

## **Science, Nonscience and Rejected Knowledge: The Case of Parapsychology<sup>1</sup>**

HARVEY J. IRWIN

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**ABSTRACT:** Parapsychologists purport to apply scientific method to the investigation of the bases of commonly reported parapsychological experiences such as extrasensory perception. Despite over a hundred years of associated research effort the status of parapsychology as a scientific endeavour is disputed by a substantial section of the contemporary mainstream scientific community. This paper identifies some of the major chronological shifts in the rationale for dismissing parapsychology as nonscientific, examining several historical attempts by parapsychologists to establish the scientific legitimacy of their discipline and the concomitant strategies of orthodox scientists to marginalise the findings of parapsychological research as rejected knowledge.

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Some experiences in everyday life appear, at least superficially, to be paranormal—that is, the experiences are commonly taken by lay people to indicate the operation of factors currently unknown to, or unrecognised by orthodox science. Ostensibly paranormal experiences include those popularly designated as extrasensory perception (ESP), ‘mind over matter’ phenomena such as psychokinesis (PK) and psychic healing, and experiences that seem to imply the existence of the spirit or some such nonphysical element of human existence (e.g., out-of-body experiences, near-death experiences, apparitional experiences, and past-life experiences). Parapsychology is the scientific investigation of putatively paranormal experiences. More formally, parapsychology has been defined by its proponents as the study of “apparent anomalies of behavior and experience that exist apart from currently known explanatory mechanisms that account

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for organism-environment and organism-organism information and influence flow” (Parapsychological Association, 1989, pp. 394-395).

Parapsychological phenomena therefore comprise experiences having the *appearance* of paranormality, and parapsychology is the study of these experiences from a scientific perspective.

A key element of the research agenda in parapsychology is of course, to determine if any phenomena justify the postulation of paranormal processes such as ESP and PK or if, on the other hand, they can satisfactorily be accommodated in terms of processes (e.g., hallucination, misperception, misinterpretation, or misrepresentation) already familiar to mainstream science. At the same time parapsychologists do not confine themselves to so-called proof-oriented research (Irwin & Watt, 2007). Many parapsychologists also investigate the circumstances under which parapsychological phenomena occur (process-oriented research), the experiential characteristics of the phenomena as they appear to the person who has had the experience (phenomenological research), the psychological characteristics of people who report parapsychological experiences, and the psychological and social functions of belief in the paranormal. Note that these issues may be scientifically studied whether or not the associated parapsychological phenomenon has a truly paranormal basis (Irwin, 1994, 2006).

Despite this fact a significant section of the mainstream scientific community disputes parapsychology’s claim to scientific status. The existence of paranormal processes is widely perceived to fly in the face of scientific knowledge, so parapsychology has variously been depicted as an unscientific, pseudoscientific, or even antiscientific enterprise and its practitioners dismissed as mere closet occultists in pursuit of the miraculous (e.g., Alcock, 1981; Moss & Butler, 1978; Park, 2000; Romm, 1977). The scientific legitimacy of parapsychology has been shown to be rejected most strongly by the ‘scientific elite’ (McClenon, 1982), the academics who exercise the greatest influence over policy decisions in the world of institutionalised science (Broad & Wade, 1982). In addition there is now a vociferous, organised social movement of self-described sceptics who vehemently denounce the fidelity of parapsychology as an academic discipline. This movement is most clearly typified by the activities of the so-called Committee for the Scientific Investigation of Claims of the Paranormal or CSICOP<sup>1</sup> (Frazier, 1996; Hansen, 1992; Pinch & Collins, 1984). Although the most vocal members of CSICOP are journalists, magicians, and philosophers rather than scientists (Hansen, 2001) this

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<sup>1</sup> On November 30, 2006, CSICOP became formally known as the Committee for Skeptical Inquiry (CSI). (Ed.)

organization is widely seen to constitute the voice of mainstream scientific opinion.

There have been many research enterprises (e.g., astrology, UFOlogy, phrenology, creationism) that with seeming sincerity have embraced the scientific method in their pursuit of anomalous knowledge, yet most of these have been short-lived: having been marginalised by the scientific institution that defines the boundary between science and nonscience, these research enterprises generally have abandoned their scientific aspirations and either have faded into obscurity or have survived by reverting to a popularist (rather than scientific) social movement. Parapsychology is rather different in this respect. Mainstream resistance to parapsychological research has been evident for over a hundred years and has been particularly vigorous since the 1930s, yet parapsychology continues to operate as a research-oriented discipline that publicly declares its observance of the premises of scientific endeavour. Although parapsychology exists on the very fringe of contemporary science its survival even in this state is remarkable. If only for this reason the status of parapsychology as a 'rejected science' deserves analysis from the perspective of the sociology of science.

