# PARAPSYCHOLOGY AND THE AMERICAN PSYCHOLOGISTS: A STUDY OF SCIENTIFIC AMBIVALENCE

SEYMOUR H. MAUSKOPF
AND
MICHAEL R. MCVAUGH

The history of science is replete with cases of new scientific fields or specialties coming into existence, often accompanied by radical conceptual or methodological innovation, and finding a berth in established scientific activity. There are also the cases of old scientific interests being dropped from serious scientific consideration as "wrong" and outmoded or, worse, superstitious. Experimental chemistry and mathematical mechanics in the 17th- and 18th-centuries, experimental psychology in the late 19th are examples of the first set of cases, Aristotelian physics, alchemy and astrology of the second. Psychical research/parapsychology provides a unique intermediate example. This field has not yet really succeeded in finding acceptance into the mainstream of science, yet it has persisted in maintaining connections with science, claims to scientific attention, and its own dream of eventual incorporation into organized science. In order to examine the complex relation between parapsychology and more established science, I shall discuss the history of parapsychology's interaction with academic American psychology down to 1940, focusing on the reactions and attitudes of the psychologists to this would-be specialty of their own field.*

In his monograph *Extra-Sensory Perception* (1934), J. B. Rhine de-

*The material for this paper is largely taken from a book on the history of experimental psychical research and parapsychology which I am writing in collaboration with Professor Michael R. McVaugh of the Department of History, University of North Carolina. In particular, Professor McVaugh deserves the credit for the material of the second half of this paper, though I have reworked it somewhat (and therefore accept the responsibility for any misinterpretation or error of fact). We wish to thank Dr. J. B. Rhine for permission to use and cite material from his papers. Much of the research for this paper was done under NSF grants GS-39680/1.

fended the denotation of his subject as "parapsychology" by arguing that it was "beside" psychology—i.e., that psychology was the science most closely related to parapsychology.[1] It was particularly appropriate that this point be made by an American in the major *experimental* study of psychic ability. For there had long been a relation between academic psychology and psychical research.

This relation was never a broad one; it was only occasionally close; and moreover, it was usually acerbic. Yet nothing quite like it existed elsewhere, and the course of psychical research and parapsychology in the United States has been indelibly colored by it. It went back some fifty years prior to the appearance of *Extra-Sensory Perception*, and was to reach something of a climax in the decade after this monograph appeared.

Psychical research became systematically organized in the U.S. at just the time when academic psychology was coming into focus as a field distinct from philosophy, medicine, and psychiatry. In 1884, the American Society for Psychical Research was born. The Society's early membership reflected the interest (or, I might say, the concern) of some of the founders of American psychology. One of its first Vice-Presidents was the great rival to William James as founder of American psychology, G. Stanley Hall. Active members included James himself, Joseph Jastrow (the first Ph.D. in American psychology and Hall's own student), Morton Prince of Harvard, and the psychologically-interested philosophers, Charles Peirce and Josiah Royce.

By 1900, American psychology had grown into an established and visible scientific field, with major emphasis in experimental psychology. There were flourishing graduate programs in psychology here at Harvard, Johns Hopkins, Cornell and Chicago, with departments at many other universities and colleges.

Organized psychical research in America had not been nearly so successful. The original ASPR disbanded and was absorbed by the London SPR in 1890, not to be revived in the U.S. until 1907. That the ASPR went under so rapidly was partly due to the fact that American psychical research suffered a lack of sympathetic intelligentsia to support it (in comparison with European countries). There was hardly anyone in the U.S. *ca* 1900 of the intellectual eminence of an Oliver Lodge, a Lord Rayleigh, or an A. J. Balfour in England, or a Charles Richet in France, to support psychical research. Hardly anyone, but not quite no one. For the psychologist William James, the most eminent of American psychologists, was highly sympathetic towards the study of psychic phenomena and carried out important studies himself.[2] And the other psychologists who belonged to the

original ASPR, most notably Jastrow and G. Stanley Hall, were, if not sympathetic, certainly concerned about psychical research. By this, I mean that they were skeptical and often scathingly critical of the field, but they were obviously intrigued by it. They were willing to be associated with psychical research societies and they took the research seriously enough to subject it to elaborate rebuttal.

As academic, experimental psychologists, their position (Hall and Jastrow) was that psychical research could only become a legitimate research subject insofar as it accepted the strategy, outlook, and professionalism of the experimental psychologist. As Jastrow put it in 1900: "If the problems of Psychical Research, or that portion of the problems in which the investigation seems profitable, are ever to be illuminated and exhibited in an intelligible form, it will only come about when they are investigated by the same methods and in the same spirit as are active psychological problems, when they are studied in connection with and as a part of other general problems of normal and abnormal Psychology. Whether this is done under the auspices of a society or in the psychological laboratories of universities is, of course, a detail of no importance. It is important, however, what the trend and the spirit, and the method and the purpose of the investigation may be; and it is equally important, what may be the training, and the capabilities, and the resources, and the originality and the scholarship of the investigator."[3] However, it must be admitted that there was a joker in the psychologist's attitude: Jastrow's and others' insistence on psychological training for the psychical researcher barely veiled a generally negative *a prior* attitude towards the existence of psychic phenomena and entities. Hence built into their attitude was a basic circularity nearly impossible to break. While they insisted on the rigor of experimental psychology to "test" psychical phenomena, they were sufficiently skeptical so as to be quite sure that such test could only yield negative results.

