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THE PLACE OF J. B. RHINE IN THE HISTORY OF PARAPSYCHOLOGY

By Brian Mackenzie

When we try to sum up a scientist's place in the history of his field, we often point to his discoveries or theories that had a lasting influence in that field. Thus, the importance of Kepler in the history of astronomy comes from his laws of planetary orbits, the importance of Einstein comes from the theory of relativity, that of Fleming from the discovery of penicillin, and that of McClelland from the theory of achievement motivation. Sometimes this is an oversimplified way of describing a scientist's importance, but it usually does not misrepresent his contribution to an unacceptable degree.

In some cases, however, this procedure can be actively misleading. The importance of Wilhelm Wundt in the history of psychology, for instance, surely does not rest on his tridimensional theory of feeling nor on his contributions to the theory of innervation. These theories have sunk almost, if not quite, without a trace, as have the specific methods of introspective analysis that he developed. These contributions on which Wundt labored so hard are today regarded as little more than side issues, dead ends in the history of psychology. Were his importance to be estimated on the basis of them, he would rank somewhere behind E. H. Weber[1] instead of in the forefront of the pioneers of experimental psychology.

Instead, what makes Wundt one of those pioneers whose influence

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[1] Weber's very restricted work on sensory magnitudes was revived by G. T. Fechner and made part of the basis for psychophysics.

is still being felt is not what he did so much as how he did it. His theories were not simply sent out to make their own way in the world. They were promulgated as part of an extensive social endeavor of which Wundt oversaw every aspect. He established a laboratory, enrolled graduate students, founded a journal for the publication of experimental reports, and supervised enough dissertations that his students and his students' students dominated experimental psychology (especially in America) for almost fifty years (cf. Boring & Boring, 1948). Wundt's activities exemplified the rapidly growing system of scientific education in nineteenth-century Germany, combining the social and the scientific aspects of scientific innovation in such a way as effectively to establish experimental psychology as a scientific discipline (cf. Ben-David & Collins, 1966).

The case of parapsychology and the place of J. B. Rhine in its history are similar but even more pointed. Rhine's parapsychological theories were not his major, nor his most influential, work. More significantly, the main phenomena that parapsychology is concerned with did not need to be discovered when Rhine entered the field. They had been known, or at least talked about and reported, for a long time. Telepathy, clairvoyance, and various kinds of psychokinetic influence had been frequently described and ostensibly demonstrated since antiquity. Instead, what these phenomena, and the field of parapsychology as a whole, needed was to be *established.* This was the need that Rhine filled. The importance of J. B. Rhine in the history of parapsychology does not lie mainly in the many substantial technical contributions he made to the field. Instead, it lies in the establishment of a distinct scientific discipline of parapsychology to which these contributions could be made. Furthermore, it does not depend on the extent to which Rhine's views are currently accepted by psychologists or parapsychologists. It does not even depend on the extent to which they, or any related ones, are valid. This introductory point must be made strongly, because it is an important one. The achievement of establishing a scientific discipline cannot be evaluated in terms of any later judgments about the legitimacy or illegitimacy, the genuineness or illusoriness, of the subject-matter of that discipline. Even if the most extreme skeptics turned out to be right about parapsychology so that there was "nothing in it," that fact would not detract from Rhine's achievement of placing the study of parapsychology on a scientific footing.

This paper will attempt to explain what it means to say that Rhine established a field of scientific parapsychology. By contrasting Rhine's work with the work of a few earlier workers in the field, it will also

attempt to outline a few of the details of how he did it.

I. Early Attempts at a Scientific Study of Paranormal Phenomena

First, therefore, it is necessary to look at the work of a few people in the history of parapsychology and its precursors to see why none of them were successful in establishing their field as a science. By the precursors of parapsychology are meant mesmerism, spiritualism, and the kinds of methodical psychical research undertaken in the first fifty years of the Society for Psychical Research (SPR) after its founding in 1882. One of the noteworthy features of this history of parapsychology and its precursors, indeed, is the way that it neatly illustrates some contemporary ideas on what is required to establish a scientific specialty. The use by individuals of objective "scientific methods" as traditionally described is not sufficient; a social dimension is also necessary. The social dimension, furthermore, must be marked by a particular kind of intensive interaction between individuals. The mere shared commitment by a group to the standards and procedures of scientific method is also insufficient.

