechanisms they suggest. Both use the fraud hypothesis as a soothing addendum to some version of the *a priori* argument. Though E.S.P. is seen as *a priori* impossible, the phenomena claimed by the parapsychologists must still be explained away. The fraud hypothesis fills this lacuna.

There can be no defence against this argument if it is pushed to extremes. In formal terms, an E.S.P. experiment requires that the experimenters demonstrate that no sensory channels of communication exist between the subject and the object of perception. Thus to design a criticism-free experiment the experimenters have to think of *every possible* normal means of communication between subject and object before the experiment takes place, so that they may arrange the protocol in such a way as to block every such channel. But it is always possible to find a normal channel of communication by the very nature of the experimental set-up. There always must be some normal channel of communication (however indirect) between the subject and the object of an E.S.P. experiment for it to constitute an experiment. Furthermore available normal channels of communication are potentially infinite in number because they are as *open to innovation* as science itself. Thus the credibility of an experiment depends on the outcome of a kind of race between experimenters and critics. How long can the experiment live before the determined critic can invent a normal means of communication which might have been used? The critic can always win the race if he is sufficiently determined and ingenious, because he is given an unlimited time to reach the finish.

However, even if the critic has any difficulty in inventing a plausible normal means by which the communication could have taken place, he has available the gambit of 'extending the conspiracy'. For instance, if he cannot think of a way in which the subject on his own could cheat the experimenters he can 'extend the conspiracy' to outside helpers. Hanlon,⁶⁶ referring to Geller, sets up this scenario between him and his manager, Andrija Puharich:

'If Uri came to Puharich and said 'Andrija, I have known you for a year now and never once have I cheated you. Now they are asking me to do things I may not always be able to do, but if I fail no one will believe in Hoova. You are a great inventor—give me something to help me just once in a while'.'

Hanlon legitimises this accusation of fraud by his appeal to Puharich's belief in Hoova (extra-terrestrial powers). He claims:

'... if Uri's request come via Spectra [the spokesman for Hoova] Puharich would be sure to obey. Thus, Puharich need not be party to a widespread and continuing fraud to have helped Uri in this way.'

If necessary, the critic can next extend the conspiracy further to include the experimenters as well (as Price does), and any observers too, at which point the actual conduct of the experiment ceases to have any relevance at all. The logic of the fraud hypothesis not only appears to remove any need for empirical tests from the scientific decision-making process but can also be put forward without any empirical evidence that fraud actually took place.

The paranoia thus engendered in the parapsychologists is well illustrated by the bizarre research technique adopted by one of our respondents. In an attempt to pre-empt the fraud hypothesis this researcher is having his experimental apparatus for demonstrating E.S.P. (a Schmidt machine) built and designed in modular units, by different scientists (unknown to each other) so that *no individual knows how the whole machine works*. This, he hopes, will prevent the charge of experimental collusion via the experimenters 'doctoring' the apparatus to favour positive results. It will be interesting to see which of the many methods of fraud still available will be cited as responsible should positive results be produced in this case.⁶⁷

Critics using the fraud hypothesis also make great play of parapsychology's historic associations. To be fair, it must be made clear that *there is* a history of unambiguous fraud in the area of psychical research. The subject started out from a basis of cheesecloth apparitions and invisible wires which were the props of fraudulent mediums, but this history may now be used unscrupulously. For instance Price's justification for accusing Soal of fraud appeals to this history of fraudulent mediums. Price writes:

'I would want seven or eight confederates in order to imitate 170 Soal sittings. And the reader who finds that he cannot conceive of the possibility that any leading modern parapsychologist could be fraudulent should compare his attitude with certain earlier judgements concerning the honesty of mediums.'⁶⁸

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Although one would not expect criticisms based on the historical associations of a discipline to appear in the constitutive forum (one does not find published criticisms of surgery, because it arose out of barbering, or of chemistry, because it arose out of alchemy), here again is an example where they have percolated through in the case of parapsychology.

The point of the above discussion of the fraud hypothesis is not to ridicule the critics of parapsychology—we must apologise for any unintended sarcasm—but to show its universal applicability and therefore to beg the question of its infrequency of use in the rest of science.

So far the arguments we have looked at have been taken from what we have called the constitutive forum. Most of these arguments, as we have shown above, have also been echoed in the contingent forum. We hope that the picture which is emerging will show further that formal presentations in themselves need never lead to the ultimate acceptance of results by determined critics. We turn now to examine in detail the activities which are found only in the contingent arena.

3. Tactics of Legitimation and Rejection in the Contingent Forum *The tactics of the parapsychologists*

(ix) Metamorphosis

As already mentioned, the strategy of the parapsychologists has been that of metamorphosis,—of 'becoming' scientists. Thus they have acquired university posts (parapsychology courses operate at many American universities), Ph.D. studentships (there are Ph.D. studentships in this area in at least three British universities now), chairs (Surrey University has recently established a new chair with psychical research connections) and government funding for research (still chiefly in the U.S.) and, to a limited extent, they publish in established orthodox journals. On a global scale parapsychology has many of the characteristics of orthodox scientific disciplines.⁶⁹

Recognition of this is evidenced in, for instance, the statement of the executive secretary of the National Institute of Mental Health (an American government agency) on the recent awarding of a grant to a parapsychologist. The secretary, commenting on the parapsychologists, said:

'They are perfectly well trained, respectable scientific investigators. They are sincere and serious and they deserve a chance.'⁷⁰

A statement reflecting similar sentiments about individual work is to be found in the editorial comment explaining *Nature's*⁷¹ publication of a recent parapsychological article (to be discussed below in more detail).