This complex of attitudes towards psychical research—and, it must be pointed out, the seriousness with which it was viewed—was clearly illustrated by the review G. Stanley Hall published of the first six volumes of the *Proceedings* of the SPR and of Gurney, Myers & Podmore's *Phantasms of the Living*, in 1887, in the first volume of his *American Journal of Psychology*.[4] Hall's review is surely one of the longest and most elaborate critiques psychical research has ever received from a psychologist. The first part of this review was given over to criticism of experimental techniques and here he was scathing: the English experimenters had omitted crucial details of their set-ups, had provided incomplete protocols, and had in general shown remarkable naiveté

concerning the possibility of fraud and hyperaesthesia. Moreover, to the experimental psychologist, it would have been important to record the details of *mistaken* guesses for the patterns they might reveal concerning the sensory modality closest to the "psychic" transfer. For example, consistent confusion of "9" and "5" in number guessing might well suggest that the means of communication resembled auditory transfer of information. None of this had been done. Hall's point in all of this was to emphasize what he considered to be the unprofessional and casual procedures of the English investigators.

But what if rigorous and competent methods were used? To Hall, they could only play a destructive role. For he was convinced that telepathy (the subject of his review) did not exist. The reason for his conviction seemed to stem in part from his scorn for the spiritualist associations of psychical research. But more specifically, telepathy seemed to undercut the psycho-physiological basis of mental activity. Telepathy, he argued, was in conflict with the law of "isolated conductivity," whereby sensory signals passing along one nerve fiber did not jump to another no matter how close they were. Hall asked rhetorically of telepathy: "Is it likely that a neural state should jump from one brain to another, through a great interval, when intense stimuli on one nerve cannot affect another in the closest contact with it?" Given not merely the difficulty of a possible neurological mechanism for telepathy but indeed its inconceivability (at least as far as late 19th-century neurology and physiology were concerned), even the use of statistical evidence favoring telepathy did not impress Hall.

Similar refutations of the possible existence of telepathy were leveled by other psychologists. Hugo Münsterberg at Harvard, for example, argued that the purported existence of psychic phenomena threatened to undercut the entire program of psychology (and the other sciences) to construct a public, objective mechanical world-view, and, as a result: "We reject every claimed feat in which the psychological fact were without a physical substratum, as in the case of departed spirits and those in which psychical facts influenced one another without physical intermediation, as in telepathy."[5] For good measure, Jastrow injected another theme into the chorus of objections to psychical research: the professional one. By its prominence in the public mind and its popular association with psychology, psychical research gave a false—and damaging—public image to experimental psychology: "The right appreciation of scientific aims and ideals by the intelligent and influential public has come to be almost indispensable to the favorable advancement of science. Psychology can less afford than many another science to dispense with this helpful influence; and no science can

remain unaffected by persistent misinterpretation of its true end and aims. If Psychical Research is to continue in its present temper, it becomes essential to have it clearly understood just how far its purposes and spirit are, and how much farther they are not, in accord with the purposes and the spirit of Psychology. The optimistic psychologist anticipates the day when he will no longer be regarded, either in high life or in low life, as a collector of ghost stories or an investigator of mediums."[6]

Thus, serious though they may have been about psychical research, the majority of American psychologists at the turn of the century could hardly be called "encouraging" in their attitude to it. To be sure, there was the major exception of William James. James' attitude towards psychical research was almost the mirror-image of Hall's and Jastrow's. Unlike them, he was open-minded to the possibility of supernormal psychic phenomena and even to the spiritualist hypothesis. His subtle, complex, holistic psychology made him much more conducive to tolerance on these matters than almost all of his colleagues. James participated actively and enthusiastically in the work of the British and American SPR and he devoted much time and energy to studying and publicizing the psychic abilities of Mrs. Leonore Piper. By the same token, however, James did not make an issue of the necessity for psychical research to be a laboratory experimental science. He himself conducted no laboratory studies in psychical research and seemed content to base his conclusions on his personal investigations of mediums like Mrs. Piper or upon reports of his English colleagues.

But, by-and-large, the skepticism and suspicion in which psychical research was held by most American psychologists, was the dominant attitude. Insofar as they took it seriously, it was to debunk the field, not to do any positive research themselves, much less to teach it. This was brought out vividly in two surveys of the place occupied by psychical research in the American university system, one of 1898 and the other of 1917.[7] In the first survey, eleven psychologists were polled; in the second, twelve. In the first, only two psychologists gave any positive or substantive evidence of teaching about psychical research (H. Gale of Minnesota, the survey-taker, and William James); in the second survey, no one admitted to treating the subject in any but a most peripheral way. (James was dead by then.)

Yet the very existence of these surveys shows that, to some people, psychical research was considered naturally to have its home in the network of American psychology departments, if anywhere. And, by 1917, despite the negative response to the second survey, experimental psychical research had in fact found a precarious home in the

psychology department at three major universities: Clark, Stanford, and Harvard. All three universities had been the recipients of special bequests for the furtherance of psychical research. The funds had been accepted with misgivings in all three cases, but at Stanford and Harvard research fellows in the psychology departments had been appointed: L. T. Troland at Harvard and J. E. Coover at Stanford.[8]

Troland was only appointed in 1916 and had time to carry out very limited experiments before America's entry into World War I disrupted his psychological laboratory. Coover had been appointed at Stanford in 1912. In the ensuing five years, he had had time to carry out what was certainly the most ambitious set of experiments in psychical research to that date. His results were published in a massive monograph by Stanford University in 1917; Troland's were published in the same year in a small, privately printed pamphlet.[9]

The research that both these men did—trained psychologists as they were—conformed closely with the attitudes towards psychical research dominant in psychology. It was carried out as experimental psychology, in the laboratory rather than the séance parlor. Test materials which were both manageable and measurable were used. Each man's work reflected aspects of experimental and theoretical approaches then current: Coover, for example, used the elaborate introspection techniques of Wundtian-Titchnerian psychology; Troland's principal theoretical idea was the reflex-arc concept, then so prevalent in functionalist and behaviorist psychology (although Troland was not a Behaviorist). Moreover, Troland introduced a thoroughgoing mechanized test situation, so as to promote exact reproducibility and "eliminate the personal equation of the researcher." Both made use of statistically analyzable data.