Mesmerism and Spiritualism: The Lack of a Community

That the use of scientific methods by individuals is not sufficient is shown by the examples of mesmerism and spiritualism. These were not for the most part scientific movements, of course. They were popular movements, and often quasi-religious ones. They were marked by mass enthusiasm for theatrical displays on the one hand, and by secret societies with occult doctrines on the other. But there were always a few individuals who, while more or less sympathetic to these movements, took a relatively sophisticated critical or experimental approach to the evaluation of mesmeric and spiritualist phenomena.

In reviewing the history of mesmerism, or animal magnetism, for instance, Alexandre Bertrand (1826) painstakingly tried to separate the wheat from the chaff in this movement. He showed how most of the phenomena could readily be accounted for by the power of suggestion, producing a state of heightened consciousness which he labelled "extase"; there remained, however, several apparently genuine cases of clairvoyance and thought-reading that called for further attention. Five years later, in 1831, the members of the Second French Commission to investigate animal magnetism showed themselves more than usually competent in experimentation. Their controls on

the performance of clairvoyants were as good, or almost, as those of a hundred years later and, repeatedly insisting that it was facts and not theories they were after, they somewhat diffidently reported the successful performances of some of their subjects (*Report on the Magnetic Experiments,* 1844). J. C. Colquhoun, in his many pamphlets and books promoting animal magnetism, laid particular stress on the evidential value of experiments that could serve as textbook examples of ABA case study designs in clinical psychology (e.g., Colquhoun, 1838, p. 16).

Turning to spiritualism, the first major experimental investigation was that of Robert Hare, a chemistry professor at the University of Pennsylvania. His *Experimental Investigations of the Spirit Manifestations* (1855) describes many ingenious pieces of apparatus he invented for quantifying the force of physical phenomena and for isolating the medium from the recording equipment. Some of Hare's apparatus was the prototype for that later used by the British chemist William Crookes in his researches on spiritualism in the 1870s. Crookes refined Hare's equipment and procedures, emphasizing the need for precise instrumental control over the circumstances in which the phenomena were to be produced. He then proceeded to successful experiments with two of the best known mediums of the time, D. D. Home and Florence Cook (Medhurst, 1972).

All of these writers, and others such as Esdaile (1846) and Gasparin (1857), made at least a good start toward a careful scientific study of paranormal phenomena. By this it is not meant that their methodology was faultless or that it was as rigorous as can be found in modern experiments in psychology or parapsychology. But they were trying. They give the strong impression of doing the best they could to find out what was genuine in mesmerism and spiritualism and what was not. Their writings shine unmistakably through the mass of enthusiastic and naïve tracts of the believers and the mass of scornful and often equally naïve tracts of the scoffers. When reading their works, one cannot help being struck at times by the cogency of their reasoning and the elegance of their experimental designs, and might well ask: Why were these not taken more seriously in their own time?

Unfortunately, there is a simple answer. They were not taken seriously because very few people were interested in a relatively sophisticated experimental approach to the study of these matters. Both the true believers and the scoffers already knew the truth about mesmeric and spiritualist phenomena. They were all true, and testified to the existence of transcendental cosmic forces (e.g., Cahagnet, 1850; Ballou, 1853); or they were all false, except for the ones that

could be assimilated to orthodox nineteenth-century physiology (e.g., Bennett, 1851; Hammond, 1876).[2] The writers with idiosyncratic scientific aspirations tended, therefore, when they were noticed at all, to be pilloried both by the enthusiasts, because they did not accept everything, and by the medical and scientific establishments, because they did not reject or explain away everything. Thus, Bertrand was virtually ignored. The Report of the Second French Commission was suppressed by the Royal Academy of Medicine that had commissioned it (Inglis, 1977, p. 165). Hare was "howled down" when he presented his experiments to the American Association for the Advancement of Science at its meetings in Montreal in 1854 and was subsequently denounced for his "insane adherence to a gigantic humbug" (Fodor, 1933, p. 158). Crookes's writings likewise evoked a storm of vituperative criticism from his fellow scientists, and he eventually abandoned the field for the safety of his more respectable chemical researches (Medhurst, 1972, p. 6).

Such criticism of research on paranormal phenomena is not altogether unfamiliar to modern parapsychologists. But these individuals lacked something more important than general scientific acceptance. They lacked anyone to talk to. That is, they had no reference group to which they could submit their findings with the expectation that they would be critically but sympathetically assessed. Approval by the scientific community at large is a very nice thing to have, but what is essential for the growth of scientific knowledge—or even pseudoscientific knowledge—is a restricted community of practitioners, a reference group, that can assess one's work as part of a shared endeavor. Such a reference group not only assesses and criticizes an individual's work, but also, because its members are engaged in similar work, to some extent insulates the individual from the values and priorities of both the popular and the general scientific culture and thereby provides the basis for a professional identity.

