



JOSEPH BANKS RHINE  
1895-1980

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## INTRODUCTION

The passing away of Dr. Joseph Banks Rhine on February 20, 1980, marks the end of an era which began with the publication of his *Extra-Sensory Perception* in 1934—a book “of such a scope and of such promise as to revolutionize psychical research and to make its title literally a household phrase.”<sup>1</sup> For almost one-half a century J. B. R. was the undisputed leader of the field in determining its course. He gave it its concepts and methods, defined its scope, mapped out its territory, and provided the instrumentalities necessary for its professionalization—including the establishment of the *Journal* and the formation of the Parapsychological Association. His admirers as well as his adversaries agree that parapsychology is what it is today largely because of him. It is difficult to find a parallel situation in the development of any other science. Therefore, it will not be an easy task for anyone to foresee the future course this discipline will take without J. B. R. around.

The Foundation for Research on the Nature of Man and Duke's Psychology Department, where J. B. R. was Professor Emeritus, jointly sponsored a conference on November 28, 1980. Held in the Zener Auditorium of Duke University, the conference was titled “On the Frontiers of Science: The Life and Work of J. B. Rhine.” From that collection of tributes, reviews, and professional evaluation of his work the editors have chosen a half-dozen representative articles to be printed here in the *Journal*.

There was much more in the proceedings of the conference which should be shared with others, and with that in mind there are plans for McFarland & Co., Inc., to publish a book, to include the six articles presented here, together with the following: “J. B. Rhine and Post Mortem Survival: A Reappraisal and Vindication” by C. T. K. Chari; “J. B. Rhine and American Psychology” by Irvin L. Child; “Dr. J. B.

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<sup>1</sup> Seymour H. Mauskopf and Michael R. McVaugh, *The Elusive Science*. Baltimore: Johns Hopkins University Press, 1980. P. 101.

Rhine and Philosophy" by Frederick C. Dommeyer; "J. B. as a Family Man" by Sara R. Feather; "Review of Research Findings: II. Psychokinesis" by Charles Honorton; "J. B. Rhine and the History of Ideas" by Brian Inglis; "J. B. Rhine and European Parapsychology" by Martin Johnson; "Psi-missing and Other Parapsychological Effects" by H. Kanthamani; "Rhine's Impact on Philosophy" by Arthur Koestler; "Extrasensory Perception: A Paradigmatic Work for Parapsychology" by Seymour H. Mauskopf and Michael R. McVaugh; "Dr. J. B. Rhine: Teacher and Friend" by Elizabeth A. McMahan; "J. B. Rhine and His Critics" by K. Ramakrishna Rao; "J. B. Rhine and Pseudoscience" by Marcello Truzzi. The book will also include a comprehensive bibliography of Dr. Rhine's writings, compiled by F. David.

As the proceedings of the conference aptly illustrate, the work of J. B. Rhine provides a solid foundation for the science of psi. Yet, as he was well aware, his work was far from complete. His last words were: "We must go on." In some of his unpublished notes he briefly indicated what he considered should be the main thrust: "The major challenge that stands out increasingly in parapsychology research today," he wrote, "is the great elusiveness of this psi, or psychic, ability. It has functions wide and lawful enough to allow a minimal grasp and scientific verification. Yet it evades most of the controlled application all the known sensorimotor abilities permit. Every researcher in the field is keenly challenged to bring the ability under easier control and repeatable demonstration."

Though he pursued psi research with total devotion, parapsychology for J. B. R. was not an end in itself. It was the implications of the existence of psi that fascinated him most. "Like many of the founders of parapsychology," he once said, "I am searching for light on man's nature with respect to the physical order. I had found it hard to hold on to a religious view that rested on the supernatural." The science of parapsychology, he hoped, would answer questions about "man's transcendent nature."

As we dedicate this issue of the *Journal* to his memory, we believe that the light he lit will continue to glow and make it possible for us to see and know those nonphysical aspects in us that have hitherto baffled our understanding and continually challenged J. B. R.

*The Editors*

## J. B. RHINE: MAN AND SCIENTIST

By LOUISA E. RHINE

A few introductory remarks under today's topic seem especially in order in connection with the personality of J. B. Rhine and his scientific work. I think that to an extraordinary degree some of his particular personality characteristics were responsible for the specific work he accomplished. I want to mention a few of these.

These particular characteristics, of course, were both hereditary and environmental. Heredity is a complex subject and I will not enlarge on it here except to mention one characteristic which I think all those who knew him will recognize and agree was inborn. That was a serious and one-track cast of mind, which meant that he was not much interested in trivialities and that to topics that interested him he gave full and consistent attention.

Not so generally known, however, are some aspects of his early environment, which it would appear left their marks on his personality. One of the earliest of these is recorded in an autobiographical sketch he once wrote for a college class. In it he tells that he was born in a log cabin in the mountains of southern Pennsylvania and that there were no near neighbors, so that for the first five years of his life he had no playmates except his sister, two years older than he. As a consequence, he was, he says, "as shy as a wild turkey," and so, instead of people, he early felt at home with the great out-of-doors. He writes, "I learned to love the big blue mountains, the rich dark woods, and the great brown hills. Even to this day," he continues, "I feel that I belong more to them than to the crowded cities." Again, I think all those who came to know him personally can readily see that the twig so bent foreshadowed the man he became.

It was not only the love of nature and the out-of-doors that was a life-long characteristic of J. B. Rhine. Another was what I might call his aloneness: a marked and definite independence of mind, a freedom from, and even an unawareness of, social pressures, especially in regard to the opinions of others. He felt no need to be influenced by them unless, after consideration, he could approve. And so, any ideas, opinions, or conclusions he held were the result of his own careful and logical scrutiny. They were not copied from anyone else and were as likely as not to run counter to those of others. The result was that he was inherently a skeptic, not a follower, and his

opinions were not casually changed or easily abandoned. They were modified only by new evidence.

Another circumstance of his childhood and boyhood should be mentioned, even in a brief sketch like this, for it was one that it would seem must have helped mould the personality that could sustain a decades-long struggle to pursue unchanged the direction of his interest however unpopular and controversial it might be. This was the fact that because of his father's unrest and unending search for a better opportunity, J. B.'s family moved eleven times, he says, during his school years before high school. This meant that repeatedly, nearly every year in fact, he was the "new boy" on the school playground. Because of what seems to be an unwritten law of juvenile male behavior in such situations, a new boy must fight his way to social acceptance by the crowd. And this is what this perennial new boy had to do. As those school years passed, he had to do it not only for himself but, as they came along, for his younger brothers too. As he says, to pick on them was his own prerogative and he shared it with no others.

As one can see now, along with the need to fight when challenged, went the corollary that you don't give up when attacked. Instead you stand your ground and battle through to peace and eventual acceptance. He may not have been aware of learning this lesson, but it must have been stamped indelibly into his unconscious.

These personality characteristics, of course, affected also his choice of topic for research. That topic, briefly stated, began as the question: Does the human spirit survive the death of the body? Eventually it took the more general form: Does human nature include any aspect other than the physical? As you all may know, his interest in what came to be the problem can be traced back to adolescence when he came to feel deeply the dominant religious attitudes of his community. With the intensity and single-mindedness of his nature, that religious response colored his outlook and directed his aspirations until his college days. As he characterized it, that period was his "age of feeling." Later, in college, came the "age of reason," when he began to look critically at the religious beliefs he had held since boyhood. This led to complete disillusionment, which he took with all the seriousness of his nature. It resulted in a thorough-going mechanistic outlook, an outlook that persisted for a period of years, including several in the Marine Corps.

But when, the war over, he returned to college and heard of the field of psychical research, it rearoused in him the old interest in the question of survival. Entirely skeptical as he was, he nevertheless had

to find out what the people in these societies for psychical research reported. He found, of course, that the question of survival was still open, although some evidence supporting the possibility was reported. But the main method of inquiry had depended on the use of mediums, and that, to him, did not seem good enough to support so weighty a conclusion as that the deceased survive and can communicate with the living.

Since by then he was in his graduate-school days and deep into the use of strict objective scientific method, he wondered what that method might produce if applied to the great question of survival. With his mechanistic outlook challenged by some of the results reported by the SPR, his very nature and background required that he look for the answer without any question about the length of time it might take. This is something of J. B.'s background when, in 1926, college days and a few years of teaching over, he decided to turn full time to his basic interest.

We all know of the quandary many young persons, and men especially, face when they come to the crossroads of career selection. In J. B.'s case any such uncertainty vanished once he realized that the question of man's total nature could be attacked by the same method used universally in the study of man's physical nature.

It was after this that the problem, first phrased as that of "survival," became more broadly thought of as the question whether human nature includes any aspect beyond the physical. Once J. B. was launched on that investigation—and given the characteristics of his nature and background—he never even momentarily, I am sure, considered for himself any other field of endeavor.