In one other respect, Troland and Coover conformed to the attitudes of their fellow psychologists: both were skeptical about psychic phenomena. If it goes too far to characterize them as *a prior* disbelievers in telepathy, it would also be a distortion to say that they expected to find anything in their data. Coover's conclusion to his massive testing program was: "That no trace of any objective thought-transference is found either as a capacity shared in a low degree by our normal reagents [subjects] in general [Richet's 'suggestion Mentale'] or as a capacity enjoyed in perceptible measure by any of the individual normal reagents."[10] A conclusion which J. B. Rhine, R. H. Thouless and Cyril Burt were later to criticize as not really warranted by the data. Troland, compiling very limited data (some 605 trials) was unable to conclude much of anything (he did note that his results were 1.5 times the standard deviation below chance). Neither man ever published any

experimental sequels to his 1917 *opus* (although Coover remained Psychical Research Fellow at Stanford until 1937).

Nevertheless, psychical research had been carried out in psychology departments of two major universities, something unprecedented in any other country, and itself a reflection of the degree of seriousness with which the subject was taken by early American psychologists. Yet the concomitant skepticism and even hostility made the housing of psychical research in university psychology departments very precarious. 1917 could well have marked the end of academic psychical research in the U.S.

That it did not was due to the arrival at Harvard of a new Professor of Psychology in the fall of 1921: William McDougall. McDougall was then in his fiftieth year, a psychologist of international reputation. Moreover, he was highly sympathetic to psychical research; and indeed had been President of the London SPR in 1920. In part, this attitude was a reflection of his English background, where psychical research enjoyed much more sympathy from the academic elite than it did in the United States. In part, it reflected the Jamesian orientation of McDougall's own psychology. In part, it was connected with McDougall's own general anti-materialism, expressed in his belief in what he himself called psychic "animism" and in purposive behavior of the organism, as well as in psychic events and abilities.

Being the senior Professor of Psychology at Harvard, McDougall was in a position to do something for which there had been no opportunity in England—to revive psychical research there under the auspices of the special bequest of the Hodgson Fund that had supported Troland's work. In 1922, a young psychologist who had, in fact, assisted Troland back in 1917 and now was completing his Ph.D. at Columbia, was named Hodgson Fellow. This was Gardner Murphy. Unlike his predecessor Troland (as well as Coover and most American psychologists), Murphy not only took psychical research seriously, but was highly sympathetic to it. In this respect, he resembled both William James and McDougall. In his actual psychic research, Murphy's resemblance to James extends further, for Murphy never felt easy about using strict "laboratory" techniques, or using readily quantifiable materials to test for psychic abilities. Rather, he felt more at home with psychic incidents or psychic material which had rich emotional associations for his subjects.

Murphy was Hodgson Fellow from 1922–1925 (publishing none of his results). He was succeeded by G. H. Estabrooks from 1925–1926. With McDougall's removal from Harvard to Duke in 1927, subsidized psychical research at Harvard once again lapsed; Hodgson Fund

money was not to be used again to support psychical research (parapsychology) until 1938.

But during McDougall's tenure at Harvard, psychical research received a wider academic exposure than at any previous time. Various graduate students assisted the Hodgson Fellows. Thus, Harry Helson who acted as Murphy's assistant, reminisces about how he and Murphy used to visit McDougall "to talk about our last séance with Margery or about some medium I had investigated or about the phenomena at our last table-tipping session in the laboratory."[11] Reference to "Margery" brings to mind the great psychic *cause célèbre* of these years: "Margery, the medium"—the wife of a Boston surgeon whose abilities were investigated under the auspices of *Scientific American* and on whose Investigation Committee sat McDougall and Murphy (as alternate). Not only did "Margery" receive national publicity; she also elicited considerable interest among the Harvard faculty and advanced students. The most notable psychologist to express curiosity (even taking part in sittings with her) was Edwin G. Boring, about whose interest in parapsychology we shall hear more presently.

One result of the focus of interest in psychical research at Harvard in the mid-1920's was the organization of a Symposium in November, 1926, at Clark University under the sponsorship of its psychology professor Carl Murchison. The papers of the meeting were published as the book, *The Case For and Against Psychical Belief.* McDougall's paper was a plea for university sponsorship of psychical research; Murphy's, a plea for experimental research.[12]

But despite this uptake of activity and curiosity, the thread of serious interest in psychical research on the part of academic psychologists was, in fact, a very thin one in the late 1920's. The reasons are not hard to discover. At best, the revival of the Hodgson Fellowship and the hubbub over "Margery" in the mid-1920's had been evanescent. None of the Hodgson Fellows had continued with serious psychical research; even Gardner Murphy, who was to return to active interest in the 1930's, had largely gone on to more orthodox projects after 1925. Coover was silent out in California. To the community of academic psychologists the results of the work at Stanford and Harvard could hardly have looked very exciting. Indeed, they were unlikely to have known of any of it except possibly Coover's, and his was the one study with a strongly negative conclusion.

Estabrooks, it is true, published an article on experimental telepathy centering on his own tests in *The North American Review.*[13] The article was a thoughtful one, but its overall tone was hardly encouraging. Estabrooks dwelt on the problem of fraud in psychical testing, citing an

ingenious trick played on himself by two sophomores, presumably when he was carrying out his tests under the *aegis* of the Hodgson Fund. While he by no means ruled out his own experiments as possible evidence for telepathy, noting that he had obtained above-chance results and a curious pattern of decline in success as each experiment proceeded and which would have been difficult collectively to fabricate, he refused to endorse them either, because of the real possibility of fraud and incomplete safeguards against sensory cues. His conclusion reinforced the pessimism of the body of the article: "Thus, you see, telepathy is still as [sic] unanswered puzzle. Those cases which seem most striking are very hard indeed to refute. But proof in science is repeatability, and every time we attempt to repeat these experiments in the laboratory we have a dismal failure. Why? I do not know. Possibly because the whole thing is a mass of fraud, superstition and faulty observation. But also possibly because we cannot introduce violent emotion into laboratory technique. Which of these alternatives is correct we must leave to the future."