Lacking such a reference group, these individual researchers were under great personal pressure to conform to the demands of one or the other of the reference groups that did exist, that of the believers or that of the scoffers. In one way or another, many of them succumbed to this pressure. Thus, Colquhoun's methodological sophistication was highly variable. He quite cheerfully mixed his elegant experimental designs with blatant ad hoccery and special pleading for the truth and the glory of animal magnetism. Hare, after his

[2] There was also a third extremist viewpoint, holding that the phenomena were genuine and the work of the devil (e.g., Munger, 1857).

researches were rejected by his scientific peers, abandoned his professorial and experimental work and spent the rest of his days in transcribing messages from his father in the spirit world; most of his *Experimental Investigations* (1855) is devoted to these. Crookes did not fare quite so badly, but vacillated in a significant and revealing way. He fell first into the arms of the true believers, writing and publishing encomiums and poems of praise to Katie King, Florence Cook's materialized companion from the spirit world (Medhurst, 1972, p. 139). Sensationalistic charges have also been levelled about his personal relationship with the medium (Hall, 1962), but these are irrelevant here. Whatever his private actions were, Crookes's public statements clearly showed that he had quite lost the objectivity and rigor with which he had approached the study of mediumship originally. Like Hare, he had become intensely involved on a personal and subjective level with the spiritualistic subject matter of his researches. When he abandoned studies of mediumship and returned to his respectable scientific field, he salvaged his reputation and went on to win many honors. But that was a return to the fold of his original scientific reference group. He never again seriously attempted to thread his way between the conflicting demands of the spiritualist and scientific communities.

Psychical Research: The Insufficiency of a Community

Again, what was lacking for these isolated individuals was an appropriate reference group, a body of sympathetic but critical readers and discussants of their parapsychological researches who could criticize the details while ratifying the attempt. Just such a reference group was provided by the founding and growth of the Society for Psychical Research in 1882 and, to a lesser extent, by the subsequent founding of similar bodies such as the American Society for Psychical Research and the Société Metapsychique. These societies were founded explicitly to make possible a wide-ranging scientific study of psychic phenomena. They attracted the interest and membership of a substantial number of accomplished scientists and intellectuals. The active members carried out a wide variety of careful methodical investigations, published them in their semiprofessional and specialist journals, and made detailed technical criticisms of each other's work. They thus made up an effective reference group of scientifically oriented psychical researchers, sharing a commitment both to rigorous standards of evidence and to the importance and legitimacy of investigating psychic phenomena. In doing so, however,

they also showed that these are not enough, that the existence of a reference group with a shared methodological commitment is also an insufficient basis on which to found a science.

The researches of the SPR investigators were extremely, and intentionally, diverse. Their view was that careful scientific observations of all the classes of supposedly paranormal events would establish once and for all which ones were genuine and which ones were not, and that from observation of the genuine ones a general understanding of the paranormal would emerge by induction. Thus, they took their problems from the broad sweep of the paranormal as it was then conceived. In its opening manifesto of 1882, the SPR announced the formation of committees to investigate thought-reading or telepathy, or more generally "any influence which may be exerted by one mind upon another, apart from any generally recognized mode of perception"; mesmeric trance, mesmeric anaesthesia, clairvoyance, "and other allied phenomena"; odylic force, apparitions, and haunted houses, and the physical phenomena of spiritualism such as raps and materializations; and a further one to collate all the evidence already in existence on these subjects (Society for Psychical Research, 1882). These were the allegedly paranormal phenomena that commanded attention at the time, and it seemed reasonable—more, it seemed essential—to address them all. Some of these were investigated more intensively than others, but all were attempted and new ones were added. Furthermore, these investigations by the SPR and others were sometimes very careful and sophisticated. However, the wide range of their investigations led to a great dispersion of their attention and made it very difficult to relate the findings in one area to those in another except by reference back to the popular interest that was the source of all of them. As an inevitable result, the "map" of the paranormal that these investigators drew up and which guided their research was the map they inherited from the popular movements of mesmerism and spiritualism, rather than one drawn up bit by bit from the results of their own researches.