This article surveys in chronological order some of the major events in the history of parapsychology that attest both to the efforts of parapsychologists to establish their research as a legitimate scientific pursuit and to the reactive efforts of orthodox scientists to maintain the findings of parapsychological research as rejected knowledge. The focus here is on the historical conflict over notional boundaries set up to exclude the discipline of parapsychology from the mainstream of science; broader and more comprehensive accounts of the history of parapsychological research are available elsewhere (Beloff, 1993; Gauld, 1968; Grattan-Guinness, 1982; Inglis, 1977, 1984; Leahey & Leahey, 1983; Mauskopf & McVaugh, 1980; Moore, 1977; Rush, 1986). The historical episodes to be reviewed concern early protoscientific attempts to subject spiritualistic mediums to controlled testing; J. B. Rhine's work to establish ESP research in a university setting; the efforts of the Parapsychological Association to secure affiliation with the American Association for the Advancement of Science (AAAS); meta-analysis and the Ganzfeld debate; and the National Research Council's evaluative report on parapsychological research. Finally, I present a contemporary description of the status of parapsychology within the broader scientific community.

PROTOSCIENTIFIC TESTING OF SPIRITUALISTIC MEDIUMS

Although many contemporary parapsychologists eschew the controversial phenomena of the séance room parapsychological research as a coherent empirical discipline was actually precipitated by the Spiritualist movement which began in America in the middle of the nineteenth century and then spread rapidly to many other Western countries. Spiritualistic mediums appeared to be able to communicate with the spirits of deceased persons and to act as a mediator in the spirits' production (usually under conditions of darkness or subdued lighting) of extraordinary anomalous phenomena such as direct speech by the spirit entity, 'spirit' raps within the séance room, the levitation of objects and people, and the apportionment of various exotic objects into the séance room. Particularly in England a small number of academics and other people became intrigued with the metaphysical implications of séance phenomena and they advocated the need for rigorous empirical scrutiny of these phenomena. Such concerns led eventually to the establishment in 1882 of the (British) Society for Psychical Research and subsequently to the formation of similar societies in other countries.

It was in this context that a few scientists undertook the investigation of séance phenomena in the latter half of the nineteenth century. In many instances such studies were conducted with the clear intention of exposing the reported phenomena as sham and humbug. The distinguished physicist Michael Faraday (1853), for example, conducted simulated séances with a table fitted with a loose top and showed that when sitters laid their hands on the tabletop they unwittingly exerted lateral pressure on it and made it turn. On this basis Faraday concluded the table-tipping phenomena of séances had a natural explanation and thus there was no need to invoke paranormal accounts. Seemingly to reinforce this conclusion scientists elsewhere (e.g., Archer, 1864) performed constructive replications of Faraday's experiment. Even in this early phase of parapsychological research the response of the scientific establishment was sometimes intended to demonstrate that paranormal phenomena do not exist and that for this reason no further empirical research was called for.

Other scientists were more open-minded in their study of séance phenomena and conducted their research without an explicit objective to achieve an exposé. Inevitably perhaps, some spiritualist mediums nevertheless were found to be engaging in fraudulent acts. On the other hand a small number of scientists did conclude the phenomena they observed during séances appeared to be genuine, that is, there seemed to be no adequate explanation of these incidents unless recourse was made to paranormal factors (e.g., see Zorab, 1970). It must be stressed, however,

that many of these researchers were ill-qualified for the task they undertook (Lamont, 2005), having for example, no familiarity with even basic conjuring techniques. Indeed, techniques for testing mediums were poorly developed at this time: often the investigator simply observed what the medium did. Even when some elementary controls over the medium's movements were imposed they were ones which readily could be circumvented by a skilled conjurer. Thus, the investigations of spiritualist mediums in this era would best be regarded as protoscientific by modern standards.

Consideration of the work of one specific scientist during the spiritualist era is instructive for the issue of the scientific reception of parapsychology. The eminent English chemist and physicist (later Sir) William Crookes undertook a comparatively extensive investigation of the séance performances of the medium Daniel Dunglas Home (see Crookes, 1874 for an anthology of his reports). In the *Quarterly Journal of Science* for July 1870, Crookes announced an empirical project to determine whether the alleged phenomena of spiritualist séances were authentic or bogus. To the extent that other scientists were at all interested in this objective it appears they generally felt assured that the authority of Crookes would do much to dispel the 'superstitious attitudes' encouraged among members of the public by the spiritualist movement. At this time, therefore, there was no vehement opposition to parapsychological research, probably because there was an implicit assumption that the findings of any such research would serve to discount paranormal hypotheses and thereby the research program would soon bring about its own demise.

Crookes' work with Home commenced in 1871. To the alarm of the scientific community Crookes reported that the séance phenomena associated with Home were entirely genuine. Crookes described the occurrence of spirit raps governed by some form of intelligence; the movement of furniture and other objects either in contact with Home or at some distance from the medium; levitation of furniture and of Home himself; materialization of spirit hands, faces, and 'phantasmic forms' or apparitions; automatic writing through the medium's hand or with a planchette; and apports, the paranormal transference of objects into the séance room from other locations. Crookes also designed specific tests of the medium's powers. He set up weights in a chemical balance and asked the medium (or the spirits working through the agency of the medium) to influence the mass of an item to upset the equilibrium of the balance. He also enclosed one end of a small accordion in a cage of copper wire and asked that the instrument be played. According to Crookes Home passed each of these tests.

The attitude of the scientific establishment to Crookes' project showed an immediate and dramatic reversal. The findings of the report on Home were clearly seen as unacceptable and evoked vehement criticism by other scientists, much of it emotionally rather than rationally based and some involving outright misrepresentation (Hyman, 1986; Lamont, 2005). Without any supportive evidence it was charged, for example, that Home had attached to the accordion a wire or some such device by which he was able to extract a few notes from the instrument, or that he had smuggled into the room another accordion that was used to produce some music (Wyndham, 1937, p. 231). These comments were not in the form of suggestions for further, perhaps methodologically more refined, investigation of Home; they were presented as conclusive demonstrations that Crookes' conclusions were patently erroneous.