There was thus no reason for academic psychologists either to see in experimental research the potential for fruitful endeavor or a serious anomaly to be debunked. Even for a James, a G. Stanley Hall or a Jastrow, psychical research had been peripheral to their principal professional work back at the turn of the century. By the late 1920's, James and Hall were long since dead; Jastrow was quiet. Their intense reactions to psychical research, both pro and con, had been all but swamped by the silence and indifference of a new generation of psychologists who had research interests of their own to pursue. Academic experimental psychical research had all but died a second time, a mere decade after the first near fatality.

It would take nothing less than a spectacular claim of university-based research to rekindle interest (and opposition) among psychologists. This was provided, once again, under McDougall's auspices at Duke University, where he went in 1927 and in the work of J. B. Rhine. Beginning in 1930, Rhine conducted experimental tests with Duke students, the results of which were written up in monograph form by the fall of 1933 and published as *Extra-Sensory Perception* in April, 1934. The importance of Rhine's book was recognized in psychical research circles at once, although it was by no means received with unqualified approbation. Rhine had not only discovered spectacular test subjects, but had also brought together into synthesis many of the issues, approaches and conclusions of the earlier psychical research tradition.[14]

Rhine also had tried consciously to relate his work to experimental

psychology, as his choice of the term "parapsychology," indicated. He gave considerable attention to the psychological aspects of the ESP test situation: the conditions of stress and relaxation, attention and abstraction, interests and boredom; the effect of stimulants and depressants, of the presence of outsiders or of change in the nature of the test. He systematically preceded his account of each major subject with a personality profile, intending thereby to suggest correlations between conscious or unconscious motivation and scoring. The very test material—the ESP cards (or "Zener cards" as they were originally called)—was designed by Rhine's colleague in the Duke psychology department, Karl Zener, with the idea of insuring greater uniformity of response from the subjects than playing cards could, as well as much greater computational ease. The strong emphasis on statistically-computable card guessing as the testing method for psychical ability was also part of the tradition of American academic psychical research.[14a]

Rhine's monograph certainly contained the ingredients to engage the community of academic psychologists. But the psychologists were at first relatively slow to react. In part, this had to do with Rhine's own professional relation to psychology. Rhine was not a trained psychologist. It was only after he had been at Duke a few years that he became a member of the Duke psychology department. Although he was a member of that department by the time *Extra-Sensory Perception* was published, he had as yet no professional credentials in that field and presumably few professional acquaintances outside the local university departments and those psychologists with whom he and his wife had come into contact during the year they had spent in auditing courses at Harvard before coming to Duke.

Moreover, *Extra-Sensory Perception* was published by a source not likely to be readily available to psychologists: the Boston SPR. As a result, there was little reaction from the psychologists to Rhine's monograph throughout the rest of 1934. The few responses that there were in that year—three in number—bear out this point. For two of them were from men with long involvements in psychical research: Joseph Jastrow (a very negative response[15]) and Gardner Murphy (an enthusiastic review in the *Journal of General Psychology*[16]). And even though the third respondent, R. R. Willoughby, had had no previous direct involvement in psychical research, he was at a university with at least marginal involvement (Clark University); indeed, Willoughby's own salary had been at first paid out of the psychical research fund there.[17]

Willoughby's attitude presaged what was going to be a general

response of psychologists over the next two years. He was skeptical but interested enough to try seriously to grapple with the methodological problems of ESP research. His first tack was to probe the statistical assumptions and methods Rhine had employed, questioning Rhine's use of a theoretical normal distribution without an empirical check.[18] Added to this later, were criticisms of the experimental method as reported in *Extra-Sensory Perception*. Despite his search for weaknesses in Rhine's work, Willoughby was intrigued by it; he remained in active contact with Rhine, paid a visit to the Duke laboratory in April, 1935, and, in 1936, put an undergraduate of his on to testing for ESP.

By 1936, more psychologists had reacted to Rhine's work; the interest in psychical research which had lain dormant for so long in the academic psychological community was beginning to revive. Some of this interest was stirred up among young psychologists who had had connections with Duke and/or Stanford: R. C. Carpenter of Bard College, R. Wilfred George of Tarkio College and Eugene Adams at Colgate (philosophy) were examples. But for others, their attention was caught by the publicity that Rhine's work had received in the press and periodicals. Rhine's monograph had been picked up by a group of science writers. Some of these, like Waldemar Kaempffert of the *New York Times*, had had previous involvement with psychical research.[19] In addition, their synoptic view of science, necessitated by their work as science writers, made these men aware of the profoundly unsettling implications of the revolution which had taken place in physics in the 1920's, for the traditional materialism which had been the philosophical bulwark against the claims of psychical research. Largely through the activities of the science writers, Rhine's work was placed and kept before the public.

As a result of dissemination through personal contacts and through the newspapers and periodicals, Rhine had received inquiries about his work from at least a dozen psychologists by late 1936. Some of his correspondents carried out their own tests, occasionally with unsettlingly (to them) positive results.[20] Perhaps the most notable indication of the rising interest of psychologists in Rhine's work was an invitation to him from Edwin G. Boring of Harvard to speak to the Psychological Colloquium about his work. Boring had been seriously interested in (if skeptical about) psychical abilities and phenomena ever since he had come to Harvard in 1922. He had taken part in sittings with "Margery" and in connection with this, he had published an article in *Atlantic Monthly* in 1926 in which he had offered thoughtful if strong criticism of psychical research.[21] His main criticism, one which became something of a keynote of his, was that psychical research was sterile

for scientific research in that it asserted *negative* propositions—that psychical phenomena and abilities were produced by no normal means. Yet, Boring was by no means unremittingly hostile or close-minded and in 1935 he was one of the psychologists who contacted Rhine about his work. Before and after Rhine's visit to Harvard, Boring and Rhine engaged in extensive epistolary discussions of the issues of parapsychology.