However, I can think of at least two possible deterrents that, given a different kind of personality, might have diverted or deterred him, either at the beginning or somewhere along the way. One of them he was aware of then; the other, certainly not. The one of which he was aware was simply a matter of economics. In those graduate-school days, soon after he first heard of psychical research, he had tried to find a way by which he could turn from plant chemistry to the investigation of psychic phenomena. On June 27, 1923, he wrote three letters of inquiry for scholarships or positions in which he might get support while training to become an investigator in the area of psychical research. In each letter he made it clear that he would leave the field in which he was about to get a Masters degree, with the PhD to the other if he could find support.

The first letter went to Frederick K. Edwards, President of the ASPR in New York. The answer was encouraging as to the need for

workers with an educational background like J. B.'s, but no financial aid was available. He advised J. B. to read up on his own, and he gave advice as to what to read.

The second letter went to Dr. William McDougall, Head of the Psychology Department at Harvard, asking if a fellowship was available by which J. B. could maintain himself if he left plant chemistry for full-time training in psychical research. The answer was that only one such fellowship had been established and it was held by Gardner Murphy. There was some money at Stanford University, but it did not seem to be available.

The third letter was addressed to Prof. Joseph Jastrow, Psychology Department, University of Wisconsin. The reply was a recommendation against J. B.'s leaving his present field for psychical research, which, Jastrow said, was "largely concerned with the elucidation of error, partly with the explanation of obscure phenomena." He said, "Positions are few and often the critical attitude is the very obstacle."

J. B., therefore, was fully aware that psychic research offered no monetary return. The fact that it was an unpopular field was very clear, and even Dr. Jastrow's emphasis on the critical attitude with which it was often met, could have been a deterrent. All that, however, did not count against the importance of the topic for humanity as well as for J. B. himself. As in the past, he assumed that such considerations as the financial one would take care of themselves incidentally, once the major objective was undertaken.

The other possible deterrent, as I said, was one which he did not then suspect: that to any positive results that might be secured in the investigation, the general reaction of scientists—some psychologists especially—would be as strongly negative as it turned out to be. This, of course, he did not know; nor, I think, did he ever imagine it until after the publication of his monograph, *Extra-Sensory Perception*, when it fell on him over succeeding years like unending tons of bricks. In his preface to that book he says, in a tone that anyone launching a new idea might use:

It is to be expected, I suppose, that these experiments will meet with a considerable measure of incredulity and perhaps even hostility from those who presume to know, even without experiment, that such things as they indicate simply cannot be.

And so, he was prepared for "a considerable measure of incredulity and perhaps even hostility," but not for the viciousness or durability of it. He did not know then or even faintly foresee that, even decades later, what he called in his preface "the inevitable reactionary re-

sponse to all things new and strange" would be resounding against his investigations just about as stridently as ever; nor could he have guessed that his last effort, even in old age and failing health, would have to be expended in defense of even his own basic honesty. No, he certainly did not foresee that. Instead, he thought already in 1934 when the monograph was published, that he would see signs of decline in this reaction when, as he said, the world turns "a scientific attitude toward the new facts." But he did not foresee then that that time—if it ever does come—would not come during his lifetime. And, without that dogged, unquestioning devotion to the solution of the problem—which he thought of as benefitting humanity as well as himself—he might have been discouraged and diverted somewhere along the way. But to a personality combination of hereditary single-mindedness and early training in meeting adversity head-on, such turning back or away was unthinkable. I'm sure it would have been so, if he could have known it, even at the start.

But he did not know then, either, that those same characteristics that kept him from being diverted when the obstacles were greater than he had foreseen, could also account for his success in eliciting from his subjects, even in the unpractised beginning of his research, the evidence that would lead to the answer to his basic question.

As we all know now, but no one knew then, psi ability is particularly fragile and elusive in its expression and dependence on the psychological "atmosphere." In the list of well-known human abilities, I think of it as somewhat like humor, which also depends on certain not easily defined conditions. Few people can fulfill, on demand, the request, "Now be funny. See if you can make me laugh."

As we all know, too, in the attempt to discover and understand almost any unknown, the first attempts to unravel the secret often seem by hindsight to have been clumsy, if not actually counterproductive. But in J. B.'s attempt to unravel this secret, it did not turn out that way, for the very seriousness and intensity with which he asked the question of his student subjects affected them with something like contagion so that they took the task he set for them in the same way. Neither teacher nor student knew that such an attitude was necessary; but once they caught the spirit and became involved, the proper attitude resulted and together they got the answer, or what was then at least the promise of an answer. And as it seems now, they got it because the spirit which they brought to it supplied what one could call the "yeast," the ferment necessary in this recipe for a psi experiment.

Later, when other experimenters often failed to duplicate the



results, even though they followed carefully all the points of technique J. B. had described, they were baffled; and for a time and to an extent, J. B. was too. But the fact was that in this recipe the "yeast," the necessary spirit, was not mentioned. I went back then to the account of the tests reported in that first book and found not the slightest mention of it.

Possibly the atmosphere in which these early subjects worked was all the more effective because they and the experimenter too were unaware of this ingredient. But as it appears now, it had been unconsciously created by the nature of the experimenter and the combination of factors that defined his personality. Together they created the yeast that was such an important ingredient of the test.

And so, as I said, I think it was certain particular characteristics of J. B.'s personality that made him able to accomplish his particular scientific work, which then became the basis on which many others have carried on the investigation. This was true in the beginning when an important condition for a successful psi test was unknown and he unwittingly supplied it. It was true also over the years when obstacles greater than he had ever contemplated confronted him. Because of those characteristics, he never dreamed of turning aside. Just as when, as the new boy on the playground, he could not give up or dodge the struggle, in this larger arena too, he could only face it and go on. And as a consequence, in his last days, I know he took a large measure of satisfaction in believing, as he did, that psi ability in the human personality is now established. He knew it was the answer to his question.

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## RHINE'S CONTRIBUTIONS TO EXPERIMENTAL METHODOLOGY AND STANDARDS OF RESEARCH

By GERTRUDE R. SCHMEIDLER

When Dr. Rao invited me to join in this day of tributes I felt grateful for the opportunity to express some of my great respect for Dr. Rhine—and was well pleased at the topic assigned me. It will be a pleasure to review both Rhine's contributions to experimental methodology and his specific innovative methods, and show how he continually maintained the highest possible research standards. His contributions were so basic that they set the research patterns for many of us. They have set my own for almost forty years. It does not seem too much to say that he both founded parapsychology and determined its direction.

Psychical research had existed before, of course, but an informal paper by LeShan and Margenau<sup>1</sup> argues persuasively that psychical research deserves to be considered a different field. Its methods are more varied than parapsychology's, and its topics cover a wider range. Its goals overlap with parapsychology's but its subgoals are different. It tries to evaluate and integrate even weak findings relevant to its problems, but parapsychology tries to study only topics where rigorous controls can be employed. If we agree with this argument, we can add that parapsychology emerged as a distinctive area because of Rhine's methods, his spectacular results, his theoretical approach (which developed the methods into a methodology), as well as his gift for eye-catching nomenclature and his skills in public relations and organization.

In thinking about his work, I keep remembering and wanting to paraphrase a quotation from the Roman emperor Octavius, who boasted that he found Rome a city of brick and left it a city of marble. Dr. Rhine was not a boastful man, but we can say for him what he did not say for himself: that when he began his life work he found the study of psychical research, and he left for us the science of parapsychology.

How did this come about? To trace it, let us put ourselves in

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<sup>1</sup> Private communication.

imagination back in the 1920s. Suppose you were trained as a scientist then, and wanted to do research. (I am talking about J. B. Rhine, of course, but am asking you to identify with him and his decisions.) You wanted it to be good research, careful and definitive; all research workers do. But beyond that, what you really hoped was that it would be meaningful, would help toward resolving important issues. One such issue is the nature of life. Is life only inanimate matter in some particular patterns, or is it something more? And other issues follow from this one, dealing with our place in the universe. Are we only corporeal, or do our spirits survive bodily death?

Your own area of specialization was the physiology of plant cells. This is relevant to what you want to learn, because the basic processes of plant cells surely relate to the nature of all life and thus indirectly will bear on the other larger issues that concern all thoughtful human beings. But is it relevant enough? Research on plant physiology demands concentrating on its own specialized technical problems. Is each such technical problem only a short detour that will soon lead back toward the important answers? Or does the whole topic relate to the larger issues so indirectly, so remotely, that its findings will not bear on them in the foreseeable future?

It must surely have been tempting for both J. B. and Louisa Rhine, with their doctorates in biology from the prestigious University of Chicago, to stay with the topics and the methods where they were already expert. But as we all know, they succumbed to the greater temptation of trying to study their most important problems as directly as possible. Their reading of prior work in psychical research made them think that psychical abilities had perhaps already been demonstrated; and this was where they turned. They hoped to check out that "perhaps" and, if they could eliminate it, go forward, to learn more.