'Despite its shortcomings, the paper is presented as a scientific document by two qualified scientists, writing from a major research establishment apparently with the unqualified backing of the research institute itself.'⁷²

Further recognition was given in the affiliation of the Parapsychological Association to the American Association for the Advancement of Science, an episode described by Douglas Dean⁷³ in an emotive document subtitled 'Parapsychology is now a Recognised Science. How it was done'.⁷⁴ The first stage in gaining affiliation was to form the Parapsychological Association (P.A.) in 1957; as Dean put it:

'... this was the first step in creating professionalism in the parapsychology area.'

Since then, after waiting the necessary five years the P.A. has applied for affiliation in 1963, 1967 and 1968, being turned down on each occasion. Finally, in 1969, its application was accepted following a rousing speech by the social anthropologist Margaret Mead. The chairman's comments at the crucial meeting indicate the type of criteria considered important in assessing such applications. He said:

'The committee on Council Affairs considered the P.A.'s work for a very long time. The Committee came to the conclusion that it is an association investigating controversial or non-existent phenomena; however it is open in membership to critics and agnostics: and they were satisfied that it uses scientific methods of enquiry: thus that investigation can be counted as scientific. Further information has come to us that the number of A.A.A.S. fellows who are also members of the P.A. is not four as on the agenda but nine.'

The value of the affiliation is captured by Dean when he comments:

'Now, however, many parapsychologists' positions in Universities would be strengthened. The P.A. membership would rise as many good scientists would be able to join now without forfeiting their jobs or their promotion. Money may be easier too . . . We can rejoice for a time, for many of us have yearned for decades to be regarded as respectable scientists . . .'

(x) *Laundering The Funds*

The question of money draws attention to one ironical facet of this process of transformation into science. 'Becoming' scientists is an

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expensive business, but parapsychology as a whole has no immediate shortage of funds. The problem is that the source of funding is itself often an embarrassment, coming as it does from wealthy widows and the like, and reminding parapsychologists and others of the science's origins in the murky world of spiritualism.

Until 'scientific' parapsychology can achieve a breakthrough into large-scale government funding it will have to rely on contributors' interest in the 'other side' for a considerable proportion of its resources, and will be unable to escape the 'spiritualist'/'scientist' schism which characterises many of its professional organisations. These occult associations, as we saw above, can be made much of by the critics.⁷⁵

Thus the external basis of parapsychology, as well as providing funding for the parapsychologists' tactics in the contingent forum, is also a weakness to be exploited by the critics. In other ways too the critics have directed their strategy against the parapsychologists' attempts at metamorphosis.

*The tactics of the critics*⁷⁶

(xi) *Ad Hominem Arguments*

A good example of the critics' approach in action can be found in the recent critiques of the Stanford work on Geller and others. For instance Joseph Hanlon and Martin Gardner⁷⁷ when discussing these experiments, consider it legitimate to mention factors concerning the personal history and interests of the researchers. Hanlon writes:

'Targ has worked in the parapsychology area on and off for fifteen years. Puthoff has gone through encounter groups and other West Coast fads, and is now a scientologist (as is Ingo Swann [one of the psychics used in the experiments]).'

The implied criticisms here are first that both experimenters are firm believers in the phenomena and secondly, presumably, that Puthoff is gullible enough to believe in 'fads' such as scientology. Gardner also describes one of the experimenters as a 'physicist and scientologist' and makes the point that 'those who record E.S.P. data are almost always firm believers in E.S.P.' Gardner and Hanlon both make great play of the fact that Targ and Puthoff were laser physicists and were thus not competent to do E.S.P. experiments, which Gardner, for instance, regards to be the province of psychologists.

He comments on the experimental report:

'That, of course, is the voice of a physicist familiar with observer effects in quantum mechanics. It is not the voice of a psychologist.'

He also criticises the experimental design, he claims:

'It is the design of a physicist trained to investigate physical laws—laws that do not exhibit psychological quirks.'

These criticisms begin to be reminiscent of 'Catch 22'. The experimenters' background in psi makes them suspect as observers, while their background in physics renders their psi-experimental expertise suspect! The personal history of a scientists' involvement in the field is rarely considered a relevant factor in orthodox science, at least not explicitly, but, when orthodoxy is challenged, this type of factor is apparently considered important.⁷⁸

Other personal details of the experimenters are considered to be relevant by Hanlon and he even goes so far as to assess the eyesight of one of them. Similar tactics are used in a recent review of parapsychology by Phillip Morrison⁷⁹ who considers it relevant that, in a parapsychology experiment done over a century ago, one of the scientists was 'advanced in age' and another was 'half-blind'. Morrison also introduces another informal factor into the evaluation of experimenters, this is experimental pedigree. He creates the image of the 'clumsy fist-ditherer' who, of course, is incapable of doing competent experimental work.