There may well be independent grounds upon which to query the competence of Crookes as a parapsychological researcher (Hall, 1962, 1984; Lamont, 2005; Stein, 1993), but these were unknown to the scientists who criticised Crookes' report on Home. The point to note here, however, is not just that scientists' response to the report may have been unfair, but more fundamentally, that the scientific establishment had voiced its implacable rejection of claims of empirical support for the paranormal, even when these claims were made by a member of its own elite.

The notional boundary between parapsychological research and legitimate scientific research therefore was firmly laid down even in the protoscientific phase of parapsychology, although the objective criteria to define this boundary had yet to be enunciated explicitly. It was sufficient at this stage that the scientific establishment 'knew' paranormal processes were physically impossible and could never be shown to be otherwise. This dogma was an instance of unadorned prejudice and occasionally was declared as such. Thus, during this era the great German physicist Hermann von Helmholtz boldly stated, "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 recognized channels of sensation. It is clearly impossible" (R. T. Birge, 1958; cited by Collins & Pinch, 1979, p. 244). Subsequent research with mediums undertaken under the auspices of the various societies for psychical research therefore was largely ignored by orthodox scientists or was casually dismissed as the misguided antics of crackpots.

J. B. RHINE'S PIONEERING STUDIES OF THE ESP HYPOTHESIS

In the two decades following World War I, increasing tension was evident within the American Society for Psychical Research (Mauskopf & McVaugh, 1980; Tietze, 1985). On the one hand there were religiously oriented members who wanted the society to maintain its focus on spiritualistic mediums and spontaneous parapsychological phenomena such as apparitional experiences in the idealistic hope of establishing the reality of the afterlife. More scientifically oriented members, however, advocated a shift to rigorous experimental investigation of basic paranormal processes such as ESP. The latter group was encouraged by a number of isolated experiments attempting to test the viability of the concept of ESP by asking participants to use their 'psychic powers' to identify concealed targets such as playing cards (for a summary of these studies see Pratt, Rhine, Smith, Stuart, & Greenwood, 1940, Table 29; Richet, 1923).

With this controversy raging in the background J. B. Rhine was appointed to the Department of Psychology at Duke University in the late 1920s and began to design and to conduct experimental studies of the ESP hypothesis. Rhine's research program had a number of advantages over most of the earlier empirical efforts toward this end. His studies were undertaken as part of his academic duties and thus carried the imprimatur of the university. Rhine designed standard and replicable procedures or experimental paradigms for the controlled investigation of ESP. Perhaps most importantly, Rhine utilised his training in the emerging science of mathematical probability as a basis for objectively evaluating the card-calling performances of his ESP subjects against what would theoretically have been expected on the basis of purely random guessing. As a result of these methodological improvements Rhine was able to establish what in effect was the first sustained program of tests of the ESP hypothesis. In this sense J. B. Rhine commonly is dubbed the father of modern experimental parapsychology (Rao, 1982), although he certainly had his conceptual predecessors, as has been noted above.

Rhine's (1934) book *Extra-Sensory Perception* described his gradual improvements in experimental protocols for the investigation of the ESP hypothesis. In Rhine's mind the primary function of the book, however, was to show how the ESP hypothesis could be approached in a rigorously scientific fashion, thereby encouraging other psychologists to undertake similar research. The success of some of his own experiments were cited not so much to establish the authenticity of ESP, but to indicate that there were individuals who may be able to perform consistently better than chance level and thus an experimental research program on ESP might well be viable. Rhine's exposition was also intended to demonstrate that this

line of research could be pursued with due methodological rigor and without any occultist overtones

There are indications that Rhine's book initially was received with cautious interest by a few members of the American psychological community (Brian, 1982; Mauskopf & McVaugh, 1980) and indeed most psychologists conceded ESP was a legitimate issue for research (Warner & Clark, 1938). Perhaps the policymakers in psychology believed that the notion of ESP had satisfactorily been dispensed with and that Rhine's work could safely be ignored as having no scientific interest, but certainly there was no immediate academic outcry. In any event, in 1935 *Extra-Sensory Perception* began to catch the public eye and not only became a national best seller, but also elicited much comment in the popular press and instigated a popular fad for ESP cards. Psychologists' assessments of Rhine's research then became vigorously antagonistic; it is uncertain if this belated reaction was due to the publicity given to the Duke University research, to the exaggerated claims made in the media, or to a resurgence of awareness that ESP was incompatible with accepted principles of behavioural science. But between 1934 and 1940 the issue of ESP raged as a major psychological controversy, with Rhine's work subjected to vehement criticism (for a summary see Pratt et al., 1940).