Now another question arose. Psychical research was not a single, simple topic; the decision to work in it could not give anyone a specific directive. Rhine categorized its phenomena neatly (Rhine, J. B., 1934/1973) while describing his own research decisions. He listed four possible sources for psychic effects, ranging from those due to "simple corporeal agency" (the example he cited was telepathy) to those due to a simple incorporeal agency such as a spirit. And for each of these four classes he listed a varied set of phenomena to investigate: psychological effects, or physiological, or physical, or pathological. His four-by-four table, with its broad areas within psychical research, left many directions open.

Then where should his own investigation begin? His useful

apprenticeship with Dr. Walter Franklin Prince had trained him in the strict standards set by the best methods of psychical research, but the time was largely spent in helping expose a fraudulent medium. Interesting and worthy though such exposure may be, it is obviously not the route to major scientific advances. Other early work with a psychic animal subject had seemed more promising. Rhine argued to himself—and to his readers, in this first book—that research on any of the 16 subtopics would have ramifications leading to the others. He then put forth the problem for his first major project in these words: "Are there really dependable evasions of psychological laws (as they are regarded today) by corporeal personalities? In other words, can we find persons able to demonstrate the more commonly reported sort of apparent exception to psychological laws—mainly, cognition of events without the usual sensory or rational experience required by our habitual concepts for the knowing act?" (Rhine, J. B., 1934/1973, p. 11).

He decided, in short, to begin by "contributing independent proof of E. S. P. as a primary objective" (p. 15). He would work in a most difficult field, but would start with what seemed the simplest part of it. He would do this by searching for gifted subjects, who could give firm, reliable results. His next questions were: Where would he find these subjects, and what methods should he use to obtain those results?

Let me break away from the narrative account of how his contributions developed, to pick it up later after introducing another theme—a theme which will make it easier for some of you to identify with Rhine, but perhaps harder for others to do so. Because his talks and writings emphasized general issues and theories, we may be slow to recognize that he was basically an empiricist; his thinking was more inductive than deductive. Often, perhaps almost always, his theory was not only suggested by observations, but was then built up and modified by further observations.

Consider, for example, because it is relevant to his next research decision, his emphasis on the need to evaluate data in mathematical terms, the need that led to his developing and advocating a forced-choice method for ESP tests. On what was the need based? On the observation that without such quantitative evaluation, controversy is endless. With a spontaneous case or other qualitatively interesting coincidence, a decision as to whether it was psychic or was due to chance or normal causes demands a subjective weighting of the alternatives. One person's private weighting may lead to one conclusion, but the next person's assessment can lead to the opposite. Again

and again such qualitative judgments have been shown to be unconvincing to others; their appeal rests on the unscientific basis of personal judgment and personal authority. It was these observations about inconclusive controversies that gave him the prescription for his own basic research. It should be susceptible to statistical evaluation, because this is objective, public, and generally accepted.

Now to pick up the chronological account. A first suggestion for a possible source of gifted subjects came from the investigation conducted by the Rhines on a target of opportunity, the horse "Lady." They had found her responses initially showed telepathic success without sensory cues, but that later her telepathic scores declined and she eventually showed dependence on sensory cuing. Perhaps careful work with animals would give the solid base that was needed. Rhine began by trying to find such suitable subjects, but soon learned—the hard way—that gifted infrahuman animals were in short supply in his neighborhood. He therefore turned to testing the humans who were so amply available at Duke and its environs.

How was he to do his testing? He had, as we saw, laid down one rule for himself: there must be quantitative evaluation of whatever results he obtained. This was most readily achieved by forced choices within some specified sample of targets, because when a large sample of such targets was randomly arranged the theoretical odds of chance success were known. He naturally began with the targets which had come down to him from previous investigators: the digits from 0 to 9 and the letters of the alphabet.

But he was too keen an observer to stay with those targets for long. He saw what was not widely recognized then: that stimulus preference for certain of the digits or letters was so strong as to distort the responses. Further, sequence effects were a major source of response bias. After responding with a 2, a person would be all too likely to continue with 4, 6, 8, or with 3, 4, 5; and the tendency to use letters of the alphabet to spell words or parts of words is almost irresistible. Rhine recognized that the target population should consist of items with equal or nearly equal drawing power and that there should be no obvious, generally used way of ordering them.

His colleagues in the Department of Psychology at Duke were encouraged by the Department's chairman, Dr. William McDougall, to offer advice on these problems. One of them, Dr. Karl Zener, was especially helpful. Largely due to Zener's suggestions, a set of target cards was constructed. Each carried one of five symbols: a circle, rectangle, plus, five-pointed star, and set of two wavy lines. After

some early exploration of ways to present them, they were arranged into packs of 25, with five of each symbol in a pack. Out of these Zener cards came, with minor modifications, the modern set of five ESP cards which is so widely known.

This was a major advance in method, and I will cite some of its advantages. One is that in general there is neither a strong stimulus preference among the symbols nor any common tendency among subjects for sequence effects. Another is that a batch of five units is not too large to be retained in primary memory. A third, extremely important in Rhine's approach, is that feedback to the subjects about their success will by chance give an intermediate level approximating one positive reinforcement in five trials. This provides enough encouragement to keep the subjects hopeful, and it provides enough failures to make the task stay challenging. A fourth is that the series of 25 is intermediate between a trivially short task and a discouragingly long one. And of course a fifth is that when a long series of the targets is randomly arranged, the obtained success can be judged objectively as falling within or outside of the limits conventionally assigned to chance.

For statistical evaluation of the data, Rhine sought the best advice and continued seeking it; he turned to and continued to depend on two fine mathematicians. Drs. J. A. Greenwood and T. N. E. Greville. He used their formulas to evaluate his data. Although it puts us ahead of our story, it seems appropriate to state here that when their formula showed his results to be extrachance and outsiders criticized his use of statistics, he was on safe ground. He appealed to the Institute for Mathematical Statistics and their president-elect signed and issued publicly a statement which Rhine often delighted to quote: "If the Rhine investigation is to be fairly attacked it must be on other than mathematical grounds."

Here, then, was a first methodological contribution: a simple and appropriate method for forced choices which avoided a whole series of pitfalls in earlier techniques.

You remember that Rhine's primary objective in his first major project was to find gifted subjects who would reliably show high extrachance scores. His dazzling success in finding them would be a pleasure to describe and interesting to examine, but because this is a topic for other papers I will merely mention it in passing. What is noteworthy for my topic of methodology is that while searching for and working with these subjects, he did not blind himself to other possibilities; he did not keep singlemindedly to his first narrow

objective. Instead, his keen observations of portions of his data led to two further important methodological advances and to a gallant attempt at a third.

The first of these three related to his decision to test for the gifted subject. This purpose implies a theory: that there is a dichotomy between those capable of prolonged, clear success and others who are incapable of it. It foreshadows Millar's recent provocative hypothesis (1979) that positive findings in psi will come only if the research includes psi stars; that is, only if there are either gifted subjects who are psi stars or else experimenters who are psi stars and can influence their subjects to respond with significant data. The thesis is defensible biologically, because some human characteristics like albinism or some forms of gigantism are similarly discontinuous. ESP ability may thus have a genetic base or some decisive congenital or developmental cause. Rhine's thinking tentatively followed this line, and one early finding tended to support it. When he asked his subjects whether members of their families showed psychic ability, almost all the star subjects answered affirmatively but the other subjects did not.

A lesser man, having developed this approach and apparently begun to confirm it, would have stayed with it and not looked further. Rhine did not. He had observed well before 1934 that when he gave ESP tests to a largish group and no one individual made an outstanding score, the group mean was still likely to be high—and he did not forget this observation merely because it was irrelevant to his theory. In his second book (Rhine, J. B., 1937) he still wrote that "it seemed entirely safe to estimate that at least one in five of the persons tested showed ESP capacity" (p. 106), but his later work showed that his thinking was changing to fit his data. Tests with unselected subjects gave such interesting results that some twenty years afterward his theory had completely reversed (Rhine, J. B., 1955), and he wrote: "the assumption that certain individuals were endowed with psi . . . will have to give way completely to the other extreme: that subjects are *made*, not *born*: that exceptional performance represents a combination of conditions within and around the subject, conditions that favor the functioning of psi to an exceptional degree and may—in fact, usually do—continue for only a limited period of time" (pp. 108–109).

This thesis is important both methodologically and theoretically. It leads to supplementing research on those who are presently gifted by doing research on anyone who is available. It permits a far wider range of investigations than does the earlier theory, and suggests a

new set of problems for study.