The visit took place on November 18, 1936. Rhine had some quite understandable trepidation about how he and his work would be received. But, in fact, he appears to have impressed his auditors with his thoroughgoing thoughtfulness, not only in handling all their objections but also raising (and answering) some they hadn't brought up.

By the start of 1937, then, parapsychology seemed at last on the verge of receiving some acceptance from the academic psychological community as a "normal" field of research. Significantly, Gardner Murphy at Columbia, who had been quietly supportive of Rhine's work, became more public, publishing a popular article in *The American Magazine* in November, 1936,[2] and making a request to Boring in the Spring of 1937 that the Hodgson Fund be reopened for use in psychical research.[23] It must be pointed out, however, how very difficult it is to gauge just how far the favorable change in attitude towards parapsychology had proceeded and exactly what it meant, due to the scanty and fragmentary nature of our evidence for these years. That the subject was discussed informally at professional meetings we know was true, but equally apparent is its absence from the professional correspondence between psychologists.[24] Those psychologists who did take up the testing for ESP did not advance beyond the attempt at replication to any of the more purely psychological issues Rhine had raised in his monograph; indeed, the parapsychologists themselves no longer had the striking success in finding good subjects that they had achieved in the early 1930's. It seems clear that to most psychologists, parapsychological research remained remote from their interests.

Yet no newly-developing sub-field attracts more than a small core of researchers at first. Parapsychology had clearly elicited serious and even sympathetic interest from a group of psychologists and showed many of the signs of building quietly into a sub-specialty of academic psychology. But, by the fall of 1937, there were ominous signs of impending confrontation. Before then, although Rhine had had his critics, such as Jastrow, they had kept their criticism private. Now, in October, 1937, the first published criticism by a professional psychologist, Chester E. Kellogg of McGill University, appeared in the semi-popular *The Scientific Monthly*.[25] This was followed by journal

articles, book reviews, and denunciations and defences at professional meetings through the winter and spring of 1937–1938.

The cause of this series of outbursts against parapsychology lay, ironically, in the same source of much of the earlier interest: publicity. From 1934 to the end of 1936, the publicity parapsychology had received, although generous, had remained fairly constant in level and format, being mostly newspaper and magazine articles by science writers, newspapermen and occasionally Rhine himself. But in November and December 1936 there appeared two articles in *Harper's Magazine* by the chairman of the English Department of Columbia University, E. H. Wright.[26] The articles, well-written synopses of Rhine's work, in themselves added nothing new to the discussion of ESP. But they were by an academic in a journal widely read by academics and the well-educated generally, on the one hand; on the other, Wright was not a scientist and yet was discoursing on and evaluating a controversial matter of scientific, and particularly psychological, import. Wright's articles became the point of departure for Kellogg's *Scientific Monthly* attack.[27]

In 1937, publicity for parapsychology intensified and broadened out to include new forms. Weekly radio broadcasts by the Zenith Radio Corporation began September 5 and included both dramatizations and mass telepathy tests. In October, a popular book on parapsychology by Rhine, entitled *New Frontiers of the Mind*, was published and made a Book-of-the-Month Club selection. Also at about this time, the Zenith Corporation made ESP cards available on a commercial basis to the public.

One usually thinks of scientific controversy as being engendered by developments within science itself. This one, however, seems to have been largely brought about by extra-scientific factors. Indeed, when, in the midst of the controversy, an important new research development was published—the work in psychokinesis—it did not cause so much as a ripple of reaction from the psychologists. There is no question that some psychologists, who either had not previously given parapsychology much attention or who had thought it unnecessary to state their views, were stung into action by what they took to be unwarranted publicity for a field which, in their eyes, still had to establish its scientific credentials.[28]

But, however motivated and however emotional in tone, most of the critiques concentrated on the specific issues of ESP investigation. Not for these psychologists (in print, at least) were the broad philosophical questions which had exercised predecessors of theirs, like Münsterberg. One would be very hard pressed to identify the "school" of

psychology to which any of the critics adhered from their published criticism of parapsychology. Issues of materialism vs. the anti-materialistic implications of parapsychology were certainly raised at this time, but by the science writers and the book reviewers, not by the psychologists.[29] The psychologists concentrated on statistical issues (particularly that having to do with statistical distribution) and methodological issues (adequate safeguards, the problem of recording errors, optional stopping, selection of data, etc.).[30]

The active opposition to parapsychology among psychologists was actually quite small, if vocal. What was the attitude of the professional psychological community generally to parapsychology in the wake of the publicity and controversy? Unlike the situation in 1936, we are in a position to give some answer to this—from questionnaires sent out to members of the American Psychological Association in February and in July, 1938, to ascertain just this.[31] The responses to the July questionnaire (which survive in the Rhine papers) show that the events of 1937 had by no means caused the profession generally to adopt a hostile posture towards parapsychology. 352 of the 603 full members of the A.P.A. replied. Regarding the existence of ESP, 5 accepted it as established, 26 admitted it as likely, and 128 as a remote possibility; 142 labelled it as "merely an unknown" and 51 ruled it out as impossible. As to its investigation, 89% agreed that this was legitimate scientific research and 76% accepted it as "within the province of academic psychology." Interestingly enough, there was practically no disparity in age between the mild-to-active espousers and the strong opponents of ESP. Where any differentiation lay was in professional activity: clinicians and abnormal psychologists tended to be sympathetic; experimentalists, critical.