The reviews by Hanlon, Gardner and Morrison all appeared in semi-popular scientific journals (Hanlon's in *New Scientist* and Gardner's and Morrison's in *Scientific American*). However this does not mean that they are unimportant scientific documents in the E.S.P. controversy. Because of the scarcity of papers relating to parapsychology appearing in the 'hard' scientific literature, many scientists form their opinions to some extent through reading the semi-popular journals.⁸⁰ This again emphasises the permeability of the boundary between the constitutive and contingent forums in this case. One of the above authors has told us that he regarded his particular article as 'an important scientific contribution'. And again, the special role which the semi-popular journals have played is indicated in the plans (stillborn) of *New Scientist* to organise and fund a series of scientific tests with Uri Geller, most certainly a bid to constitute new knowledge.

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(xii) *Magnifying Anecdotal Evidence*

Papers written by the experimenters which were never intended for publication and were, at the most, provisional statements indicating that *prima facie* there was something to investigate have also been used to criticise their authors. Hanlon uses this tactic on two occasions in his review of the Geller experiments. Firstly, he considers it legitimate to make detailed criticisms of the experiments done at Birkbeck College based on an unpublished paper which one of the experimenters had produced for private circulation. Secondly, he attacks Puthoff's capabilities as an observer on the basis of anecdotal evidence (later incorporated in a privately circulated letter) that Puthoff believed a description of the workings of a magnetometer by a psychic (with no scientific training) to be adequate.

(xiii) *Denying Orthodox Publications*

The issue of publication in orthodox journals has played an important part in the tactics of the critics. Despite the considerable amount of experimental evidence for psi phenomena published in the parapsychology journals, there have been comparatively few reports in orthodox journals. The parapsychology journals are read mainly by parapsychologists and are taken by few university libraries, so the main way of attracting the attention of orthodox scientists is via a publication in one of the orthodox interdisciplinary journals such as *Science* or *Nature*. As already mentioned, the little survey work which has been done in this area has indicated a bias against publishing the positive results of parapsychologists in contrast to the space given in these journals to the sceptics such as Bridgman, Brown, Price, and Hansel.

Parapsychologists have long claimed that the refereeing system of orthodox journals is biased against them. For example, among others, the distinguished parapsychologist and psychologist, R. A. McConnell, has written:

'On the basis of experience in other fields I am convinced that the refereeing system frequently operates to suppress the publication of new and important material that happens to be personally distasteful to the referees to whom it is referred.'⁸¹

These comments were made in the context of an incident involving a paper submitted to *Science* by a group of parapsychologists which was rejected after two favourable and two unfavourable referees'

reports, was resubmitted, and was again rejected after a further three referees had recommended publication and one had been against it. This evidence together with the comments of the journal editor (above) who admitted that there was little point in sending parapsychology papers out for refereeing, indicate that processes of rejection operate against parapsychology before its knowledge claims are formally presented.

(xiv) *Diluting Orthodox Publication*

The subtlety of certain of the mechanisms limiting parapsychology's metamorphosis is illustrated in a case of which we have slightly more detailed knowledge—the publication in *Nature* of the Stanford Research Institute's (S.R.I.) investigation of Uri Geller and other subjects, and referred to briefly above. This publication was accompanied by comment specially explaining editorial policy. In addition, the editorial included extracts from unfavourable referees' reports, and was written in part by Chris Evans, the author of several articles sceptical of parapsychology. In this way, the triumph of publication in *Nature* was diluted, the seeming stamp of legitimacy emasculated by making it a case of special treatment—at best a kind of tokenism.

Moreover, just in case anyone should mistake the S.R.I. article for real science, the *Nature* editorial went on to draw its readers' attention to the polemically critical article published in *New Scientist* in the same week (mentioned several times in the previous section) thus:

"The *New Scientist* does a service by publishing this week the results of Dr. Joe Hanlon's own investigations into a wide range of phenomena surrounding Geller."

What the editorial does not state is that the Editors of *New Scientist* and *Nature* had collaborated in publishing their papers, a collaboration which included some discussions and exchanges of documents. We have been told that Hanlon's critique of Geller could have been published well before the S.R.I. paper appeared in *Nature* but that the *New Scientist* deliberately delayed publication. Any belief in simple publication of positive experimental results in the orthodox literature as a means of establishing the validity of paranormal phenomena is naive indeed.

It should be stressed that we are not suggesting that co-operation

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between journals constitutes a conscious conspiracy to discredit parapsychology. There can be no doubt that all parties acted in what they honestly believed to be the best interests of scientific truth. Indeed the editorial board of *Nature* have gone out of their way to extend their evaluation process in the case of Geller-type metal-bending by actually accompanying scientists in their visits to the homes of 'mini-Gellers' to see the phenomena for themselves. The point we are making is that the modes of action which are considered legitimate in relation to parapsychology are more extensive than for other 'young' sciences.