The second of these three early observations, prominent even in Rhine's first book, describes what he later called "psi-missing." Though Rhine's aim was to find high scores, when he saw consistently low ones he did not neglect or suppress them; he utilized them for new thinking—and new methods. He interpreted them as being a result of tension or of "mental habits and patterns of association that are . . . obstructive to psi" (Rhine, J. B., 1969, p. 17), that is, as resulting from cognitive interference due to stress. (This theory has not yet been adequately tested; the interaction of stress and response bias still deserves more intensive investigation than it has received.) Thereafter, throughout Rhine's career in parapsychology, his methods were enriched by taking psi-missing into account. He recommended ways of avoiding it or of encouraging it; he began to make it predictable. A second methodological innovation, then, was to examine for consistent low scores as well as for consistent high ones. This also expands the area to be investigated.

His attempt at a third methodological advance has not yet been successful. It concerns methods of testing which will elicit psi; and most parapsychologists recognize with regret that we do not yet know how to do this. Even in 1934, however, Rhine's recommendations were so full and seem to me so wise that I consider it astonishing that they failed to be incorporated into a successful method.

They fall into three categories. The first relates to conditions conducive to psi in the person being tested. These are, he wrote, a combination of alertness and detachment; in his own phrase, "relaxation of all sensory functions and abstraction from all sense-stimuli"; also effort, striving, motivation for money or for kindness, for play or for display; self-control; capacity for attention; confidence; patient persistence; effort and voluntary attention; easy informality; tendencies to daydreaming, to high imaginativeness, to artistic ability and hypnotizability and sociability.

His second category described conditions that adversely affect psi in the subject: distraction, fatigue, haste, strain, self-consciousness, either an unwelcome change of procedure or any procedure that the percipient expects to be inhibitory, doubt, negativism, monotony, drowsiness.

His third category, prescriptions for an experimenter, include stopping when the subject wants to—or even earlier; giving short runs; expressing no doubt; following the subject's preferences on procedure at first; not giving extravagant praise; showing playful



informality and light humor; encouragement; varying the procedures; and, in general, the approach that makes for effective salesmanship in any area.

In spite of this wide range of insightful recommendations, Dr. Rhine was unable to develop a dependable method for eliciting the experimenter-subject relationship that would produce evidence for psi. One of his practical solutions for this difficulty was to observe each experimenter's data, infer that experimenters whose data were consistently null were unable to set up the necessary rapport, and recommend that they refrain from conducting psi research. He argued that failures to produce significant data are not necessarily instructive; and of course this is true. But though the expedient of excluding such experimenters may be useful in the short run, especially when money to support experimenters is limited, Rhine recognized that this policy could sacrifice what might be a useful field of inquiry. He later recommended (Rhine, J. B., 1976) that conditions—including experimenters—expected to produce null results be employed if they were only one aspect of a project and their data were to be compared with data from other conditions expected to yield significant results.

Before I leave this topic, let me add a personal note of appreciation for Dr. Rhine's insights into it. In a recent study comparing experimenters who typically elicit psi with those who typically do not (Schmeidler & Maher, in press) I compiled a list of adjectives that might discriminate the two groups. Many from Rhine's writings were included because I too thought them appropriate, but I hesitated about "playful" which seemed to me to be inappropriate, and eventually included it only because Rhine had suggested it. And "playful" was one of the adjectives which, as he had anticipated, significantly discriminated the psi-conductive experimenters from the psi-inhibitory.

These last three methodological contributions have all been general ones; it is time to describe some ingenious specifics. Within ESP, Rhine thought long and hard about the mode in which it appears. Although his earliest research was formally conducted to test the possibility of telepathy, he soon recognized that when the target was both a record or object and also the thought about this in the agent's mind, extrachance data could come from clairvoyance, from telepathy, or from a combination of the two. His earliest methods could not distinguish among them.

Rhine and his staff members soon developed a wide range of testing techniques, from calling down through a concealed deck to

screened touch-matching; another set of research contributions. Among these methods were one or two which seemed to exclude even precognitive telepathy, so that their extrachance data established clairvoyance as a mode of ESP.

He then devised an ingenious technique to test for "pure telepathy" with randomized targets. The telepathic agent would translate a random list of digits into ESP symbols by a private code, never spoken or written; the subject would try to respond with the symbols, not the digits. Pure though this seems, Rhine later disavowed its extrachance successes as evidence for telepathy by considering that they might come from clairvoyance of the agent's brain processes, a metaphysical argument which seemed to make the telepathy problem insoluble.

A major contribution to methodology dealt with PK. From the days of his first interest in psychical research Rhine had considered PK an important topic for investigation, and in 1943 he and Mrs. Rhine published an innovative report of a quantitative method for working with it: throwing dice while hoping a particular face would turn up (Rhine, L. E., & Rhine, J. B., 1943). The method was basically parallel to the work with ESP cards. Theoretical expectation for chance success became 1 in 6 instead of 1 in 5; the run unit soon became 24 instead of 25. Justification for using theoretical probabilities even with imperfect dice was achieved by counterbalancing, so that the standard procedure soon demanded that each subject hope an equal number of times for all of the six die faces. This statement of a readily available objective method, sensitive to small effects, inspired attempts at direct replication as well as new ingenious techniques based on the same general principle; it opened up the field.

I have been writing almost as if Rhine's eventual rigorous techniques sprang full-fledged from his brow. This is not only false; it does a disservice both to other parapsychologists and to his own qualities as a man of intellectual integrity and zeal for rigor. What actually happened with both the ESP and the PK research is that Rhine proposed major innovative breakthroughs, tested them in the first heat of enthusiasm, and obtained data too striking to disregard. Critics both within and without parapsychology examined the first work and found, or thought they found, methodological loopholes in it. Rhine welcomed all such criticisms and transformed them into constructive contributions: with the help of his colleagues he invented or utilized new methods which would make the research fully rigorous.

The work on PK is a case in point, for the striking data from his first procedures were provocative but unsatisfying; and indeed by the

time the Rhines published their first report (Rhine, L. E., & Rhine, J. B., 1943), more refined methods from his laboratory had been used and were published in the same issue of the *Journal of Parapsychology*. Still later work from his laboratory controlled for motor skill in throwing dice by using mechanical releases; it required subjects to call all faces the same number of times; and it instituted various other controls such as those against optional stopping. It provided some control for recording errors by having two independent records for each outcome. Rhine had earlier insisted on blind recording for all ESP work, and with the advent of Schmidt's random events generator, the recent PK research from Rhine's laboratory has closed this loophole too by using mechanical recording for PK.

Three further methodological contributions need to be described: one which helps toward rigor; one for reliability; and one, not yet fully implemented, which may provide another important breakthrough. The first has already been anticipated. It is in effect a series of caveats, of "Thou shalt not's"—but modified characteristically, by this wise and modest man, to "Thou shalt not at present." Rhine considered that clairvoyance had been established as a mode of ESP and that complicated techniques like Mangan's permitted precognition to be established as another mode, distinct from PK: he also considered PK to be established, as distinct from precognition. He recommended therefore that investigators not befuddle themselves by attempting to work in other, cloudier areas (unless they were able to invent some new technique to make the areas clear). He specifically recommended against attempts to study telepathy, since no method now known could distinguish telepathy from clairvoyant response to agents' brain processes. He recommended against attempts to study retrocognition, because the facts which would establish retrocognitive success might have been available to clairvoyance. He recommended against studying retro-PK, because success here could be interpreted as an experimenter's precognition of the subject's later assignment and then (unconscious) PK upon the targets to make them match the assignment. He recommended against attempts to study either out-of-body experiences or survival because of the possibility of "superpsi" interpretations of all affirmative data in terms of clairvoyance, precognition, or PK in various combinations. But he modestly kept open the possibility that someone might later invent an adequate procedure for any of these; his recommendations were only against unparsimonious attempts to study them with our present inadequate methods.

The second piece of methodological advice was to complete two

formal stages of research before publication: to conduct first an exploratory and then a confirmatory project. Most scientists would consider this unconventional. In psychology, for example, it is usual to state a hypothesis, then test it, and take significant support for the hypothesis as adequate confirmation. If pilot studies are performed they are usually informal, merely preliminary attempts to find a workable procedure. Stating one's hypothesis substitutes for Rhine's first formal exploratory stage.

Rhine's more conservative requirement can be taken as the hallmark of a man who has learned to hold theories lightly; of the empiricist who demands replication of any interesting finding before giving weight to it. Perhaps also it reflects special characteristics of psi research, where an experimenter's initial enthusiasm might, through psi, influence subjects so as to support the hypothesis (even where there is no subtle cuing by verbal suggestions). Since the enthusiasm is likely to diminish after the first series, data which support the hypothesis in a second series will be considered more robust. Replication is in any event appropriate statistically, since even a method which yields a significance value of  $p = .01$  will, on average, be due to chance about one time in a hundred. The demand that significant data be labelled exploratory the first time they are obtained, and considered confirmatory only when replicated, may seem unnecessarily onerous to some of us, but surely testifies to Rhine's high standards for research in parapsychology.