The survey reflected the persistence of the ambivalence towards parapsychology which had characterized American psychology earlier in the century. Skeptical though most psychologists were of ESP, most could not bring themselves dogmatically to rule it out as an impossibility and they were also willing to see it treated seriously. If the publicity of Rhine's work in 1937 had highlighted the opposition within psychology to parapsychology, it had also brought out the felt need to examine the issue seriously. In order to do so, a symposium on parapsychology was arranged for the national A.P.A. meetings held in Columbus, Ohio in September, 1938. John Kennedy of Stanford and Harold Gulliksen of Chicago were the critics; Rhine and Murphy the principle defenders of parapsychology.

Not unexpectedly, confrontation more than dispassionate examination marked the session, even to the factionalized auditors. Yet, to

many minds, parapsychology came out ahead from this session, the reasonableness of Rhine and Murphy contrasting with what looked to some psychologists as unscientific dogmatism on their opponents' part. Psychology and parapsychology had, for once, officially been brought nto contact at a national meeting.

In the ensuing developments, there were signs that the events of 1937–1938 actually enhanced the potential for parapsychology's winning a niche in psychology. In 1939, Rhine was voted Associate Membership in the A.P.A.; an Advisory Committee of A.P.A. members was established to vett and comment upon articles submitted to the *Journal of Parapsychology*. The parapsychologists, for their part, made efforts to respond to criticisms and suggestions of the psychologists. The Pratt-Woodruff paper of 1939, for example, had an unprecedented degree of detailed description of the experimental situation and design, eliciting from the Advisory Committee generous praise: "The members of the Committee have been impressed with the thoroughness with which the experimental work had been conducted and the report written up. From the standpoint of 'repeatability' the report is very satisfactory. The procedure has been described in complete detail. Every step is explicitly written up."[32] In the early summer of 1939, Rhine and his group wrote a large-scale updating of the 1934 monograph, this one quite consciously written with the academic psychologists in mind and containing elaborate expositions and rebuttals of the main criticisms which had been levelled against experimental parapsychology. Titled *Extra-Sensory Perception After Sixty Years*, the book was published in 1940 and was the climactic effort by the Duke group to "normalize" parapsychology within academic psychology.

By the end of the 1930's, then, there were promising indications for the future of parapsychology *vis-à-vis* academic psychology. But the process of normalization did not advance beyond the level reached in 1939–1940. It would be beyond the scope of this paper to account for what went wrong. But I shall hazard some general thoughts. First, it should be apparent that to view the reception of parapsychology by psychologists in the 1930's and even earlier in blanket terms as the refusal of orthodox science to entertain radical innovation would be a gross oversimplification. It is perhaps closer to the truth to say that this reception exemplified the difficulties any marginal area of research encounters in gaining recognition and support, aggravated by the aspects peculiar to parapsychology: its background association with spiritualism and the occult, its radical departure from accepted scientific values, and its perennial appeal to the non-scientifically

trained public. Scientists had and continue to have mixed (and therefore, perhaps, all the more intense) feelings towards parapsychology, arising out of their own image of what science is and how it came to its present state. On the one hand, the spiritualist, occult and antimaterialistic context of psychical research and parapsychology are viewed by scientists as just those features of pre-modern belief that the Scientific Revolution and its aftermath succeeded in combatting and expunging from scientific consideration. On the other hand, there has always been an important component of scientific ideology since that very same Scientific Revolution, which has emphasized the virtue of open-mindedness towards radical innovation and the liability of dogmatism. Did not Galileo succeed by his daring? Was he not made to suffer because of the intransigence of his Aristotelian opponents?

There has always been a considerable degree of this ambivalence in the reaction of American psychologists to psychical research and parapsychology. In the post-1934 years, the component of open-mindedness began to come to the fore in the wake of Rhine's claims and, despite the reaction of some to the publicity of 1937–1938, continued to be present in the minds of most psychologists, as the 1938 surveys bear witness. But we must be careful to specify what this "open-mindedness" or "receptivity" towards parapsychology actually meant—and what it did not mean. Clearly, it meant willingness to allow Rhine's results and claims to receive serious consideration. But much less clearly did it mean that academic psychologists then or earlier were prepared to push the new field themselves: to do any sustained investigations, to encourage their own students to take up research in ESP, or to hire a parapsychologist for their department. As we have seen, there was a group of psychologists who did respond to Rhine's work in the mid-1930's, and there was a handful of young psychologists who elected to take up parapsychological research on something like a full-time basis. It is just possible that the *rapprochement* following the September, 1938, A. P. A. symposium might have led to a more significant advance of parapsychology into a branch of academic psychology. But from the first, research and career opportunities in parapsychology were severely restricted and most of the young psychologists who took up parapsychology in the 1930's left the field after only a few years each. And in any case, it would seem that events were overtaken by World War II and the ensuing disruption of academic life. Even Rhine's staff was cut to skeleton size. In 1946, parapsychology was forced to begin again the struggle for academic and professional acceptance.

## REFERENCES

[1] Rhine, J. B., *Extra-Sensory Perception* (Boston: Boston Society for Psychical Research, 1934), p. 7.

[2] There were also the neurologists and psychotherapists, headed by Morton Prince and James Jackson Putnam, who were sympathetic to psychical research from its inception. I thank Ms. Molly Noonan for letting me read her as yet unfinished dissertation, "Psychic Research and the American Scientific Community, 1880–1890" which deals with the attitudes of psychologists and psychotherapists to psychical research in Chapter 5.

[3] Jastrow, J., *Fact and Fable in Psychology* (Boston: Houghton Mifflin Co., 1900), p. 54–55.

[4] Hall, G. Stanley, *American Journal of Psychology*, 1887, *1*: pp. 128–146.