It must be stressed that only a small portion of the psychological community became actively involved in the ESP debate; Mauskopf and McVaugh (1979) claim that only about 30 of some 600 psychologists engaged in the formal exchanges. The attacks on Rhine nevertheless were exceptionally acrimonious, and Rhine himself felt he was being tried for 'heresy' (L. E. Rhine, 1983). Much of the comment was purely rhetorical: for example, the Duke research was ridiculed as being rooted in superstition and mysticism and as such it was held to be putting the science of psychology in disrepute. Challenges to Rhine's statistical analyses dominated the debate for some time, but in the face of declared support by eminent mathematicians and statisticians for the legitimacy of the analyses it soon became evident that psychologists were simply being intractable in relation to this facet of the debate (Mauskopf & McVaugh, 1979). Some rather facile methodological criticisms focused on Rhine's earliest experiments which he himself had acknowledged in the book as the very bases for improvements in the experimental protocol. Other methodological concerns at least were more rational than rhetorical, and cognizance of these was taken by Rhine in his later research (Pratt et al., 1940). The thrust of Rhine's book, however, was not to substantiate empirical conclusions but rather, to show that properly controlled empirical investigation of the ESP hypothesis was feasible, and it was for this stance that Rhine was attacked so trenchantly by the scientific establishment.



The ESP debate waged for several years and although it was never resolved its intensity waned after 1940. The view of elite psychologists nevertheless remained inflexibly unsympathetic to parapsychological research. Rhine had great difficulty in getting his experimental findings accepted for publication in mainstream psychological journals (Allison, 1979; Mauskopf & McVaugh, 1980) and he eventually decided to establish his own outlet for parapsychological research, the *Journal of Parapsychology*. People who had not even been directly involved in the debate remembered its rancorous atmosphere for many years, and it was probably this emotional context rather than the merit of any methodological criticisms that survived to maintain the divide between parapsychology and psychology during the following decades. Leading psychologists had been so bitter in their denunciation of parapsychology that their successors simply assumed parapsychological research had been shown to be beyond the pale. As late as the 1960s, when the psychedelic ‘consciousness explosion’ encouraged (young) researchers to turn their attention to parapsychological and other states of consciousness, older mentors recalled tales of the attacks on Rhine and cautioned their enthusiastic juniors that involvement in parapsychological research would irreparably harm one’s career prospects. Even a prominent sceptic writing in the 1950s acknowledged the dogmatism inherent in the prevailing zeitgeist, remarking on the “enormous, irrational prejudice on the part of most American psychologists . . . against even the possibility of extrasensory powers” (Gardner, 1957, p. 299). As parapsychological research findings continued to be published in journals that orthodox scientists did not read there was little awareness of any empirical evidence that might otherwise have weakened the perception of parapsychology as a purveyor of rejected knowledge.

#### PARAPSYCHOLOGICAL ASSOCIATION’S AFFILIATION WITH THE AAAS

In the 1950s several parapsychologists saw the need to establish a scientific organization to represent their interests and in particular, to promote the image of the discipline among orthodox scientists and the general community. To these ends the Parapsychological Association (PA) was founded in 1957. Like any scientific society the PA has set down various professional qualifications as criteria for membership; it holds annual conventions where research findings may be presented and policy may be debated; and its Board may create special committees with diverse responsibilities ranging from the promotion of the field to the formulation of guidelines for the ethical conduct of PA members. In recent years the PA

has had approximately 110 members, with another 100 or so listed as associate members. As in many scientific societies it would seem only a minority of PA members are active researchers.

In addition to a scientific discipline having its own society it is usual for this society to be affiliated with more general scientific associations to give it a voice in the formulation and promotion of science policy. In the United States this general body is the prestigious American Association for the Advancement of Science (AAAS). The Parapsychological Association was admitted to the AAAS in 1969. The achievement of this affiliation, however, was far from straightforward.

The saga of the PA's affiliation is documented by McClenon (1984) and in a document compiled in 1969 by Douglas Dean, the PA president and a principal protagonist in the application (Dean, 1969; see also Dean, 1990). Affiliation with the AAAS requires support in the form of a majority vote from a committee within the AAAS that exercises a judgement concerning the status of the applicant's field as a science or nonscience. This committee comprises elite scientists with an outstanding international reputation in their respective disciplines (McClenon, 1984). The PA therefore was unable to argue its case before the general scientific community, and under the scientific zeitgeist prevailing in the 1960s the PA found it very difficult to secure a favourable judgement from the AAAS committee.

Successive applications by the PA for AAAS affiliation were rejected in 1961, 1963, 1967, and 1968, essentially on dogmatic grounds. In 1961 the stated ground for rejection of the application was that "at present, parapsychology is not firmly or generally accepted as a science" (AAAS Minutes, cited by McClenon, 1984, p. 109). In reviewing this decision in 1962 the committee explained, "the use of unscientific methods of inquiry by an applying society could properly be used as a basis for denying affiliation" (AAAS Files, 1962, cited by McClenon, 1984, p. 109). The application in 1963 was again rejected because the committee still was not satisfied that the PA had met this requirement, putting the onus clearly on the PA to demonstrate that its methods were scientific and obviating any further need for the AAAS to specify why parapsychology's methods should be deemed unscientific.