The last on my list of Rhine's methodological contributions has as yet been insufficiently utilized and seems likely to prove more important in the future than it has been already. It was stimulated by the discovery of the decline effect in PK. With 24 successive calls for one face of a die, PK successes are likely to be markedly higher at the beginning of the series than at the end. The statistical significance of this decline in success has in general been greater than the significance of all calls, pooled, tested against mean chance expectation. Since decline effects are also common in ESP, they seem to be a characteristic of psi when the subject is required to make a quick succession of forced choices.

Rhine utilized these effects and then generalized from them. He borrowed Ehrenwald's term *tracer* to designate any internal difference within psi data which is observed so often that it seems characteristic of psi (Rhine, J. B., 1975). His basic argument about such tracer effects is that their significant and predictable differences between calls could not occur in the absence of psi. They therefore show that psi has affected the responses. Tracer effects are thus

doubly useful. They show that psi has occurred and they also give information about how psi operates, about the nature of psi.

Using tracer effects is potentially so powerful a methodological advance that I will try to spell out some of its implications. The thesis proposes that after a body of data has been obtained in a psi experiment and used to test the experimenter's hypothesis, further searches should routinely be made. One, of course, might be for the decline effect. Others might be for the differential effect; for a relation to the experimenter's (prerecorded) mood or for a correlation between run scores; and there are other possibilities such as sheep-goat differences.

If the search for tracer effects has been predated as a formal part of the research design, a correction of the probability value for significance of any particular finding must of course be made; but the gain in extracting further information from the data may outweigh the loss from partitioning  $p$ . And if the search for tracer effects is considered only pilot work, or perhaps exploratory, even this disadvantage will not be present. By the expenditure of a little extra effort in data analysis, the experimenter may have gained important insights into factors that interact with the original hypothesis, and important leads for later confirmation. Learning through these tracer effects about how psi operates may make it possible to design more fruitful research projects; and Rhine (1975) even speculated that they might be a key to adequate study of the problem of survival.

Have I been neglecting part of my assigned topic: Rhine's contributions to standards of research? Not entirely. They enter in one aspect, through Rhine's demands for rigorous controls and his insistence, long before it was common in psychology, for double-blind experimentation. They explain why he welcomed criticism and then used the criticism to make methodological advances. They enter in another aspect, too, with his rejection of what he considered unparsimonious interpretations of psi data, from telepathy to out-of-body travel.

But there are further ways that they enter his work. I shall cite two examples; there could be many more. Not only did he himself avoid the use of anything resembling deception of the subject in psi testing, but he had so strong a distaste for it that he urged others also against any such attempts. A formal argument against deception is that the subject's ESP might be able to penetrate it, but it seemed clear, as Rhine discussed the topic, that deception per se was so distasteful to him that he felt any experimenter willing to deceive a subject would be incapable of establishing proper rapport and perhaps even felt

such an experimenter to be suspect, unworthy to be a parapsychologist.

These high moral standards permeated both his scientific and his popular writing. In his lectures and in all his work from the earliest to the latest, he was careful as he presented his persuasive data or arguments to point out any of their inadequacies which he had privately evaluated. He was cautious in warning his listeners or readers not to overgeneralize from the results. He presented his interpretations in an admirably tentative way, distinguishing carefully between his opinions, which might be strong, and the facts on which the opinions were based.

To a large extent, Dr. Rhine's standards were the determinants of his methods. His zeal for rigor in investigations drove him to seek, invent, and encourage methodological advances and refinements.

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## REVIEW OF J. B. RHINE'S RESEARCH FINDINGS: I. EXTRASENSORY PERCEPTION

By JOHN PALMER

If there is one term in parapsychology that is associated with the name of J. B. Rhine, that term would have to be *extrasensory perception*, or ESP. His contributions to research on this topic were conceptual, theoretical, methodological, and empirical. Although this paper will focus on his empirical contributions, it must be recognized that the other kinds of contributions cannot be separated from the empirical ones. Likewise, Rhine's empirical contributions cannot be adequately assessed by referring exclusively to the papers on which his name appeared as an author. As director of a major research laboratory for over fifty years, he sponsored much research by other investigators, both at his own laboratory and elsewhere, which represented or extended his thinking. It would be impractical for me to try to cite all the research he sponsored, but I will cite some of it that I consider particularly indicative or representative of Rhinean parapsychology at the time it was conducted.

It is not my primary intention in this paper to provide an encyclopedic compendium of the research findings accumulated by Rhine and his co-workers, but rather to highlight the trends which seem to define the evolution of Rhine's approach to ESP research. Thus the perspective of the paper will be somewhat historical. I am speaking as one who is familiar with most of Rhine's published articles throughout his career but whose personal contacts with him date only from 1966, when I began the first of two summers working as a research associate at the Institute for Parapsychology. These various exposures have left me with a personal perspective on his work which I will share with you today. It is a perspective which may or may not coincide with that of persons who were associated with him more closely over a longer period of time. Yet I offer my perspective with no apologies. After all, it is the responsibility of all of us whose lives he touched, however indirectly, to collectively define and evaluate his contribution to parapsychology. Indeed, this conference is a first step toward a discharge of that responsibility.

Rhine's first published research report, ironically enough, was not based on a Duke college student, but on a horse by the name of Lady

(Rhine, J. B., 1929). The horse's owner, Mrs. C. D. Fonda, claimed that Lady had telepathic abilities. When Mrs. Fonda would think of a number or letter of the alphabet, Lady would touch her nose to the corresponding block on a table. Lady was excellent at this task when Mrs. Fonda was standing by her side, but Rhine recognized that Mrs. Fonda, who knew the targets, could unconsciously provide Lady with sensory cues. Rhine thus proceeded to institute a series of controls that progressively isolated Mrs. Fonda from Lady. Results became progressively poorer as the controls were tightened, but they remained impressive enough to suggest to Rhine that Lady had genuine psychic abilities. However, sensory isolation was never complete and the statistical tests that later were to become the hallmark of Rhine's research were not applied, rendering the results only suggestive by modern standards. Moreover, a later visit yielded such poor results that Rhine was forced to conclude that Lady had lost whatever abilities she may have earlier possessed (Rhine & Rhine, 1929). Such decline effects were to haunt Rhine throughout his career.<sup>1</sup>

While the research with Lady was not particularly representative of Rhine's later work, it did contain the rudiments of the forced-choice methodology which he progressively refined during his career. The high point of that career came in the early 1930s. Having been a first-hand witness to the fiasco of the Margery mediumship (Rhine, J. B., 1927), he realized the importance of simple and highly controlled experimental designs in demonstrating the reality of psi. He also had come to appreciate the value of statistics in objectifying the significance of quantifiable outcomes. What he needed was subjects who could produce such significant outcomes under controlled conditions. Unable to find other gifted animal subjects, Rhine pursued his quest for human subjects in a logical fashion by screening classes of college students at Duke using simple card-guessing tests (Rhine, J. B., 1934/1973). The results of these tests were unimpressive on the whole, but they did yield one outstanding subject, A. J. Linzmayer. Soon thereafter, several other outstanding subjects were revealed among the Duke student population, the most notable being Hubert Pearce and Charles Stuart.

Rhine patiently accumulated thousands of trials of card-guessing

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<sup>1</sup> Rhine's other major contact with animal research occurred in 1952 when he undertook an investigation, sponsored by the U.S. military, of the ability of dogs to locate underwater mines by ESP. For each trial, a land-mine case was randomly placed in one of five positions on a linear trail, and the dog indicated his response by sitting down at one of the five locations. Initial results were significantly positive, but a decline effect later began to set in and the project was abandoned (Rhine, J. B., 1971).



data from these subjects, all of whom consistently provided scores which, if not always spectacular, were nonetheless above chance to a highly significant degree when statistical tests were applied. In 1934, an account of these tests was published in a monograph entitled, inconspicuously enough, *Extra-Sensory Perception* (Rhine, J. B., 1934/1973). The furor which this monograph created in orthodox academic circles will be discussed by other speakers. It is easy with twenty-twenty hindsight to criticize the lack of ideal controls which characterized this early work, but one must realize that Rhine was pioneering a relatively new methodology and new methodologies must be refined over time. It is to Rhine's credit that he took to heart the legitimate criticisms of his antagonists and refined his methods accordingly. These advances were reflected in his next major scholarly book, entitled *ESP After Sixty Years*, which was published in 1940 (Rhine, Pratt, Stuart, Smith, & Greenwood, 1940).