With the SPR and similar bodies, in short, the problem was not the lack of a reference group, a body of sympathetic but critical discussants. The problem was that the reference group was not sufficiently cohesive to provide a consistent direction and common focus for research. The explicit intent was rather to provide a forum for the investigation of all claimed paranormal events in a scientific manner. This aim was at times reasonably well fulfilled, but at the cost of a great dispersion of attention and, consequently, of the inability to bring the results together in a way that could permit the continuous,

cumulative development of the field. For this reason, the investigations of the SPR and similar bodies, while sometimes seeming to be individually impeccable, never coalesced into a firm and continuous scientific movement in which data, theory, and method could all dovetail and support one another. Kuhn's remark (1962) on preparadigm science seems particularly applicable to psychical research of this period: "Though the field's practitioners were scientists, the net results of their activity was something less than science" (p. 13). They tried to do too much, to conquer all worlds at once. Viable scientific movements, however, do not conquer all worlds at once, but more modestly, only one at a time.

The personal consequences for the individuals in this later period were not nearly so severe as in the earlier one. They did not face so much pressure to conform to the views of either the enthusiasts or the conservatives, since they had a reasonably high status reference group to bolster their identity as dispassionate scientific investigators. The SRR's policy of recruiting famous men to act as president, whether they had made any major contribution to the field or not, served that body well in this regard. But while the individuals were able to maintain a relatively secure identity as scientific researchers, the lack of a common direction for their research prevented them from making that research a genuinely cooperative endeavor. They remained individuals, doing much research in the field, but essentially as amateurs, going off in a variety of directions determined for each of them by their personal predilections and their professional and educational backgrounds.

Psychical research in the United States displayed this same lack of integration up to the early 1930s. There was a variety of studies on mental and physical phenomena in mediumship, on tests of telepathy and clairvoyance in university students and others, on mind-reading horses and other trick animals, and more. Some of these were done well and some poorly, but they all remained separate and almost unrelated studies. In a nutshell, we can say that if the isolated scientific investigators in the earlier years of the nineteenth century had no one to talk to, the loose community of investigators in the later years were not quite sure what they wanted to talk about.

What was necessary for the field to achieve any scientific coherence was for the investigators to abandon the amateur pattern of studying any and all interesting paranormal phenomena. They needed instead to concentrate more modestly on the most workable, rather than the most interesting, of current problems. Parapsychology would necessarily lose some of its lay appeal and gain something in professional-

ism as a result. Future developments in the field would have to depend on the outcomes of these first intensive studies; the topics for future research would have to be those suggested by those outcomes, rather than by the previously existing body of parapsychological questions. Enough investigators would have to agree on these shared priorities to form a relatively cohesive group that could by example define the direction of progress in the field.

II. Community Formation in Parapsychology

This was the situation in the field of psychical research when J. B. Rhine began his activity in it. It was only with the early—but not the earliest—work of Rhine that psychical research, redefined as parapsychology, began to acquire the unity of outlook necessary for any kind of cumulative development. This is what is meant in saying that Rhine established a distinct discipline of scientific parapsychology. He was the nucleus of what became a reference group of professional parapsychologists, ones who agreed not only on the application of scientific method in general, but also in detail on the choice of procedures, problems, standards, language, and audience. Through his influence, workers in the field came to share priorities and techniques, as well as a commitment to the field as a whole.

There were many factors that entered into Rhine's having such an influence. His early studies of extrasensory perception, published in the monograph of that title in 1934, used sophisticated and rigorous, but simple and easily copied, experimental methods (Rhine, J. B., 1934/1973). His data analysis emphasized the objective criteria of statistical significance, rather than subjective ones of similarity or personal meaningfulness. Working at Duke University, with the full support of the professor of the psychology department and the president of the university, he had a strong university backing. And of course, in that first major set of experiments, he had some dramatically successful results to report. All of these factors undoubtedly helped Rhine, through that book, to have a major influence.

But these were not sufficient. They were, after all, not new. If all philosophical ideas can be found in the ancient Greeks, as someone has said, likewise all parapsychological ideas can be found in the publications of the SPR. The use of careful, controlled experimental methods, card-guessing as a technique for investigating telepathy and clairvoyance, statistical analysis of the data, and impressively significant results are all to be found in the SPR *Proceedings* in the fifty years prior to the publication of Rhine's *Extra-Sensory Perception*. Some of the

earlier researchers also had secure university positions, and while these were not solely in parapsychology, neither was Rhine's until long after publication of his monograph in 1934.