[5] Munsterberg, H., "Psychology and Mysticism," *Atlantic Monthly*, 1899, *83*: p. 82.

[6] Jastrow, *Fact and Fable*, pp. 76–77.

[7] Gale, H., "Psychical Research in American Universities," *Proceedings of the Society for Psychical Research*, 1898, *13*: pp. 583–587; J. H. Hyslop, "Psychical Research in American Universities," *Journal of the American Society for Psychical Research*, 1917, *11*: pp. 444–458.

[8] Dommeyer, F. C., "Psychical Research at Stanford University," *Journal of Parapsychology*, 1975, *39*: pp. 173–205.

[9] Coover, J. E., *Experiments in Psychical Research* (Leland Stanford Junior University Publications, Psychical Research Monograph No. 1, 1917); L. T. Troland, *A Technique for the Experimental Study of Telepathy and Other Alleged Clairvoyant Processes* (Albany, N.Y., The Brandon Printing Co., 1917).

[10] Coover, *Experiments in Psychical Research*, p. 124.

[11] In his autobiographical account in E. G. Boring and G. Lindzey (eds.), *A History of Psychology in Autobiography* (Vol. 5) (N.Y.: Appleton-Century-Crofts, 1967), p. 201.

[12] McDougall, W., "Psychical Research as a University Study,"; Murphy, G., "Telepathy as an Experimental Problem," C. Murchison (ed.), *The Case For and Against Psychical Belief* (Worcester, Mass.: Clark University Press, 1927), pp. 149–162 and 265–278 respectively. There were also articles by Coover and Jastrow.

[13] Estabrooks, G. H., "The Enigma of Telepathy," *The North American Review*, 1929, *227*: pp. 201–211.

[14] McVaugh, M. R., and Mauskopf, S. H., "J. B. Rhine's 'Extra-Sensory Perception' and Its Background in Psychical Research," *Isis*, June, 1976.

[14a] In *Extra-Sensory Perception*, Rhine claimed for parapsychology a place "clearly within the field of Psychology and, of course, full into the midst of Experimental Psychology," p. 6. Of course, many of the psychological points, and the card-guessing technique, had also been important in the psychical research tradition.

[15] As reported by Rhine to Lydia Allison, May 21, 1934, Rhine Papers, Department of Manuscripts, Duke University.

[16] Murphy, G., "Extra-Sensory Perception. A Review," *Journal of General Psychology*, 1934, *11*: pp. 454–458.

[17] Willoughby had come to Clark in abnormal psychology. One of the complimentary copies of *Extra-Sensory Perception* had been sent by Walter Franklin Prince to Carl Murchison at Clark. Murchison had probably given it to his associate, R. S. Hunter who, in turn, had interested Willoughby. There is extensive correspondence between Willoughby and Rhine in the Rhine papers.

[18] E.g. Willoughby, R. R., "A Critique of Rhine's 'Extra-Sensory Perception,'" and C. E. Stuart, "In Reply to the Willoughby Critique," *Journal of Abnormal and Social Psychology*, 1935–36, 30: pp. 199–207 and 384–388 respectively.

[19] Kaempffert had been on the Board of Trustees of the *American Society for Psychical Research* in the 1920's.

[20] E.g. J. F. Brown of the University of Kansas.

[21] Boring, E. G., "The Paradox of Psychic Research," *Atlantic Monthly*, 1926, *137*: pp. 81–87, especially 84–85.

[22] Murphy, G., "Things I Can't Explain," *The American Magazine*, 1936, *122, no. 5* (Nov., 1936), pp. 40–41, 130–132.

[23] This request following on Rhine's visit to Harvard and successful by the end of the year.
[24] Which we have examined at the Archives of the History of American Psychology, University of Akron.
[25] Kellogg, C. E., "New Evidence(?) For Extra-Sensory Perception," *Scientific Monthly*, 1937, *45*: pp. 331–341.
[26] Wright, E. H., "The Case For Telepathy," and "The Nature of Telepathy," *Harper's Magazine*, 1936, *173*: pp. 575–586 and *174*: pp. 13–21 respectively.
[27] Kellogg made this clear on the first page of his *Scientific Monthly* article.
[28] This was certainly true of B. F. Skinner, who also exposed the commercially prepared ESP cards of the Zenith Corporation as being imperfectly manufactured.
[29] The science writers stressing the relation of the anti-materialistic implications of the new physics to parapsychology; the *literati* book reviewers, curiously enough, defending traditional materialism.
[30] Perhaps the most searching of these being the one by Dael Wolfle, "A Review of the Work of Extra-Sensory Perception," *American Journal of Psychiatry*, 1938, *94*: pp. 943–955 (with reply by Rhine, pp. 957–960.
[31] The results of both surveys were published in the *Journal of Parapsychology*: L. Warner and C. C. Clark, "A Survey of Psychological Opinion on E.S.P.," 1938, *2*: pp. 296–301, and J. C. Crumbaugh, "A Questionnaire Designed to Determine the Attitudes of Psychologists Toward the Field of Extra-Sensory Perception," pp. 302–307. The Warner and Clark survey was the July, 1938 survey whose original data is preserved in the Rhine papers.
[32] "Letters and Notes," *Journal of Parapsychology*, 1939, *3*: p. 246.

## *DISCUSSION*

DOMMEYER: I'm sure that Dr. Mauskopf is aware of this, but it was of interest to me that Coover and the authorities at Stanford reacted in an antagonistic way to Rhine's successes. Coover had taken the stand, of course, against psychical research and yet here was Rhine and the Duke people having this success, so when Coover retired, and they were thinking of getting a replacement for him, there was great caution always about not taking in a Duke man to succeed him. They seemed to be very much afraid that if they had brought in a Duke man, there would have been successes recorded and this would have been very embarrassing to them. I don't know whether you care to comment about that, but it was an interesting phase of this history at Stanford University.