In 1967 the committee actually supported another application by the PA, although it added that this decision should not be taken to imply an endorsement of the existence of ESP. The Board of Directors of the AAAS then decided, however, not to proceed with this recommendation, justifying its action in terms of the proposed introduction of a new committee for processing applications for affiliation. At the same time four other applications in the 1967 round were endorsed by the Board. Administrative

procedures were again used as the reason for not granting affiliation when the PA applied in 1968; the committee argued that it should properly await the completion of a planned review of standards and procedures for granting AAAS affiliation. When in 1969 a majority (5 to 2) vote finally supported the affiliation of the PA, the much heralded review of affiliation procedures was immediately terminated, thereby exposing the proposed review as little else than a delaying tactic. When the committee recommendation came before a meeting of AAAS delegates, about 85 percent voted in support of the motion for affiliation (Dean, 1990). At last most members of the AAAS committee had conceded that parapsychology “uses scientific methods of inquiry” (Dean, 1990, p. 8), and a substantial majority of AAAS delegates evidently concurred with this view.

Nonetheless, there was still some deeply-felt resistance to the affiliation of the PA: as Friedlander (1995) was later to remark sardonically, “This action was not universally popular” (p. 112). The official acknowledgment of parapsychology as a scientific discipline not only was grudgingly made, but also was taken as an affront by some members of the scientific establishment. Indeed, in the late 1970s there was a highly emotional plea by one AAAS member to have the PA disaffiliated (Hansen, 2001). Although this tactic was unsuccessful it testifies to the persistence of a residual dissatisfaction with the condonation of parapsychological research by the AAAS. In addition, although the AAAS had conceded that parapsychologists used the scientific method in their research, there remained a general consensus among mainstream scientists that paranormal processes themselves were certainly controversial and possibly nonexistent. One of the AAAS delegates who voted against the 1969 application remarked, “These so-called phenomena of parapsychology do not exist and it is impossible to do scientific work in this area, so that we have a null science” (Dean, 1990, p. 8). Thus, there may well be some truth in Allison’s (1979) description of the acceptance of the PA by the AAAS as only “token recognition” (p. 288).

#### META-ANALYSIS AND THE GANZFELD DEBATE

The affiliation of the PA with the AAAS had at least one positive outcome for parapsychologists: it effectively undermined the case of commentators who may have been inclined to dismiss parapsychological research out of hand as patently ‘unscientific’. Dean (1990) deemed the AAAS affiliation “a kind of intellectual turning point” (p. 8). The style of the parapsychology debate had to adjust to this changed intellectual environment.

In the 1960s, while the debate over AAAS affiliation was being waged, most sceptics were still inclined to focus on possible procedural flaws in parapsychological experiments. One of the major critiques of this sort was that offered by Hansel (1966) who argued that the findings of ESP experiments should not be accepted if the experimental protocol hypothetically admitted even a remote opportunity for fraudulent activity. Thus, Hansel chose specific ESP experiments and posed various scenarios under which the experimenter, the subject, or both in collusion could have cheated so as to generate the statistically significant data. It is doubtful, of course, that experimental studies of any psychological phenomenon would be immune to such a charge. As Wallis (1985) observed, “this can, if pushed to extremes, be a counsel of perfection impossible of achievement” (p. 596). That ESP findings might have been due to fraud is itself an unfalsifiable hypothesis of fundamentally rhetorical value. Thus, the implicit subtext of Hansel’s case was that parapsychological experimenters were fraudulent or at best, incompetent. While this approach may have had some rhetorical advantages for the sceptical movement, it became clear in the aftermath of AAAS affiliation that neither these extreme imputations nor methodological quibbles could provide the means by which the black sheep of parapsychology could be cast out from the scientific fold.

In light of the decision of the AAAS on PA affiliation, both sceptics and parapsychologists therefore began to take greater cognizance of possible criteria for defining the notional boundary between science and nonscience. The elementary notion that a nonscience did not observe the scientific method had failed to exclude parapsychology from AAAS endorsement. But clearly the application of scientific methods to the assessment of parapsychological hypotheses was insufficient to persuade many mainstream scientists that parapsychology had any authentic phenomena to investigate. Under this view the definition of a scientific discipline had to encompass more than the mere adherence to scientific procedures. Now, some philosophers of science had proposed that a legitimate science was characterised by replicable observations, that is, a valid experimental effect must be repeatable by other competent researchers. Thus, Heiman (1999) states, “The logic behind replication is that because nature is lawful, it is also consistent. Over many studies, therefore, the correct hypotheses will be consistently supported, while the erroneous, coincidental ones will not” (p. 26). Strong replicability or ‘repeatability on demand’ nevertheless could not be claimed for ESP experiments: as parapsychologists themselves conceded (Rhine, 1955) there was no guarantee that under one of Rhine’s card-guessing protocols any experimenter could educe a statistically significant performance from any experimental participant at any nominated time. Sceptics, too, had long

been aware of this; Cohen (1966), for example, had declared, "Obviously successful E.S.P. experiments are not repeatable and thus do not meet a basic requirement of all scientific experiments" (p. 552). The principle of replicability therefore was mooted anew as a key characteristic by which to demarcate parapsychology from legitimate sciences. Note in this context the concept of replicability still plays a largely rhetorical role; the very act of conducting a successful replication of a parapsychological (or indeed, any other type of) experiment appears to have limited academic significance (Neuliep & Crandall, 1993; but see also Alcock, 2003).