Instead, what was crucial to Rhine's influence was something that is easy to describe but more difficult to put into practice. It was his restriction of attention to a small subset of paranormal phenomena and his commitment to making an extensive investigation of them. Those problems—the experimental study of telepathy and clairvoyance, soon combined as ESP—and the specific approach taken to working on them, were not chosen adventitiously or randomly. They were isolated as the most readily interpretable and operationally specifiable of paranormal phenomena. Again, it was not the development of the specific methods used in the Duke Laboratory that gained it preëminence in the field, but the persistence of Rhine and a few collaborators in using them, the making of a long series of closely linked studies with them that could serve as an example to others in the field. This restriction of attention to a small set of related problems and methods, and the persistence in concentrating on them, made the research that each person was doing in that restricted area able to be related to the research that everybody else in the area was doing. The common focus on a small number of related issues forced a degree of cohesiveness in the small group at Duke that had been notably lacking in the larger psychical research communities. As a result, the research that Rhine initiated at Duke gradually acquired a systematic status that attracted others to replicate and extend it.

It did not happen overnight. Popular acclaim followed quickly after publication of *Extra-Sensory Perception* in 1934, but professional acceptance was slower. Replication was neither easy nor guaranteed, and the existing psychical research societies were naturally inclined to see Rhine's experimental work as an interesting but narrow sideline to their main concerns. But within five to ten years of the publication of *Extra-Sensory Perception,* its influence was being fully felt. The *Journal of Parapsychology* was established in 1937 to provide a vehicle for Rhine's kind of behavioral studies of ESP, and the SPR and American SPR had, by the early 1940s, come to emphasize the same kind of experimental approach in their own publications. Throughout the 1940s and later, the parapsychological journals acted as professional organs devoted to the kind of interrelated, restricted behavioral studies of the kind Rhine had emphasized. It was due to this kind of influence that McVaugh and Mauskopf (1976) rightly judged Rhine's monograph of 1934 to be a paradigmatic work for parapsychology.

If this paper were concerned solely with the history of parapsy-

chology as a scientific specialty, it could stop here, having pointed out the systematic influence that Rhine's work had on the development of the field and the different nature of his contribution from that of earlier investigators who attempted to make a scientific study of paranormal phenomena. But even paradigms can have a personal history, and in a paper that proposes to discuss J. B. Rhine as well as the specialty he established, it is appropriate to consider how Rhine was able to have the influence he did. His own early professional history is in any case a fascinating case study in the development of research methods.

III. Personal and Professional Factors in the Influence of J. B. Rhine

J. B. and Louisa E. Rhine first committed themselves to parapsychology, or psychical research, in 1926.[3] They had recent PhD degrees in botany from the University of Chicago and an aggressive confidence in the power of scientific method. They also, however, had religious-cum-metaphysical doubts about the place of human beings in the universe, the implications of reductionist biology, and the existence of the soul. They hoped to resolve these doubts by scientific studies of phenomena that, on the surface, seemed to challenge the materialism they had been steeped in at Chicago. To turn their backs on their professional training and forsake their careers for such a cause was a bold step. It was a comprehensible one, however. It was the same kind of step, though more extreme, as the early workers in the SPR had taken, and was taken for the same kind of reasons. Like those earlier workers, the Rhines entered the field as dedicated amateurs. There was, after all, no other way to enter it.

The Rhines' first-hand experience in the field began with an informal study of Mrs. Mina Crandon, a renowned Boston medium known professionally as "Margery," whose séances were widely acclaimed in the ASPR and elsewhere as positive proof of survival. This choice also was not surprising. The Rhines' initial interest in psychical research had been excited by Oliver Lodge and Arthur Conan Doyle, both of them champions of different varieties of spiritualism. Furthermore, the Rhines began their active involvement in the field as protégés, in a minor way, of the spiritualistically inclined leadership of the ASPR (Mauskopf & McVaugh, 1980).

[3] All biographical details, except where otherwise noted, are from Rhine & Rhine (1978).