MAUSKOPF: I don't know anything about a fear that a Duke man might get successes. That I've never come across. I do know that Coover was very evasive and then antagonistic towards Rhine and any of Rhine's emissaries. I would say that Coover himself is perhaps the best exemplification of this dilemma that this ambivalence towards parapsychology presents. It's very difficult for me, and I gather for you as well, to really figure out exactly what was Coover's attitude and why did he stick with this.

DOMMEYER: Well, I think Coover's, at least, expressed view was that

he didn't regard Rhine's work as scientific. That was one of the reasons he didn't want Duke people in. He said that Rhine had nothing that would correlate with these excesses over chance scoring that he had got and therefore that it was not a scientific endeavor at all—that the excesses over chance had been the result of hidden variables, etc. This was the kind of thing he believed. So he didn't want someone trained in Rhine's methods to come over and succeed him. And there is correspondence that indicates that they feared that, if the Rhine people got in there using these unscientific methods, they would get successes in their results. Of course, Charles Stewart came in '42 and did get successful results and then this was officially denied by the university later on.

French: I just wonder if some of the ambivalence that you've been talking about amongst the orthodox psychologists might be due to the fact that in the history of psychology itself during the period in question, the orthodox psychologists were themselves occupied with the defense of their own subject against attacks from the physical sciences, so that the inclusion of parapsychology might have, in their view, made them even more susceptible to the kinds of attacks they were trying to fend off.

Mauskopf: I think you're probably right. There is relatively very little concrete evidence, although almost on *a priori* grounds it makes sense and we thought of that. Perhaps the only thing in my paper that bears on it, is the rest of the quotation from Joseph Jastrow of which I only read a sentence. At the very beginning of organized academic psychology, he says specifically, "We musn't allow this association with psychical research to continue. We have to make our case to the relevant fund-giving organizations." He puts it in those terms, "we need support, and if people think we're ghost-hunters and medium watchers, they won't support us."

Nicol: Dr. Mauskopf made a comparison between the early British SPR and the early American SPR and pointed out that the SPR of London got support from many scientists and scholars, whereas it was something rather different in the ASPR. The actual facts, Dr. Mauskopf, are somewhat different. The SPR was founded predominantly by spiritualists. They managed to bring in Frederic Myers and Edmund Gurney, who in turn persuaded Henry Sidgwick to accept the Presidency. There were hardly any scientists or scholars in the SPR in its early days. Apart from the physicist William Barrett there were two minor figures named Coffin and FitzGerald, and one Fellow of the Royal Society, Walter Weldon; and all four were spiritualists.

MAUSKOPF: I don't think I made, and I certainly didn't mean to make, the distinction that there were simply eminent scientists. I could see why it seems that way because I named Lord Rayleigh, Oliver Lodge, etc., but it was more the general intellectual support of eminent intellectuals of a variety of different disciplines in England as compared to America, that I was referring to.

NICOL: Rayleigh was not an original member, nor was Lodge; they came in later. By contrast, the American SPR was distinguished by the large number of scientists and intellectuals in its Council and membership. Nevertheless, in a few years it collapsed. I don't think the absence of spiritualists or the presence of scientists was the explanation of its failure. The membership consisted largely of prosperous Bostonians living in large houses in the Back Bay. But they failed to provide the necessary funds, and so in 1890 the Society died. The British SPR took over its assets, such as they were, and created an American Branch of the British SPR. Thereafter Sidgwick and Myers subsidized it very privately.

On another matter, I don't think that Stanley Hall is deserving of our respect. William James had serious reservations about him, saying that though Hall was not a liar in any vulgar meaning of the term, he had a talent for mystification. I may add that he practised a talent for misleading his readers. Hall and his assistant Amy Tanner conducted a series of investigations of Mrs. Piper, that very respectable lady medium. In a book nominally written by Dr. Tanner, Hall claimed that their report contained a full account of what transpired at the séances. But we have only to notice the peculiar brevity of the séance reports to realize that some items are missing. Indeed, William James afterwards wrote to J. G. Piddington, in London, that the alleged spirit of Hodgson had accused Hall of murdering his wife—no doubt an exaggeration, though the lady did die tragically.

Concerning the so-called "Zener cards," Dr. Zener told my wife (who spent a dozen years at the Parapsychology Laboratory), that he had objected to the use of his name for the ESP cards, because he was only one of several people who suggested symbols. In view of Zener's objection, the Duke Laboratory writers thereafter referred to the cards only as "ESP cards."

Lastly, Dr. Rhine's *Extra-Sensory Perception* is marred by a crippling weakness. The book consists mainly of reports of ESP experiments. Now, the test of the validity of any scientific report is: Could we, on the information provided, repeat the experiments under the same conditions? Unhappily, as R. H. Thouless pointed out, the book rarely describes what the experimental conditions were. Therefore nobody

can even attempt to repeat the experiments. We cannot judge whether the original experiments were sound or unsound. The book therefore falls by the wayside.

MAUSKOPF: I bow, certainly, to your superior detailed knowledge about the early period, especially. About Zener's invention of the ESP cards, that I would say is still a matter of obscurity and controversy. We've asked numerous people at Duke, and on the whole, colleagues of Rhine and Zener who were around in the early thirties, have supported the claim that Zener invented them. I must confess, I don't know precisely that Zener did, but the weight of the evidence at Duke, at least, is that he did invent them. The final thing I would say is that granting many of your points, I don't think that the main import of my argument is invalidated, namely, that these people were profoundly ambivalent in their attitude towards psychical research, and it is this degree of ambivalence that, as it were, allowed a little breach in the curtain of what otherwise might have been uniform opposition, to get parapsychology established precariously, and it's still precarious, in the American university system.