The earlier rhetorical exchanges on parapsychology's capacity to meet the scientific requirement of replicability had essentially been inconclusive because there was no definitive empirical method for assessing the claims of replicability or nonreplicability of parapsychological research; that is, an independent observer could not determine whether the statistically significant card-guessing performances were simply 'freakish' outcomes or, on the other hand, it was the failed replications that were anomalous if the record of observed findings were to be considered as a whole. A potential resolution of this difficulty came with the development of meta-analysis, a statistical technique enabling the aggregation of results across all known experiments (whether statistically significant or not) on a given phenomenon (Green & Hall, 1984). If the phenomenon was not replicable the aggregate record should show that the phenomenon's overall effect size was essentially zero. A statistically significant effect size over all known experiments, on the other hand, would attest to replicability, even if the underlying phenomenon was so unstable or so inherently weak that there could be no guarantee that any new study would inevitably be successful (Beloff, 1994).

There is now an extensive literature of meta-analyses of parapsychological findings, both in relation to the existence of psi (ESP and PK) itself (proof-oriented experiments) and the circumstances under which psi performance may be facilitated (process-oriented experiments); for a brief summary see Irwin and Watt (2007). Although parapsychological effects certainly cannot be claimed to exhibit perfect replicability, if replicability or 'robustness' is considered to vary in degree, it seems that the phenomena do show a statistically significant level of replicability. Indeed, for several parapsychological relationships the effect size seems to be reasonably impressive (Utts, 1991).

Two major objections nevertheless may be raised against the meta-analytic approach. First, no matter how assiduous the meta-analyst has been, it is unlikely that the database for the analysis has included every single experiment ever undertaken on the given phenomenon. In particular, an unknown number of experiments that yielded nonsignificant findings

may never have been written up and the data may well be lost in the depths of a filing system or have been destroyed. It might be pleaded that the inclusion of all these null findings in the database could have shown the overall effect size was essentially zero. Again, there is a meta-analytic technique that may accommodate this objection. This supplementary calculation provides an estimate of the number of statistically null studies the existence of which it would be necessary to assume if the effect size yielded by the analysis were to be reduced to nonsignificance. In the parapsychological meta-analyses that now routinely cite this computation it seems that the number of hypothetical null studies would have to be so large that any claim of their existence would be implausible. It should be noted, however, that there has been some criticism of the theoretical assumptions upon which these so-called 'file drawer' calculations are based (e.g., Scargle, 2000).

A second possible objection to the meta-analytic findings is that if the database comprises methodologically flawed experiments, or even a substantial number of such experiments, their aggregation surely should not be taken to attest to any genuine phenomenon at all; the apparent consistency of experimental outcomes may be a measure of the replication of procedural errors rather than an index of the replicability of a phenomenon (Hyman, 1986; Krippner et al., 1993). This difficulty may be addressed by coding each study for the presence of alleged or actual flaws and seeing if the effect size yielded by the meta-analysis is correlated with the frequency of each of these flaws. The identification of methodological flaws is of course an essentially subjective judgement, but at least in one series of rival meta-analyses of a parapsychological effect, a parapsychologist (Charles Honorton) and a sceptic (Ray Hyman) actually sought to resolve their differences in regard to this assessment. The parapsychological effect in question was the hypothesised facilitation of ESP by a technique of sensory restriction known as the Ganzfeld. The result of the collaboration was an agreement there was an anomalous effect here to be explained, whether or not the underlying process is truly paranormal (Hyman & Honorton, 1986). It is regrettable that similar fruitful collaborations between parapsychologists and their critics have been so rare in the ESP debate. A later meta-analysis of ganzfeld-psi experiments (Bem & Honorton, 1994) also indicated the effect size to be strikingly robust, although this continues to be a topic of vigorous debate (e.g., see Milton & Wiseman, 1999, 2001; Storm & Ertel, 1999, 2001).

In any event in the mid 1980s it seemed that meta-analysis would provide a means for resolving one of the major points of contention over the scientific legitimacy of parapsychological research. Thus meta-analytic findings prompted one parapsychologist to dub the technique "the

controversy killer” (Broughton, 1991, p. 279) because he believed it would clearly be irrational in the light of the meta-analyses to deny the reality of psi. A somewhat more realistic expectation was that the meta-analyses of psi experiments would at least dampen critics’ allegations on the issue of replicability (Krippner et al., 1993). Each of these expectations has proved grossly optimistic. Even in the mid 1990s sceptics still were boldly declaring, “in the field of parapsychology, there are no repeatable experiments” (Schick & Vaughn, 1995, p. 226). As we shall see, the scientific elite continued to construe parapsychological research as rejected knowledge.

#### THE NATIONAL RESEARCH COUNCIL REPORT

The National Research Council (NRC), a research arm of the National Academy of Sciences, was commissioned by the US Army to assess a number of techniques claimed to enhance human performance. One of the six topics addressed by the NRC investigation was psi phenomena (ESP and PK). The council’s report, *Enhancing Human Performance*, concluded, “The Committee finds no scientific justification from research conducted over a period of 130 years for the existence of parapsychological phenomena” (Druckman & Swets, 1988, p. 22). Given the status of the NRC within both the scientific and the general community this conclusion stood to have serious repercussions, particularly for the future funding of parapsychological research.