Indeed, the only surprising thing about the Rhines' investigation of Margery was its results. They "came to Boston," as they wrote, "with a favorable notion of the case already formed" (Rhine, J. B., & Rhine, L. E., 1927, p. 401). Nevertheless, while witnessing phenomena that, they were told, had been shown to satisfied sitters scores of times, they were appalled to find widespread trickery in the séance room. Organizing their observations systematically, they found six "conditions which permitted fraud and which were not necessary for genuine mediumship," nine "inconsistencies which look suspicious and which fraud alone will explain satisfactorily," and four pieces of "positive evidence of fraudulent action" (Rhine, J. B., & Rhine, L.E., 1927, pp. 406, 409, 412). Their report was published in the *Journal of Abnormal and Social Psychology* in January 1927, and the Rhines ended their cordial relationship with the ASPR leadership, which was still actively promoting Margery. Interestingly, while the ASPR officers replied heatedly in their *Journal* to critiques of Margery by Dingwall, Hoagland, Houdini, and others of the time (e.g., Bird, 1926), they never responded to the Rhines' report.

The Rhines' exposé of Margery exemplified the strengths they brought to psychical research. Those included keen powers of analytical observation, moderate skepticism, and hard-headed common sense, sharpened by their scientific training in the no-nonsense fields of botany and plant physiology. These were sufficient to permit them to see through Margery because her activities were all in the range of ordinary human action. The question to be addressed in studying her was not exactly a scientific question requiring specialized scientific knowledge in psychology, any more than in botany. It was instead a kind of judicial question, a question of truth or falsity, bona fides vs. fakery, requiring the testimony of competent witnesses. This the Rhines were able to provide in good measure. There were psychological questions involved, too, regarding Margery's motivations and thought processes, but these were not the Rhines' concern.

In their next piece of research, however, the Rhines displayed the limitations that went along with their strengths. When the question at issue required the sophisticated application of scientific knowledge in a field outside their own, they were, inevitably, not able fully to resolve it. They showed this in their study of Lady, a mind-reading horse (Rhine, J. B., & Rhine, L. E., 1929a). They were attracted to the study of telepathy in this animal for a number of reasons. The main one was that telepathy had always been the counterhypothesis to survival in accounting for the information delivered by successful mediums, and it was clear that it needed study for its own sake. A secondary reason

was that telepathic animals, unlike humans, would be unlikely to try to commit fraud. In addition, J. B. Rhine (1925) had recently reviewed Bechterev's experiments with telepathic dogs and was very impressed by them; the procedures that the Rhines used for studying Lady were very largely based on Bechterev's.

Lady made her guesses by touching her nose to alphabet or number blocks to answer questions addressed to her mentally or verbally. The main question in assessing her performance was not one of fraud, although fraud on the part of the owner/trainer was considered. It was rather one of eliminating the counterhypothesis that she was guided by minute movements indicative of expectant attention made by her owner or by the questioners. A great deal was known on the subject from the research of Pfungst (1911/1965) on the horse known as "Clever Hans," and from other sources.

It has sometimes been alleged that the Rhines were quite ignorant of Pfungst's research and similar studies and were therefore easily taken in and tricked by Lady's owner. This seems quite untrue. They were well read in the literature, cited Pfungst's research as well as others, and introduced what seemed to be adequate controls on movements of the observers, including sending Lady's owner out of the tent where the performances were taking place. Lady's performance on trials with her owner absent was significantly better than chance. Indeed, from the standpoint of the critical common sense that had served them so well with Margery, the controls were adequate. It was only from the standpoint of the psychological methodology and theory of the day that the controls were clearly insufficient.

The Rhines' rationale for their controls was that "the theory of unconscious guidance . . . assumes involuntary gestures, but the same psychologic assumption must grant also voluntary control over them" (Rhine, J. B., & Rhine, L. E., 1929a, p. 462). They therefore usually had the owner and themselves sit motionless and avoid eye movements while Lady was making her choices of blocks, and found that her success rate remained high under these conditions. This procedure was inadequate, however. Pfungst had found that the questioners could *not* refrain from making minute guiding movements. After prolonged self-training, he could take the part of the horse and respond to mental questions even when the questioners were intent to avoid giving any cues (Pfungst, 1911/1965, Ch. 4). But Pfungst did not dwell at length on the impossibility of controlling such movements. He did not need to. His psychological readers in the early part of the century would have shared with him a familiarity with and

general acceptance of what was called a motoric theory of consciousness. The experimental demonstration that, in a particular case, conscious expectations would express themselves in minute motor acts would therefore have been immediately understood. It was merely the successful application of an established theory to a new instance.