Finally, to illustrate the problem of publishing results in parapsychology we will refer to our own limited experience of publishing in this field. One of us (Collins) has been involved in some experiments, carried out in collaboration with colleagues in the physics department at the University of Bath, investigating 'mini-Gellers'. These experiments found no evidence of paranormal metal-bending and, on several occasions, 'normal' methods of bending (cheating) were observed. The results of these experiments were published in the correspondence column of *Nature*.⁸² The editor has indicated that if an experimental report of this (correspondence) type had contained positive findings it would not have been accepted.

4. The Levy Affair—The Machine at Work

Despite the tactics of the critics the tactics of the parapsychologists have paid off in the sense that they now appear to be able to invoke many of the internal social processes of legitimate disciplines in handling their affairs. In order to illustrate this we will, in the last part of this paper, focus on the reaction of the parapsychologists to a recent discovery of fraud within their discipline.

In 1974 the director of one of the best known parapsychology research centres was accused by co-workers of faking results. The scientist concerned was Dr. J. Levy, a protege of Rhine and director of the famous research centre at Durham, North Carolina, where Rhine had done most of his early work. Levy had been producing excellent results working on experiments with rodents. Indeed these experiments had caused great excitement in the parapsychological world, and were the basis of the following comment made in a recent review of biological aspects in the paranormal:

'From among the heterogeneous collection of experiments described here there is only one fact which has, in the writer's opinion, been established beyond all reasonable doubt, namely the existence of precognition in rodents. The fully automated experiments with mice and gerbils have been replicated sufficiently often and yielded such clearly significant results that we are now justified in claiming this phenomenon as one of the most firmly established in parapsychology. To the workers in the field the establishment of one fact may not seem very much to justify the large amount of effort expended. To the academic biologist, however, even this one fact must appear revolutionary, since it seems to overthrow many of the present day assumptions of his science.'⁸³

However, towards the middle of 1974 (almost simultaneously with the publication of the above comment) Levy began to attract the attention of his co-workers by acting suspiciously in the vicinity of the automatic data-recording devices while experiments were in progress. Technicians made their own secret records of data and, when these were compared with Levy's, his were found to be inaccurate, being positively biased. At first sight, bearing in mind the extravagant claims made for Levy's results, his position of eminence in the foremost research institution, and the perpetual rearguard action against the fraud hypothesis, this scandal seemed to have the potential to finish parapsychology once and for all. How did parapsychologists react?

The most striking part of the way in which parapsychologists dealt with the Levy case was the openness that they exhibited in describing the events. On discovery of the fraud, details were immediately sent to all persons known to be planning to use Levy's work in articles, books or other presentations. Likewise those involved in efforts to repeat his experiments were notified. J. B. Rhine then published a long statement in the *Journal of Parapsychology*⁸⁴ in which he announced that all Levy's experimental reports, published or unpublished, authored by him or jointly were to be considered unacceptable. All Levy's work confirmed independently by one or more other experiment should, for the present, be evaluated entirely on the strength of the replicated work itself. But Rhine also pointed out that Levy had been caught by other parapsychologists, so that there was no possible case for collusion. Also, he drew parallels with cases of fraud in orthodox science and claimed that the implication for parapsychology was no greater than the Summerlin affair was for medical research.⁸⁵

In general the parapsychologists have not over-reacted to the Levy affair. They managed to keep it out of the hands of the press until all parapsychologists had been informed, and they managed to avoid any

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charges of a 'cover up'. Those parapsychologists working in the same area as Levy, to whom we have talked, have stressed that this case of fraud is only a temporary set back and the parapsychologists should concentrate on attempting to replicate Levy's original experiments. In general the motives ascribed to Levy have been similar to those attributed to Summerlin. Levy was, like Summerlin, a 'high powered' young man, sponsored by an older established researcher. He had made a major breakthrough in the field with experimental results, but when they failed to continue, he faked his data so that the experiment should continue. Levy was also subject to extra pressure as the administrative head of a large research laboratory and pressure from his family who wanted him to work in the more orthodox field of medical research, for which he had been trained. The final parallel between Levy and Summerlin is that there have been reported independent replications of both sets of faked results!

The reaction of the parapsychologists to a potentially catastrophic occurrence seems to have been effective. Critics have been disarmed to some extent by its use as an illustration of the effectiveness of parapsychology's internal control mechanisms. Thus Puthoff and Targ⁸⁶ use the example of the Levy affair in precisely this way in a paper published in the prestigious orthodox journal *Proceedings of the I.E.E.E.*, they comment:

'It should also be noted that parapsychological researchers themselves recently exposed fraud in their own laboratory when they encountered it.'