The two experiments which Rhine felt provided the strongest evidence for ESP were both conducted with his principal colleague, J. G. Pratt, as the experimenter: the Pearce-Pratt series and the Pratt-Woodruff series. In the former, a BT clairvoyance procedure was utilized, with the subject and experimenter isolated in different buildings on the Duke campus (Rhine & Pratt, 1954). The cumulative results over 74 runs yielded a mean of 7.5 hits per run; the probability of so large a deviation occurring by chance was less than  $10^{-22}$ . Equally impressive were the results of the Pratt-Woodruff experiment, which employed the screened-touch matching (STM) method of clairvoyance testing (Pratt & Woodruff, 1939). A total of 2,400 runs from 32 subjects yielded a mean of 5.20 hits per run, which is associated with a chance probability of less than  $10^{-6}$ . Neither of these experiments has escaped criticism, but the critics at least were forced to speculate about unusually ingenious fraud on the part of the subjects and/or fraud by the experimenter(s) to explain away these findings (e.g., Hansel, 1979). A particularly effective defense of the Pratt-Woodruff experiment was recently published by Pratt (1976).

#### IN SEARCH OF THE LIMITS OF PSI

A strong secondary interest of Rhine at the time these data were being accumulated was the establishment of the physical limits of psi. Large amounts of additional card-guessing data were piling up from testing done at Duke and elsewhere, which allowed for more generalized conclusions along these lines. Rhine repeatedly found himself required to conclude from these data that the physical limits of psi

simply did not exist. ESP seemed all-pervasive and, more importantly, it seemed to transcend the laws of classical physics.

#### *Source of Information*

Rhine's embrace of the term *extrasensory perception* reflected his premise that ESP is an alternative means of accessing information from the environment. This, of course, had been the model accepted by most parapsychologists up to that time. It also was assumed by most researchers, at least in the English-speaking world, that the paradigmatic form of ESP was telepathy. So pervasive was this assumption in the U.S. that John Coover (1917) insisted upon treating clairvoyance as a non-ESP control condition when presenting the results of his controversial ESP experiments at Stanford.

One of Rhine's most important research contributions was to demonstrate that his subjects could score as well on clairvoyance tests (i.e., when no one knew the target order at the time the subject made his responses) as they could on telepathy or GESP tests (Rhine, J. B., 1934/1973). Although particular individuals would score better with one procedure than another, these trends were never consistent across individuals, suggesting that they were attributable to psychological factors such as preferences. It is perhaps debatable whether the alternative hypothesis of precognitive telepathy had been ruled out in the clairvoyance tests to quite the extent that Rhine (1945) supposed, but he nonetheless drove home the important point that one could no longer blithely assume that only another mind could be the source of ESP impressions. Clairvoyance had dethroned telepathy as the major subspecies of ESP.

#### *Space and Distance*

If ESP is not limited by the source of the information, what about the location of the information? Because ESP was implicitly conceptualized as an analog to normal sensory perception, much of the early research had the subject in close physical proximity to the targets. However, even the monograph *Extra-Sensory Perception* included reports of experiments conducted with the targets in a different room, a different building, or even a different city from the subject (Rhine, J. B., 1934/1973). In some cases there did seem to be a decline with distance. In the Cooper-Ownbey "pure telepathy" (PT) series, for example, Cooper averaged 9.2 hits per run when he and Ownbey were in the same room and only 5.8 hits per run when they

were one room apart. However, the decline could readily be attributed to psychological factors, as the participants were not blind to their partner's location. Moreover, in the Zirkle–Ownbey experiment, for example, which also used the PT procedure, Zirkle's scores increased from an average of 14 to 16 hits per run as he moved from the same room with the agent to two rooms apart. Neither did distance seem to be a factor in the better controlled Pearce–Pratt series. Although the mean number of hits declined as the distance was increased from 100 to 250 yards, some individual run scores at the longer distance matched the more consistently high run scores at the shorter distance.

Rhine (1937) later summarized the results of all available work over longer distances ranging from 70 to 3,000 miles. The mean for 4,083 runs was 5.11, which was significantly above chance ( $CR = 3.4$ ).

In the 1940s and 1950s, Rhine and his colleagues, most notably Elizabeth McMahan, reported a series of long-distance tests with a physician from Yugoslavia named Carlo Marchesi, who had reported dramatic success testing himself with cards (McMahan & Bates, 1954; McMahan & Rhine, 1947; Rhine, J. B., & Humphrey, 1942). Several clairvoyance and precognition tests were conducted over a period of approximately 14 years, with the targets in Durham and Marchesi in Yugoslavia. The results were hardly spectacular, but the total of 1,352 runs did manage to reach statistical significance ( $CR = 2.77$ ). There also were significant internal position effects in the early tests. I will discuss these later in the paper.

In summary, it appeared that ESP was independent of space and distance. Later research, including studies designed to control for psychological factors (e.g., Osis & Turner, 1968), has done little to alter this conclusion in the opinion of most parapsychologists, although the possibility remains that more reliable data or more refined analysis techniques may yet provide evidence of a dependency of ESP upon distance.

### *Time*

If ESP is not limited by space, perhaps it also is not limited by time. Specifically, can ESP extend into the future?

Rhine initially tackled this problem through a simple modification of his standard card-guessing procedure: instead of shuffling the deck of cards before the subject makes his responses, shuffle it *after* the subject makes his responses. In 1938, Rhine reported the results of 15 series involving 11 experimenters and 49 subjects utilizing DT

and matching procedures (Rhine, J. B., 1938b). The mean number of hits per run over 4,523 runs was 5.14, which was highly significant statistically ( $CR = 4.5$ ). Moreover, the level of scoring was comparable to that obtained in clairvoyance series using the same test procedures. It appeared that precognition could now be added to the catalog of demonstrated psi effects.

Unfortunately, there was a hitch. Was it possible that whoever shuffled the cards could shuffle them in such a way as to match the subject's guesses, for example, by using ESP to know when to stop shuffling? Rhine was not inclined to favor this "ESP-shuffle" hypothesis, because the precognition results were so similar in pattern to the clairvoyance results and because the results for each experimenter (or shuffler) varied depending upon the particular subject he or she was testing. Nevertheless, it deserved a test. In conjunction with Burke Smith and Joseph Woodruff, Rhine tested the ESP-shuffle hypothesis directly by having subject and experimenter each shuffle decks of cards and match the corresponding sequences (Rhine, Smith, & Woodruff, 1938). A total of 203 subjects and 13 experimenters at both Duke and Tarkio College participated. The results from 8,461 runs yielded a highly significant mean of 5.20 hits per run ( $CR = 10.4$ ), a mean slightly higher than the 5.14 obtained in the original precognition work. The ESP-shuffle hypothesis was a matter to be reckoned with after all.

A new series of precognition tests now had to be undertaken to rule out the ESP-shuffle hypothesis. This was attempted in the first series of tests by utilizing a matching procedure in which the key cards were selected either by mechanical shuffling or by mechanical dice throws (Rhine, J. B., 1941a). Twenty-four subjects completed 1,608 runs, but the mean was almost exactly at chance. However, there was a strong difference in scoring between the 19 adult and 5 child subjects. The adults scored significantly below chance ( $\bar{X} = 4.78$ ;  $CR = 3.59$ ), while the children scored significantly above chance ( $\bar{X} = 5.25$ ;  $CR = 2.75$ ), with the difference being highly significant ( $CR_d = 4.29$ ). Although each of the previous precognition studies also involved both adult and child subjects, this was the first time such a difference was significant in precognition work (although it had appeared in some clairvoyance experiments). Nevertheless, this post hoc effect seemed strong enough and sensible enough to confirm the precognition hypothesis in the context of previous research.

However, even this series proved inadequate methodologically. Could not the mechanical target selection have been influenced by PK? This new counterhypothesis was dealt with in a subsequent

report of 2,302 precognition runs completed by 22 children and 19 adults using a DT procedure (Rhine, J. B., 1942). Target orders were determined with reference to the maximum and minimum temperatures of the preceding day from the Durham newspaper, a process seemingly less influencable by PK. The overall deviations were nonsignificant for both adults and children, but some significant evidence of ESP was obtained post hoc through examination of position effects. These effects, which appeared in a number of other studies, will be discussed later.

Precognition became a standard test procedure at Rhine's laboratory from then on, and numerous studies obtained significant results using this method. Rhine's name generally was not attached to these studies, but one notable exception was a rather ambitious undertaking in which *Maclean's Magazine* and the Canadian Broadcasting Company collaborated with Rhine in a mass precognition test (Rhine, J. B., 1962). An article on ESP appeared in one issue of *Maclean's* along with a postcard on which readers could record a sequence of ten digits (0-9) that later would be matched to a target order generated by computer. A separate target sequence was generated for each of the 29,706 cards received. It was a reflection of the times, perhaps (both in parapsychology and the real world), that the results were significant in the psi-missing direction ( $CR = 2.53$ ).

Rhine also never found any convincing evidence that precognition scores declined as the time interval between responses and target generation increased (e.g., Humphrey & Rhine, 1942; Rhine, J. B., 1942). ESP really did seem to be independent of time as well as of space, although again we must allow for the possibility that further refinements may someday alter the picture.