Lacking that theoretical and methodological background, the Rhines relied on common sense in controlling for signals, and thus failed to do all that was necessary. To be sure, there were practical difficulties. They had to keep on good terms with Lady's rather temperamental owner and were thus limited in the controls they could impose. Nevertheless, the methodologically appropriate controls would have been procedurally simpler and less intrusive than the most rigorous one that they did successfully impose, that of sending Lady's owner out of the tent. That control eliminated fraud as the sole explanation. To eliminate the "Clever Hans" effect, however, it should instead have been J. B. Rhine who went out of the tent, on some of the twenty-one trials when he alone knew the target. (Of the visitors, it was usually J. B. Rhine who mentally "controlled" the horse; the other members of his party, including Louisa Rhine, William McDougall, and John Thomas, were less successful.) If Rhine's complete removal was impractical, he could have approximated it by blocking his view of the horse with a screen, or even by closing his eyes while she was choosing the target. There were a number of tests in which the *horse's* view of all those who knew the target was more or less restricted: by hats pulled down over their faces, by a small screen, and by a larger screen. Her performance deteriorated as the extent of visual blockage increased. The relevant control, however, was to prevent the *questioners,* those who knew the target on a given trial, from seeing *her.* It was their uncontrollable movements, indicative of expectant attention, that would provide the signalling function—according to the motoric theory of consciousness as applied to the "Clever Hans" phenomenon. Control of the horse was secondary. However, lacking the necessary technical background, the Rhines did not appreciate that fact; and so they addressed their controls instead to the announced or suspected performers, the horse and her owner.

It remained, therefore, not certain but highly possible, that Lady was responding to small motor movements despite the Rhines' best efforts to eliminate them. Their apparently cautious conclusion, that "no other hypothesis [than telepathy] . . . seems tenable in view of the results" (Rhine, J. B., & Rhine, L. E., 1929a, p. 463) was incorrect as a

result. The counterhypothesis was not ruled out. When Lady's sensitivity, from whatever source, declined some months later so that she required obvious signals in order to perform, they could only report regretfully that her abilities had vanished (Rhine, J. B., & Rhine, L. E., 1929b).

The intent of this discussion is not simply to find fault with this fifty-two-year-old study. It is more serious than that. The difference between the Margery and the Lady studies shows clearly the difference between fact-finding in a structured social context, however bizarre, and testing a scientific hypothesis. For the former, an intelligent and critical application of the observational procedures of daily life is often sufficient. The question, again, is a kind of judicial one. Scientific training may be helpful, especially if it is not related to the matter being investigated, but it is not essential. For the latter, however, a precise technical methodology, appropriate to the hypothesis and the specific problem situation, is essential. It does not have to be procedurally complex, as we have seen; but it does have to take close account of the relevant knowledge and theories already existing in the field. Judicial procedures, based on the codification of common sense, are inadequate.

Had the Rhines followed the lead of many earlier psychical researchers at this point, then, disappointed with Lady, they might have gone on to look for other diverse instances of inexplicable behavior and reported them hopefully as demonstrations of a new force. Had they followed the lead of Pfungst and other psychologists in his tradition, they might have studied human and animal psychology deeply enough to become experts in the interpretation of performing animals such as Lady and Clever Hans. Instead, they did something quite different, avoiding both the dilettantism that threatened seekers of the unexplained and the narrowness that would have resulted from concentrating on a technical area only tangentially relevant to their own goals. What they did was to develop the needed technical proficiency, from the ground up, in their own subject area. They began, that is, to construct the technical methodology and conceptual framework for a science that did not yet exist.

The careers of J. B. and Louisa E. Rhine began to diverge at this point. Louisa Rhine took mainly a supportive role in developing the basic methodology of parapsychology and later began the mammoth and optimistic task of analyzing the distribution and patterns of spontaneous cases, a task which is still in progress (e.g., Rhine, L. E., 1949, 1981). The task was a mammoth one because of the volume of material. It was an optimistic one because it laid the groundwork for a

natural history of psi, a groundwork that could be built upon only after the experimental studies had provided firm guidelines for making judgments on the presence of psi in the spontaneous cases. In the meantime, however—and this was the original basis for undertaking the study—it could usefully serve as a source of research hypotheses.