Rather than seeing the Levy affair as the last nail in the coffin of parapsychology, at least one other parapsychologist now feels confident enough to use the affair as an illustration of parapsychology's legitimacy as a science. McConnell remarks:

'The recent uncovering of an intentional falsification of experimental data by a relatively well known parapsychologist has provided a new impetus to consider the problem of fraud. In a sense, this incident is a *cause for congratulation* inasmuch as it was this parapsychologist's close professional associates who discovered and revealed his dishonesty. From this fact the world at large may draw reassurance that in parapsychology, as in any other field of science, professional deception will not be tolerated.' (Our emphasis.)⁸⁷

The handling of the Levy affair certainly appears to us to be on a par with the handling of cases of fraud in orthodox science and is thus an indication that parapsychology has acquired many of the social

characteristics of an orthodox science. The tactics of acceptance thus seem to have provided the parapsychologists, if not with legitimacy, at least with a reservoir of credibility which acts as a buffer against some of the criticisms, such as widespread fraud, brought to bear on it by the sceptics.

Conclusion

What we have done in this paper is to look at some aspects of the explicit debate over the acceptance of 'scientific' parapsychology. We have divided the exposition into two halves by reference to the notions of 'constitutive' and 'contingent' forums. These are categories which at one time might have been thought to have epistemological significance, but recent work in the sociology of science has, we believe, dissolved this idea, showing that the constitution of scientific ideas can be seen as a product of contingent actions. Nevertheless, these categories can still be used to distinguish between theorisation, experimentation, professional publication and the other activities of scientists in connection with their work. Also, the two forums embody different expectations of types of visible action. In the constitutive forum actions normally should be seen to be based on universalisable premisses and in the contingent forum actions should normally be seen to be incidental to the constitution of knowledge.

In looking at parapsychology, we have found a number of cases of, as it were 'full frontal' breakdown of this norm. In the constitutive forum, we have found prejudice and other particularistic bases of belief openly endorsed. In the contingent forum, we have found items intended as substantive contributions to the constitution of scientific knowledge, rather than commentary and exposition. In themselves, these boundary crossings can be seen to represent a tactic of the orthodox scientists, they expose the fact that parapsychology should not be treated in the same way as a science, i.e. that 'something unscientific is happening', despite its practitioners' attempts to metamorphosise themselves—so that '*nothing* unscientific is happening'.

Throughout, we have intended this paper to be entirely neutral as to the existence of paranormal phenomena. In saying that the constitution of scientific knowledge can be looked at as a product of contingent actions, we are endorsing a relativistic thesis within which consideration of the 'actual existence' of a phenomenon is redundant. It is, we believe, only through this perspective that the full richness

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of the social processes involved in the construction of scientific knowledge can be revealed. In addition, from this perspective, a strategy of metamorphosis makes eminent sense, for when 'scientists' turn their attention to something, then 'that really is something'!

University of Bath.

Note Added December 1976. Since writing this paper further examples illustrating the processes discussed on pp. 250-252 have appeared. For instance see Harmon, and Puthoff and Targ.⁸⁸ Harmon writes:

'We are asked to believe that no conceivable communication channel existed between "transmitter" and "receiver" other than by some exotic attenuationless seemingly magical information propagation.

'I feel certain that your readers can conceive of many possible alternative conventional channels. Both electronic engineers and magicians, for example, will be at no loss to suggest many.'

Note Added June 1977. By June 1977 this paper was itself being brought into the argument. It is perceived, it seems, as a paper which might favour the parapsychologists' case. Thus, we have received a spleenful letter from a well known professional magician-and-sceptic which attempts to persuade us to change our attitude to research in the paranormal and claims that:

'Seriously, how men of science such as yourselves can make excuses for . . . [Targ's and Puthoff's] incompetence is a matter of astonishment to me . . . [and] . . . I was shocked at your paper; I had expected science rather than selective reporting.'

On the other hand, parapsychologists have been complimentary. An American academic, intending to forward the paper to possible sponsors among others, has commented in a letter to us:

'It is just so accurate and well done and coming from an "outsider", it will carry so much more weight.'

These comments are sociologically interesting in themselves but also give us some cause for concern, lest the paper be generally misconstrued outside the *sociological community*, for which it was intended. The paper is neutral regarding the existence of paranormal phenomena—its relativism makes the question of their existence redundant. We are delighted that a discussion of the tactics of scientific legitimisation should be of use to practitioners, but we must point out that it can only be of use to those who have *already* decided whether they want the phenomena to exist or not, and, can only be seen as *pro* parapsychology by those who have decided that the tactics of denying legitimacy to the subject are less honourable than those used to try to gain it. For ourselves, as professional sociologists, we are disinterested in these questions. Thus, we do not condemn the departures from normal scientific practice which we describe, nor laud the tactics of metamorphosis essayed by the parapsychologists.

¹ L. Wittgenstein: *Philosophical Investigations*, Blackwell, Oxford, 1953, and; *Remarks on the Foundations of Mathematics*, Blackwell, Oxford, 1956.

^{1a} J. P. Emerson: 'Nothing Unusual is Happening', in T. Shibutani (ed.): *Human Nature and Collective Behavior*, Prentice-Hall, Englewood Cliffs, 1970 pp. 208-222.