Parapsychologists were understandably perturbed by the council’s claim and on behalf of the Parapsychological Association a detailed response to the NRC report was prepared (Palmer, Honorton, & Utts, 1989). Some of the points made in the PA response are perhaps disturbing, but by the same token they serve to attest to the nature of the scientific establishment’s strategies in dealing with the field of parapsychology.

The committee that prepared the report on parapsychology for the NRC evidently took some rhetorical liberties. By way of illustration, the committee’s assessment was limited to four specific facets of parapsychological research that had been conducted in the previous 20 years, yet the authors’ conclusion was couched in terms of the entire history (‘130 years’) of the discipline. Rather more disconcertingly, the composition of the committee was deemed by Palmer et al. (1989) to have been purposefully prejudicial. None of the committee’s members were parapsychologists. The two main contributors to the parapsychology committee’s report were Ray Hyman (also the committee’s chair) and James Alcock, both academics with a long record of publishing negative

assessments of parapsychological research and members of the Executive Council of CSICOP, the popularist sceptical movement dedicated to ridiculing parapsychology and pseudosciences more generally.

Further, the NRC report's selection of reference material appears to have been selectively biased. When one commissioned background paper proved to present a favourable assessment of the ganzfeld-psi research, one of the authors of the background paper was reportedly asked by John Swets, a co-editor of the NRC report, to withdraw this assessment (Palmer et al., 1989); the authors did not do so, and in the parapsychology section of the final NRC report this background paper was not even cited. Hansen (2001) describes the latter as "a blatant suppression of data" (p. 199). By contrast, a background paper prepared by Alcock, the perspective of which was consistent with the committee's conclusions, was extensively cited, as was other sceptical literature supporting the committee's position. Certainly this evidence of bias rings of irony in light of Hyman's (1986) earlier sage assessment that on the issue of parapsychological research "the level of debate during the preceding 130 years has been an embarrassment for anyone who would like to believe that scholars and scientists adhere to standards of rationality and fair play" (p. 848). Finally, the NRC report on parapsychology sidestepped the emerging meta-analytic findings by declaring it nonsensical to think in terms of 'degrees' of a phenomenon's replicability: despite its clear applicability to a wide range of orthodox psychological phenomena this notion of replicability was deemed to be "quite different from the consistent and lawful patterns of covariation found in other areas of inquiry" (Druckman & Swets, 1988, p. 175).

There are reasons, therefore, to suspect the even-handedness of the NRC report on parapsychological research. According to Hansen (2001) the NRC has never deigned to reply to the criticisms presented by the PA (Palmer et al., 1989), but of course a politically empowered body can afford to ignore a fringe group's criticisms without compromising its strategic position. Many parapsychologists are inclined to view as deplorable the tactics used in the assessment of parapsychological research by such an eminent scientific body as the NRC. The fact remains, however, that such tactics are a feature of how contemporary scientific controversy is waged. As Feyerabend (1975) maintained, it is not so much the logic of the case that determines the outcome of a scientific controversy, but rather the rhetorical skills and political power of the advocates for each side. For parapsychologists the ultimate cause for concern should be not so much the tactics that are employed as the premise upon which this Machiavellian approach is based, namely, the perceived illegitimacy of parapsychological research.



THE CONTEMPORARY SCENE

Despite parapsychologists' repeated efforts to satisfy the demands of their critics the discipline of parapsychology continues to have a marginal status in the eyes of the scientific establishment. Pseudo-scepticism, the implacable *a priori* view that parapsychological knowledge 'just can't be true', continues to permeate the conventional scientific zeitgeist; Hacking (1993), for example, has sweepingly declared, "every claim to persistent, subtle but statistically detectable psychic phenomena has been refuted" (p. 591). Parapsychologists generally are regarded as deluded, incompetent, or even fraudulent. The use of rhetorical devices to belittle parapsychologists is commonplace; as Meynell (1996) observes, some sceptics are not above "subjecting to public contempt and ridicule those who sincerely report evidence which is not to their liking" (p. 25). This approach is especially characteristic of the popularist sceptical movement represented by CSICOP.

The scientific establishment's responses to parapsychology regrettably go beyond facile rhetoric in that criticisms are used as grounds for maintaining parapsychology's marginal status and denying it the privileges of a scientific discipline (Irwin & Watt, 2004). That is, some activities of sceptical scientists seem designed to perpetuate a cultural boundary between parapsychology and mainstream science.

One such activity is the obstruction of parapsychologists' access to orthodox journals. The editorial boards of many prestigious scientific publications appear to be unwilling to publish papers favouring parapsychological hypotheses. As Wallis (1985) remarked, this practice constitutes an implicit statement of the journal's nonendorsement of parapsychology as well as a form of censorship. In addition, parapsychologists' reports accepted for publication in such journals as *Science* and *Nature* frequently have been diluted by negative comments made in an accompanying editorial or in a commentary by scientists outside the field (Collins & Pinch, 1979); often such commentaries do not appear to have been subjected to the usual editorial processes. Parapsychologists who elect to publish in lower status specialised outlets (Martin, 1998) not only compromise their prospects for promotion, but also unwittingly serve the sceptics' cause by preventing their research findings being exposed to the widest possible audience. The field's lack of exposure is evident even in university textbooks: in introductory psychology texts the tendency is either to ignore parapsychological research altogether (Irwin, 1991) or to describe only weak and outdated studies (Roig, Icochea, & Cuzzucoli, 1991).