#### THE DISSECTION OF ESP

I noted earlier that Rhine was haunted throughout his career by the decline effect. This was true, not only of particular subjects, but also of the research in general. The consistent success of the early 1930s was not to be duplicated in subsequent decades. There never again appeared a group of star subjects that rivaled Pearce, Linzmayer, et al., although occasionally a few stars appeared on the horizon only to quickly fade away. Perhaps the brightest of these stars was the schoolgirl Lillian, who once achieved a perfect score of 25 hits (Reeves & Rhine, 1942; Rhine, J. B., 1964). More typical of the new trend was the performance of a teenage boy identified as P. H., whose promising psi-hitting when tested informally reverted to significant psi-

missing when better controls were applied (Russell & Rhine, 1942).

On rare occasions Rhine would venture out to test more established stars. A two-year card-guessing study with the noted medium Eileen Garrett produced significant results the first year (especially on GESP), but results in the second year declined to nonsignificance (Birge & Rhine, 1942). Psi-missing intruded again into a field study of the dowsing abilities of Henry Gross (Rhine, J. B., 1950). This psi-missing, which received only passing mention in the monograph *Extra-Sensory Perception*, became an increasingly prominent feature of psi results as the decades passed.

### *Motivation*

What was responsible for this decline of fortunes? One finds only oblique references to the problem in the literature (e.g., Rhine, J. B., 1946), but I think it is safe to say that the decline was attributed at least in part to the dwindling enthusiasm of the staff as the novelty wore off, as the battles with the critics became more intense, and as the battles of World War II became everyone's primary preoccupation. Rhine always felt that strong motivation on the part of subjects was essential for success and that this motivation had to be incited and maintained by the experimenters, who must have this same motivation themselves. \*

There were indications from the early research reported in *Extra-Sensory Perception* that supported such an interpretation. Scores of the star subjects tended to decline to chance when testing sessions became too long and motivation waned. Shifts to novel procedures tended to rejuvenate dwindling critical ratios. In the two cases where scoring fell below chance (Frick and Linzmayer), Rhine attributed the psi-missing to unconscious negativism engendered by the length of the testing. Fatigue, illness, and the depressant drug sodium amylal were found to dramatically reduce scoring rates, whereas the stimulant caffeine caused them to partly recover.

Given the importance Rhine placed upon motivation, it is ironic how little systematic research has been done to explore its effects on ESP scoring. Subjects were often offered small rewards for high scores (e.g., Rhine, J. B., 1941a), but such rewards were never manipulated systematically. Rhine suggested that the children scored more positively than the adults in the 1942 precognition study because they were more highly motivated (Rhine, J. B., 1942), but this lead was never followed up. It was not until 1953 that Remi Cadoret (1953) confirmed the effects of depressant and stimulant drugs on ESP in a controlled study. Later in the paper, I will suggest a reason

why the motivation hypothesis was not pursued more directly.

The one motivational factor that was systematically explored, at least in terms of retrospective analyses of data, was the effect of delaying feedback of their ESP scores to the subjects. It was found with both clairvoyance and precognition procedures that scores were above chance when subjects received feedback after each run and below chance when feedback was delayed several days (Rhine, J. B., 1938a; 1941a). These latter runs often were conducted at other locations, with the record sheets being scored at Duke and then mailed back to the test site. Thus the sets of runs being compared may have differed in other respects besides delay of feedback. Nonetheless, such delays remain the most likely interpretation of these findings.

### *Separating Hitters and Missers*

The lack of star subjects caused the research emphasis to shift to studies amassing data from relatively unselected volunteers, often high-school and college students. Rhine quickly realized that the overall chance deviations frequently obtained in these studies could be attributed to a cancellation of psi-hitting and psi-missing. Thus a major research strategy at his laboratory from the 1940s through the 1960s was to identify factors that might discriminate hitting and missing. In the 1940s and 1950s, emphasis was placed on individual-difference or personality variables as the discriminators. The person primarily responsible for this emphasis was Betty Humphrey, one of the most prolific of Rhine's colleagues. The most widely used and successful of these predictors were certain personality inventories, especially the Bernreuter (e.g., Humphrey, 1945); the Stuart Interest Inventory (Humphrey, 1949, 1950a); and freehand drawings rated for expansiveness and compressiveness of form (Humphrey, 1946a, 1946b). These variables were combined as predictors in retrospective analyses to demonstrate greater separation of hitters and missers (e.g., Humphrey, 1950b).

In the 1960s, Ramakrishna Rao (1965) introduced the concept of the differential effect. This concept caused more emphasis to be placed on experimental manipulations, especially with regard to the nature of the targets as discriminators of hitting and missing (e.g., Rao, 1962, 1963). The concept of run-score variance provided a mechanism for evaluating changes in the direction of ESP scoring within a session and stimulated much research in the late 1960s (e.g., Carpenter, 1968; Rogers, 1966).

*Position Effects*

The contrast of psi-hitting and psi-missing was also manifested through the study of position effects within the run. As we all know, Rhine was a strong advocate of forced-choice as opposed to free-response methods of testing ESP. The only significant amount of free-response work ever to crack the *Journal of Parapsychology* were the drawing studies of Charles Stuart in the 1940s (e.g., Stuart, 1946, 1947). The relative virtues of the two kinds of tests have been debated vigorously (Honorton, 1975; Kennedy, 1979), and it seems that free-response methods are slowly winning the day. However, an important advantage of forced-choice tests, which Rhine fully exploited, was their conduciveness to the examination of internal scoring patterns in ESP data.

As early as the 1930s, Rhine noticed such scoring patterns in the runs of his star subjects (Rhine, J. B., 1934/1973). Some of these were decline effects, but the most striking were the salience effects, or U-curves, which seemed restricted to the DT test procedures. Specifically, he noticed with most of his star subjects that the psi-hitting tended to be concentrated in the first and last five-trial segments of the run. He attributed this effect to greater motivation on the part of the subject at these points in the run.

In the early 1940s, a series of research reports appeared that examined the salience effect more systematically. In the first and most elaborate of these studies (Rhine, J. B., 1941b), 1,114 DT clairvoyance runs were completed by 30 adult and child subjects. Experimental manipulations were introduced in an effort to vary the likelihood of the salience effect. Reasoning that salience would be most likely if the segmentation of the run was made identifiable to the subject, Rhine introduced brief interpolated tasks after each segment in some of the runs. Specifically, subjects were asked to either draw a picture or guess a number at these points. The uninterrupted runs were divided into those where subjects wrote down their responses and those where they called them out. Rhine reasoned that salience should be most apparent on the written runs because the record sheets used at Duke highlighted the segmentation.

This segmentation allowed Rhine to extend the concept of salience from the run to the segment; that is, greater ESP deviations would be expected on the first and fifth trials of each segment. Thus there was segment salience as well as run salience, and both were evaluated in the study I am describing.

As expected, the salience effects were significant only for the



interrupted runs, and they appeared with both the adult and child subjects. Segment salience also was evidenced in the uninterrupted runs, but only by adults when they wrote their responses. This seemed reasonable, since adults would be more likely than children to pay attention to the structuring of the record sheet. It is also noteworthy that the adults, who scored below chance overall, revealed an inverted-U type of salience effect; that is, the extreme psi-missing occurred in the first and fifth segments.

Finally, Rhine noticed that the patterns of run salience and segment salience seemed to coincide in the data. This led to the development of a new test statistic to measure this "covariance of the salience ratios," and application of this statistical test demonstrated that the covariance was indeed significant.

Although Rhine's salience analyses, especially the salience ratio, have been criticized as an abuse of the principles of probability pyramiding through post hoc analysis (e.g., Hilgard & Atkinson, 1967), it should be noted that the salience effects were replicated in a number of the early card-guessing experiments. The covariance effect was significantly confirmed in the early precognition work (Humphrey & Rhine, 1942; Rhine, J. B., 1942), in the preliminary sheep-goat research of Schmeidler (1944; Humphrey & Rhine, 1944), and in the first of the long-distance experiments with Marchesi (Rhine, J. B., & Humphrey, 1942).

On the other hand, replication by no means has been universal. Salience analyses tended to fall into disuse after the 1940s at the Duke laboratory and they never really caught on anywhere else. A brief renewal of interest in position effects occurred at Rhine's laboratory in the late 1960s, highlighted by the introduction of the concept of the cancellation effect. This concept, which spawned a couple of exploratory experiments before expiring (Rogers, 1967; Stanford, 1966), states that psi-missing in one half of the run cancels out psi-hitting in the other half. It was devised as an attempt to explain significantly low run-score variance in some ESP data.

The tradition of internal analyses of forced-choice ESP data survives most conspicuously in the response-bias research pioneered by Rex Stanford, which Rhine enthusiastically supported in the 1960s (e.g., Stanford, 1967).