² P. Feyerabend: *Against Method*, New Left Books, 1974; I. Lakatos and A. Musgrave (eds.): *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge, 1970; I. Lakatos: 'Proofs and Refutations', *British Journal for the Philosophy of Science*, Vol. 14, 1963, pp. 1-25, 120-129, 221-245, 296-342; T. S. Kuhn: *Structure of Scientific Revolutions*, Chicago University Press, Chicago, 1970.

³ B. Barnes: *Scientific Knowledge and Sociological Theory*, Routledge & Kegan Paul, London, 1974; H. M. Collins and G. Cox: 'Recovering Relativity: Did Prophecy Fail?' *Social Studies of Science*, Vol. 6, 1976, pp. 423-445

⁴ H. M. Collins: 'The Seven Sexes: A Study in the Sociology of a Phenomenon', *Sociology*, Vol. 9, 1975, pp. 205-224.

⁵ H. M. Collins: 'Upon the Replication of Scientific Findings: A Discussion Illuminated by the Experiences of Researchers into Parapsychology', paper read at the S.S.S.S., I.S.A. Conference, Cornell University, 1976.

⁶ T. J. Pinch: 'What does a Proof do if it does not Prove?', in E. Mendelsohn, P. Weingart and R. D. Whitley (eds.): *The Social Production of Scientific Knowledge*, Reidel, Dordrecht, 1977.

⁷ J. Beloff: *New Directions in Parapsychology*, Elek Science, London, 1974, p. 1. Beloff's quotation continues:

'Such phenomena are variously referred to as "parapsychological", "parapsychical", "psychical" or "psychic" but . . . the convention now widely current in the technical literature [is to use] the abbreviation "psi".'

Psi phenomena fall into two main categories: psi cognition, better known as ESP (Extrasensory Perception), and psi action, better known as PK (Psychokinesis). Psi cognition or ESP can further be subdivided according to whether it is presumed to depend on another person (the 'agent') whose normal cognitive processes constitute the source of the information, in which case it is referred to as 'telepathy', or whether it is presumed to be independent of any such mediation so that the information comes directly from the target object or event, in which case it is referred to as "clairvoyance".'

⁸ C. Evans: 'Parapsychology—what the Questionnaire Revealed!' *New Scientist*, 25 January 1973, p. 209.

⁹ The interview data on which this paper is based comes from fieldwork conducted by Collins in 1971-75, and by Collins and Pinch in 1975-76.

¹⁰ C. Honorton, M. Ramsey and C. Cabibbo: 'Experimenter Effects in Extrasensory Perception', *Journal of the American Society for Psychical Research*, Vol. 69, 1975, pp. 135-149, Appendix.

¹¹ M. Billig: 'Positive and Negative Experimental Psi Results in Psychology and Parapsychology Journals', *Journal of the Society for Psychical Research* Vol. 46, 1972, pp. 136-142.

¹² R. K. Merton: *Social Theory and Social Structure*, Free Press, New York, 1967, pp. 550-561.

¹³ R. K. Merton: 'The Matthew Effect in Science: The Reward and Communication System of Science', *Science*, Vol. 199, 1968, pp. 55-63; B. Barber: 'Resistance by Scientists to Scientific Discovery' in B. Barber and W. Hirsch (eds.): *The Sociology of Science*, Free Press, New York, 1962, pp. 539-558.

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¹⁴ For example, Collins: *op. cit.*, 1975, 1976; B. Wynne: 'C. G. Barkla and the J. Phenomenon: A Case Study of the Treatment of Deviance in Physics', *Social Studies of Science*, Vol. 6, 1976, pp. 307-348.

¹⁵ We underline '*seen to be*' and '*look as though*' to stress that we are talking only of the expected *appearance of things*.

In a recent unpublished paper (M. J. Mulkay: 'Norms and Ideology in Science', mimeo, York University, 1976), Mulkay treats norms in science as 'vocabularies of justification'. This approach would fit well with the discussion pursued here, and this discussion could be interpreted as exploring the differing vocabularies of public and private communications. Mulkay identifies what we would call different constitutive values and shows how they are used in the contingent forum in order to secure political status for the scientific enterprise as a whole. For more discussion of the language of the constitutive forum, see G. N. Gilbert: 'The Transformation of Research Finding into Scientific Knowledge', *Social Studies of Science*, Vol. 6, 1976, pp. 281-306. For two other interesting sociological studies of parapsychology see P. D. Allison: 'Social Aspects of Scientific Innovation: the Case of Parapsychology', unpublished Masters Thesis, University of Wisconsin, 1973; and M. D. Gordon: 'The Institutionalisation of Parapsychology: A Study of Innovation in Science', unpublished M.Sc. Dissertation, University of Manchester, 1975.

¹⁶ Pinch: *op. cit.*, found in this study of the reception of Bohm's ideas on the foundation of quantum theory that several heterodox interpretations of quantum mechanics were published but failed to cause any ripples on the surface of physics. For an attempted analysis of why Bohm's ideas were explicitly rejected as opposed to the implicit rejection of many ideas in quantum theory, see Pinch: *op. cit.* The Editor of a well known science journal has also told us that many papers published in his journal are 'wrong' but most pass into the literature uncontested.