Many academics aspiring to undertake parapsychological research are discouraged from doing so. They encounter difficulties in obtaining

funds for their research; committees that assess research proposals and allocate public funds to them have frequently been found to be unsympathetic to parapsychology. According to Hansen (2001), "Sociologists, psychologists and folklorists are allowed to study *beliefs* about paranormal events, but there is a taboo against attempting to verify their reality" (p. 185). In academia a declared interest in parapsychological research still can harm one's chances of employment or promotion, and there continue to be cases of academics being dismissed (Cornwell, 1997) or ostracised (Hastings, 2002) for pursuing parapsychological research. Bem (1996; cited by Trocco, 2002) has cautioned, "There are only two kinds of people who should work on this [i.e., parapsychological research]: undergraduates and tenured professors. Everyone else is at risk" (p. 35). There may also be opposition to the teaching of parapsychology in universities and colleges, despite nominal endorsement of the principle of academic freedom in these institutions (Hansen, 1992; Hess, 1993). The lack of adequate formal acknowledgement of the field on college campuses severely hinders progress in parapsychology and may inhibit recruitment of new people to the discipline.

This is not to say that parapsychological research is, or should be immune from academic criticism. Considered critical analysis is intrinsic to scientific endeavour. Indeed, notwithstanding the trends of meta-analyses of parapsychological experiments, there is some merit in the charge that parapsychologists have yet to demonstrate they have a sufficient degree of experimental control over their hypothesised phenomena. At the same time this limitation could be interpreted as characteristic of a young science, or perhaps it is the case that psi phenomena are authentic, but inherently weak or little open to conscious production (Beloff, 1985). Be this as it may, properly conducted studies investigating the existence and nature of hypothetical paranormal processes do constitute a legitimate scientific endeavour, and it is understandable that many parapsychologists may resent the unyielding antagonism toward their research among orthodox scientists.

Perhaps for this reason recent reactions by parapsychologists to sceptical criticism are tending to become more radical. In the view of most parapsychologists sceptical scientists have been too bigoted and far too reluctant to acknowledge parapsychologists' accommodation of methodological and conceptual criticisms, but some parapsychologists and indeed other commentators have decided there no longer is any point in seeking to convince sceptics of the validity of parapsychological research and phenomena (Hess, 1993). That is, it is said the sceptics should be ignored. According to Zingrone (2002), for example, in the past parapsychologists were much too ready to pay attention to sceptical rhetoric. More fundamentally, Evans (1996) argues, "parapsychologists

should realise that it is futile to try to convince some of their skeptical opponents” (p. 75). As Alcock (2003) observes, the debate on the status of parapsychological research may be becoming “a *dialogue aux sourds*, a dialogue of the deaf” (p. 30), with arguments voiced by each side simply ignored by the opposition.

A few parapsychologists have gone even further in rejecting the need to respect contemporary criteria for scientific status, and they see parapsychology as the context of an imminent Kuhnian shift in the basic scientific paradigm (Kuhn, 1962). Such a shift is envisaged to extend far beyond the limited traditional contribution of a fringe science, namely, to discourage mainstream science from becoming atrophied (Friedlander, 1995). Particular appeal here is made to postmodernist and feminist views of science (Krippner, 1995, 2001; White, 1991, 1992, 1994). These writers depict parapsychology as a discipline that instantiates a broader disenchantment with the scientific, deterministic, reductionistic and mechanistic perspectives of contemporary science and their domination of cultural values. Thus, parapsychology is increasingly promoted as potentially playing a key role in the process of transforming modern science by giving due recognition to the value of alternative (e.g., holistic) research paradigms and to the spiritual and other nonmechanistic human qualities of the person. These increasingly radical views may well be deemed extremist and even antiscientific by conservative scientists, and at the very least they may be taken as symptomatic of parapsychologists’ dissatisfaction with the scientific establishment’s grounds for continuing to treat the findings of parapsychological research as rejected knowledge.

According to Friedlander (1995), “demarcation of the boundary between science and its imitators has been repeatedly attempted, never to everyone’s satisfaction” (p. 162). Perhaps in part for this reason the efforts of conventional scientists to create and sustain a notional division between parapsychology and legitimate sciences have been the focus of many academics interested in the practice of science from a sociological perspective. Instructive case studies of parapsychological research within this context include those undertaken by Allison (1979), Collins and Pinch (Collins & Pinch, 1979, 1982; Pinch, 1979; Pinch & Collins, 1984), Hansen (2001), Hess (1993), and McClenon (1984). These analyses serve to reinforce the conclusion that efforts to assert a demarcation between accepted and rejected knowledge have not been motivated primarily by the search for a defining criterion for legitimate scientific activity, but are more fundamentally an intrinsic feature of elite scientists’ sociopolitical strategies aimed to sustain the cultural image of science as the ultimate arbiter of knowledge. In this context the very subject matter of parapsychology, with all its occultist and superstitious associations for the general public, is seen

by many mainstream scientists as an affront to science and therefore the findings of parapsychological research, regardless of the methods by which these data are generated, continue to be construed as rejected knowledge.

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*School of Psychology*  
*University of New England*  
*Armidale NSW 2351*  
*Australia*

E-mail: [hirwin2@une.edu.au](mailto:hirwin2@une.edu.au)