J. B. Rhine took the lead in developing workable methods. Although he was originally sent to Duke with private funding to analyze some mediumistic records, his interests even before the Margery affair had tended to focus on studies of telepathy and clairvoyance as being more easily interpretable. As soon as he could, therefore, he began looking for appropriate techniques for the experimental study of these. Knowing what he was after, he was able implicitly to follow the simple rule: keep trying until you find something that works, and then stick with it. A number of more-or-less successful experiments of the time involved the supposedly telepathic transmission of playing cards or pictures. So, in the summer of 1930, Rhine tested groups of children in summer camp, having them guess the number from 0 to 9 printed on a card concealed in his hand. There were no interesting results. In the fall of 1930, Helge Lundholm, a new member of the Duke psychology department, suggested that he hypnotize students to test their telepathic ability in the hypnotic state. Lundholm and Rhine tested thirty students in this way, with no results. Also in the fall of 1930, another member of the psychology department, Karl Zener, suggested that they print numbers or letters on cards, seal them in envelopes, and give them to students to guess. No results followed—except for the discovery of one high-scoring subject who was retained for later study (Rhine, J. B., 1934/1973). Rhine then asked Zener to design cards with more distinctive symbols than ordinary numbers or letters (Mauskopf & McVaugh, 1980). Zener, whose field was the psychology of perception, accordingly designed the ESP cards that for a time, and to his distaste, bore his name.

With these they began to get results. Tests involving over 800 trials by unselected undergraduates in the winter of 1930–1931 yielded results significant at well beyond the .001 level. The one high-scoring subject discovered previously did even better with the new cards. Throughout 1931 and into 1932, Rhine worked out the techniques used with these cards in collaboration with other members of the faculty, students, and friends. They developed the "Down-Through" and "Before-Touching" techniques and variants on these, made the operational distinction between clairvoyance and clairvoyance-plus-telepathy, became surer of their use of the probability calculus,

identified a number of additional high-scoring subjects, and began to try to identify the psychological correlates of successful performance (Rhine, J. B., 1934/1973). These early studies culminated in Rhine's first paper on ESP in 1932, which he delayed publishing for two years, however, to make sure that he was not taking another false step (Rhine, J. B., 1934).

In these early and exploratory studies, Rhine displayed, individually and with a few collaborators, the same pattern of persistence and systematic restriction of attention that marked the Duke group in the years after 1934 when the work started to become known. The genesis of that group cohesiveness that enabled parapsychology to become a scientific specialty was evident from the beginning of Rhine's own experimental work at Duke, before there was much of a group to be cohesive or not.

IV. Conclusion

The personal and professional factors involved in J. B. Rhine's major contribution to parapsychology—establishing it as a coherent scientific specialty—were thus those that enabled him to serve as the nucleus of an intensive and restricted research community. These of course included experimental skills, a commitment to the importance and legitimacy of the field, confidence in the applicability of scientific methods to its problems, a favorable institutional setting, and—an important factor that has not been dealt with here—a forceful personality that inspired enthusiasm and commitment in many of his co-workers. These were all necessary, but were neither unprecedented in the field nor sufficient for the purpose. The additional crucial factor was a combination of flexibility and dogged persistence. It was a willingness to try a variety of approaches to investigating the field, followed by an unprecedented persistence and narrowing of focus once a workable approach had been identified. The first part led Rhine to hop from mediums to trick horses to card-guessing, and from one technique to another in the study of card-guessing. The second led him to stop once he had found something that worked, to devote more energy—both his own and that of the researchers working under his leadership—to studies of a highly restricted topic than had ever before been expended on any experimental topic in the field. The initial flexibility was necessary at first to prevent premature closure before a successful approach had been identified. Once it was identified, however, it was the subsequent persistence that paid off. Additional research topics and extensions of the original ones would

be introduced slowly and cautiously, and would be based as far as possible on the work already done.

In 1977, the author asked J. B. Rhine how he accounted for his early success in parapsychology. How, in his view, had he been so much more successful than others in building up a systematic body of evidence? Rhine replied that he had always been fairly confident that if there was anything to be found, he would have a good chance of finding it. It was not because of any special brilliance or gifts on his part, he emphasized, that he had had this confidence. It was rather because he had the doggedness and determination to push on with the methods of science until they supplied the answers to his questions, one way or another.

These comments may seem to reflect only a becoming modesty in an elder statesman in the field. To a considerable extent, however, they appear to be justified. Whatever "special brilliance or gifts" J. B. Rhine might have possessed, it was his doggedness and determination, supplemented by his eye for the selection of workable problems, that largely transformed psychical research into experimental parapsychology. It is in this sense that the establishment of parapsychology's scientific status, grudging though that status often still is, has an intensely personal history. That history, more than in most sciences, is the history of the work and the influence of one individual. That work, and what it led to, made parapsychology into a scientific discipline and, along with the numerous technical and professional contributions he went on to make in the field, assured J. B. Rhine a key place in the history of parapsychology.

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