¹⁷ *ibid.*

¹⁸ Wynne: *op. cit.*

¹⁹ Collins: *op. cit.*, 1975.

²⁰ R. G. A. Dolby: 'What can we usefully learn from the Velikovsky Affair?', *Social Studies of Science*, Vol. 5, 1975, pp. 165-175; A. De Grazia (ed.): *The Velikovsky Affair*, University Books, New York, 1966.

²¹ Many of the cults discussed by C. Evans (*Cults of Unreason*, Panther, St. Albans, 1974) would fit into this box. The idiosyncratic numbering of the boxes is deliberate. Increasing cognitive distance from the cultural centre of modern science (wayoutness) follows the anticlockwise direction indicated.

²² We are referring here, not to the impact of the content of the material published in constitutive journals, but to the legitimating effect of getting any material into this forum.

²³ It is important to remember that our prime classification division concerns the *places* where actions (arguments) occur. It would therefore, be appropriate but clumsy to give a full inventory of arguments under each heading.

²⁴ Hence no parapsychology journals are referred to in the central discussion. The parapsychology journals have no place in our forums. Rather the parapsychology establishment has its own parallel constitutive and contingent forums, related, we would expect, as those of orthodox science.

- ²⁵ C. Honorton: 'Error Some Place', *Journal of Communication*, Vol. 25, 1975, pp. 103-116.
- ²⁶ For a discussion of the relationship between mathematisation and professionalisation, see R. D. Whitley: 'Changes in the Social and Intellectual Organisation of the Sciences: Professionalisation and the Arithmetic Ideal,' Mendelsohn *et al.*: *op. cit.*
- ²⁷ A review of the methodological advantages of this type of instrumentation is to be found in H. Schmidt: 'Instrumentation in the Parapsychological Laboratory', in Beloff (ed.): *op. cit.*, 1974, pp. 12-37.
- ²⁸ For a review of some personality correlates with E.S.P. see G. R. Schmeidler: 'Personality Differences in the Effective Use of E.S.P.', *Journal of Communication*, Vol. 25, 1975, pp. 133-141.
- ²⁹ See, for example, R. L. Morris: 'Building Experimental Models', *Journal of Communication*, Vol. 25, 1975, pp. 117-125.
- ³⁰ H. Schmidt: 'Mental Influences on Random Events', *New Scientist*, 24 June 1971, pp. 757-758.
- ³¹ D. O. Hebb: 'The Role of Neurological Ideas in Psychology', *Journal of Personality*, Vol. 20, 1951, pp. 39-55.
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- ⁴³ *ibid.*
- ⁴⁴ Price: *op. cit.*, 1955.
- ⁴⁵ Beloff: *op. cit.*, 1974.

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- ⁴⁶ E. G. Boring: 'The Present Status of Parapsychology', *American Scientist*, Vol. 43, 1955, pp. 108-116.
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- ⁶⁴ Hansel: op. cit.
- ⁶⁵ Hanlon: op. cit. Although Hanlon's article was in the semi-popular journal *New Scientist*, the permeability of the boundaries between the two forums has meant that this contribution was intended (and has probably succeeded) in constituting scientific knowledge. Hanlon's contribution is discussed in more detail below.
- ⁶⁶ *ibid.*
- ⁶⁷ It is an interesting exercise to try and 'conjure up' such objections (*Quis Custodiet Custodes?*).
- ⁶⁸ Price: op. cit., 1955.

⁶⁹ Of course, it could be argued that there is a crucial 'size' which demarcates a science from a non-science and that parapsychology's still small institutional base does not make it a science. However, it is difficult to determine the 'size' of a science by any abstract criteria. Certainly there are specialist areas in orthodox science which are on a smaller institutional footing than parapsychology. It seems that the appropriate size of a discipline will only be established by a combination of factors such as available resources, cognitive importance in relation to other areas, etc., and in general these factors can only be identified after the discipline has 'taken off'. The problem of finding criteria connected to size is illustrated by at least one physicist turned parapsychologist informing us that the area was already saturated and that there were not many new problems for researchers to take up. This contrasts with the view of most parapsychologists who consider that parapsychology has yet to 'take off'.

⁷⁰ Quoted in N. Wade: 'Psychical Research: The Incredible in Search of Credibility', *Science*, Vol. 181, 1973, pp. 138-143.

⁷¹ 'Investigating the Paranormal', *Nature*, Vol. 251, 1974, pp. 559-560.

⁷² *ibid.*, p. 560. Evidence of the type of factor which is considered important by parapsychologists in gaining academic acceptance comes from an article by R. A. McConnell. Under a sub-heading, 'Recent Landmarks of Academic Acceptance', McConnell mentions publication of a review of E.S.P. in a major psychology text-book, publication of a paper on E.S.P. in the *American Psychologist* and affiliation to the A.A.A.S. as key events in the acceptance of psi. See R. A. McConnell: 'Parapsychology and the Occult', *Journal of the American Society for Psychical Research*, Vol. 67, 1973, pp. 225-243.

⁷³ E. D. Dean: 'The Parapsychological Association Becomes Affiliated with the American Association for the Advancement of Science', unpublished informal document, 1969.

⁷⁴ At the time Dean was secretary of the Parapsychological Association.

⁷⁵ A factor considered relevant by the critics of parapsychology, but not usually mentioned in critiques of other sciences is the origins of the grants received by the parapsychologists. To give one example, M. Gardner ('Concerning an Effort to Demonstrate Extrasensory Perception by Machine', *Scientific American*, October, 1975, pp. 114-118) somewhat gleefully concludes a critical review of the parapsychological research done at the Stanford Research Institute by commenting on the termination of the researchers' (Targ and Puthoff) legitimate source of funding—the North American Space Agency.

The importance of funding to critics is understandable in the context of parapsychology's attempts to gain legitimacy. The acquisition of resources from the normal scientific channels can be seen as an important step in parapsychology's attempted metamorphosis and hence also a 'sensitive' area for critics to focus on. The critics seem to be using the funding issue to add leverage to their other criticisms. Hanlon and Gardner refer to the responsibilities attached to such legitimate funding with the implications that if the parapsychologists don't succeed they will be wasting public money. The parapsychologists' attempts to gain legitimate sources of funding can thus be seen as a 'double edged sword'.

⁷⁶ In this section we will be considering those contingent activities which have not emerged into the constitutive forum. For instance, we will not discuss further the attempts to discredit the results of the parapsychologists by

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reference to prejudice, philosophical technicalities, occult associations, lack of theory, repeatability and fraud, although all such criticisms are to be found in the contingent forum.

⁷⁷ Hanlon: op. cit.; Gardner: op. cit.

⁷⁸ Charles Tart has made this point in discussion with us.

⁷⁹ P. Morrison: 'Uri Geller: International Pied Piper of the Credulous, and Other Matters', *Scientific American*, February 1976, pp. 134-135.

⁸⁰ This suggestion is supported by the result of survey work showing how orthodox scientists got to hear about E.S.P. See Evans: op. cit., 1973; L. Warner: 'What the Younger Parapsychologists think about E.S.P.', *Journal of Parapsychology*, 19 December 1955, pp. 228-235. Ransom in an excellent review of criticisms of parapsychology also reached this conclusion. See C. Ransom: 'Recent Criticisms of Parapsychology: A Review', *Journal of American Society for Psychical Research*, Vol. 65, 1970, pp. 289-307.

⁸¹ Honorton et al.: op. cit., p. 144.

⁸² B. Pamplin and H. M. Collins: 'Spoon Bending: An Experimental Approach', *Nature*, Vol. 257, 1975, p. 8. The correspondence column of *Nature*, as opposed to the Letters section, allows for material which is unlikely to get past a referee and is generally written in a more anecdotal style.

⁸³ J. L. Randall: 'Biological Aspects of Psi', in Beloff (ed.): op. cit., 1974, p. 92.

⁸⁴ J. B. Rhine: 'A New Case of Experimenter Unreliability', *Journal of Parapsychology*, Vol. 38, 1974, pp. 215-225.

⁸⁵ B. Culliton: 'The Sloan-Kettering Affair, A Story with a Hero', *Science*, 16 May 1974, pp. 644-650 and 'The Sloan-Kettering Affair (II): An Uneasy Resolution', *Science*, 15 June 1974, pp. 1154-1157.

⁸⁶ H. E. Puthoff and R. Targ: 'A Perceptual Channel for Information Transfer over Kilometer Distances: Historical Perspective and Recent Research', *Proceedings of the I.E.E.E.*, Vol. 64, 1976, pp. 329-356; and 'Reply', *Proceedings of the I.E.E.E.*, August 1976, p. 1259.

⁸⁷ R. A. McConnell: 'The Motivations of Parapsychologists and Other Scientists', *Journal of the American Society of Psychical Research*, Vol. 69, 1975, pp. 273-280.

⁸⁸ L. H. Harmon: 'Comments on "A perceptual channel for information transfer over kilometer distances: historical perspective and recent research"', *Proceedings of the I.E.E.E.*, August 1976, p. 1259; Puthoff and Targ: op. cit.

H. M. Collins and T. J. Pinch

APPENDIX

Main Published Sources of Material Referred to in Forums

CONSTITUTIVE FORUM

International Journal of Neuropsychiatry

Journal of Personality

Nature

Psychiatric Quarterly

Science

J. Beloff (ed): *New Directions in Parapsychology*, Elek Science, London, 1974.

G. S. Brown: *Probability and Scientific Inference*, Longmans, London, 1957.

C. E. M. Hansel: *E.S.P.: A Scientific Evaluation*, Charles Scribner's Sons, New York, 1966.

D. H. Rawcliffe: *Illusions and Delusions of the Supernatural and the Occult*, Dover, New York, 1959.

J. R. Smythies: *Science and E.S.P.*, Routledge and Kegan Paul, London, 1967.

CONTINGENT FORUM

New Scientist

Scientific American