#### RHINE THE EMPIRICIST

Many of the studies discussed in the previous section provided much potentially valuable information about the psychological pro-

cesses underlying ESP. Although these theoretical issues were doubtlessly of considerable interest to at least some of Rhine's colleagues, it is my impression that they were never really of primary concern to Rhine. One reason for this might be that he was trained as a botanist rather than as a psychologist. A more important reason may be that his principle objective throughout his career was not to understand psi theoretically but to provide evidence for its existence that would be sufficiently conclusive to the scientific community to annihilate the skeptical position. I do not think this reflected so much a lack of interest in the psi process (which, after all, he did discuss in many of his writings) as a sense that priority must be given to the evidentiality question.

So then why did Rhine support so much research at his laboratory which seemed to address theoretical and psychological issues about the nature of the psi process? I suspect that this was because he saw this research as a new way to demonstrate the existence of psi effects in data that seemed on the surface to be nonevidential. If the overall deviation was nonsignificant, one could still demonstrate psi by showing that the deviations of predefined subgroups were significant and that these deviations differed significantly from each other.

Perhaps the best illustration of the empiricistic nature of this research were the studies involving the Stuart Interest Inventory. This relatively successful predictor of ESP scoring was developed, validated, and cross-validated without any regard to the relevance of the items to any explicit or, as far as I can tell, implicit theory of the psychology of psi. The same could be said for the scoring scheme. Only briefly in the discussion section of one article could I find any mention of how the Stuart inventory profiles of hitters and missers might be interpreted, and this interpretation was never followed up (Humphrey, 1950a).

There are other indications of Rhine's empiricism that could be cited, only a few of which I will mention here. I found it revealing that, in his 1969 article on psi-missing, he hailed the differential effect, not as suggesting anything about the psychology of psi, but as a way to create conditions for demonstrating statistical significance (Rhine, J. B., 1969). I think his empiricism explains why he never saw any reason to abandon the critical ratio as the principle method of analyzing psi data statistically. If one is interested only in whether there is evidence of psi in the data, the *CR* is quite adequate. Finally, his empiricism explains why so many suggestive effects about the psychology of psi were never followed up, including the many intriguing patterns uncovered by Louisa Rhine in her analyses of

spontaneous cases (e.g., Rhine, L. E., 1956, 1962).

Although my own approach to parapsychology has been considerably more theoretical than Rhine's, my purpose in dwelling upon his empiricism is not to criticize it. I dwell on it because I think it is the fundamental cornerstone of what might be called the Rhinean paradigm, and its central place in his thinking must be acknowledged if one is to come to grips with the nature of his research contribution. His professional life, in my view, was a single-minded quest for conclusive evidence of psi. The fact that it could not be achieved in his lifetime is the challenge he has left to those of us seated in this room—a challenge to follow through. He laid for us a firm foundation, and in the final analysis that was his most important contribution to psi research.

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## J. B. RHINE ON THE NATURE OF PSI

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Throughout his long career Rhine was the acknowledged spokesman and representative of parapsychology, not only for the general public, but even more for the scientific community. As such he wrote and lectured extensively on every aspect of parapsychology, its aims, its achievements and the challenge which it presented to received ideas.

In attempting to expound Rhine's teachings on the nature of psi, the first thing one has to realize is that Rhine was primarily a practical man, a scientist and administrator, and that his two main objectives were (a) to make sure that research should proceed along the most productive and effective lines and not lose itself in futile projects, and (b) to justify parapsychology to the world at large as a pursuit of preëminent importance for mankind and to dispel the occult associations that it had inherited. Rhine was an enthusiast but he was not a philosopher. He was passionately committed to his views, but he had no interest in abstract ideas for their own sake. We may look in vain for any systematic exposition of his theories or for any concerted attempt to defend his position against its critics or for any profound analyses of the concepts that he uses so freely. These have to be disengaged from a general survey of his pronouncements.

What I propose to discuss in this paper is his conception of psi and how he thought it fitted into the scheme of things. Since one must perforce be selective, I shall confine the discussion to four themes to which, in his writings, he constantly returned. Naturally, they by no means exhaust the many topics on which he had something important to say but they illustrate, I think, as well as anything what is most characteristic of the Rhinean conception of psi. They are, in the order in which I shall discuss them: (1) the nonphysicality of psi, (2) the unconscious nature of psi, (3) the problem of survival, and (4) the universality of the psi faculty.

1. Undoubtedly, the most central tenet of Rhinean doctrine is that psi is nonphysical. Like his predecessors who founded the Society for Psychological Research, Rhine was convinced that parapsychology, and parapsychology alone, could provide the scientific answer to materialism. This is how he puts it in one of the many passages in which he makes the point:

It is now fairly clear that psi phenomena are identified by the fact that they defy physical explanation and require a psychological one. They always happen to people (or animals) or involve some associated or at least suspected agency or experience; but at the same time they do not follow conventional physical principles. (Rhine, 1953, p. 150)

In short, psi is mental but not physical. And the reason for calling it nonphysical was, so far as Rhine was concerned, quite straightforward; namely, that neither the physical properties of the ESP target nor its positioning relative to the subject seemed to have any effect on that subject's scores. Neither distance nor intervening material barriers were relevant variables in the psi experiment. Moreover, although this claim has periodically been challenged, I think it safe to say that so far no serious evidence has been produced that contradicts it. There may well be deeper, philosophical reasons for regarding psi as nonphysical, but these were the primary considerations that weighed with Rhine, coupled, of course, with the facts of precognition. For, even if we could conceive of some mechanism, some hypothetical radiation, that could bridge the space gap in ESP, we would still be left with no physical explanation of the time gap. Indeed, I think this accounts for Rhine's well-known predilection for the precognitive mode in ESP testing; at one stroke it disposed of any hypothetical mechanism of transmission.

In the passage I have quoted, Rhine advisedly uses the words "conventional physical principles," for by then he was aware that quantum theory had opened up a whole new world of subatomic particles whose behavior was as much at variance with the laws of classical physics as psi itself. The concept of "nonlocality" or of "space-time independence" is today entertained by quantum theorists without any sense of their having thereby repudiated physicality. However, "nonphysicality" is, as Rhine insists, only the negative criterion of psi; its positive criterion, as I have said, is its mentalistic attributes. "Nothing but a psi phenomenon . . .", he points out, "appears to defy all these criteria of physical operation and at the same time displays intelligent purpose in the process" (Rhine, 1953, p. 164).

The critical question that arises in connection with the mind-brain relationship is whether, as the materialist holds, the brain conceived as a purely physical system is alone responsible, in the last resort, for the whole of our behavior and experience, in which case mental phenomena may be regarded as mere "epiphenomena" of their underlying brain processes, or whether, as the mentalist holds, mental events can be causally efficacious. In plain language, are our thoughts, feelings,

and strivings no more than the subjective reflection in consciousness of what is going on in our brain; or do they, as our intuitions would suggest, make a difference to what we actually do? Rhine keenly appreciated what was at issue and rightly, in my opinion, argued that, if psi is a reality, the materialist thesis collapses, since no one has been able to suggest how the brain, conceived purely as a physical system, could produce a psi effect.

It is, perhaps, not always realized by the layman how widely the materialist thesis is currently held by psychologists, brain physiologists, philosophers, and other authorities. Many of them simply take their materialism for granted; but the more perspicacious among them, like the philosopher David Armstrong, would agree with Rhine that parapsychology *does* pose a threat to the materialist thesis. They differ from him only in refusing to acknowledge that the case for psi has been made. The ramifications of materialism are, truly, very far-reaching. For, if it is the case that all our decisions in life are made for us by the state of our brain at the moment, in accordance with impersonal electrochemical forces, then the notion of free will—that is, of man as a responsible moral agent—that is at the heart of so much that we have come to accept in ethics, in jurisprudence, in religion, and so on, is radically undermined. Rhine was right, therefore, to place so much emphasis on the nonphysicality of psi if he wanted to show that the importance of parapsychology went far beyond its purely scientific implications. At the same time it was, Rhine insisted, by virtue of its attachment to scientific methodology and criteria of evidence that parapsychology could hope to exert its influence in the modern world. His faith in the power of science is nicely illustrated by his repeated plea that parapsychology was the best defence which the free world had against the threat of totalitarian communism inasmuch as the materialist foundations of communist doctrine would simply crumble away once the facts about psi had become more widely known.

In the context of the traditional mind-body problem, Rhine would qualify as a dualist-interactionist. Yet he persistently disavowed the designation of dualist or, rather, confessed to being at most a relative or provisional dualist. This has always struck me as a curious idiosyncrasy for one who so clearly envisaged psi phenomena as representing a mind-matter interaction, but I think I can discern the reasons that led him to adopt this position. In the first place he was always concerned to dissociate himself from any kind of supernaturalist or transcendentalist view of psi. He clung firmly to the view that psi was as much part of nature as life itself, that there was nothing other-



worldly about it, that it had its own set of laws and could be studied as a natural phenomenon like any other. Secondly, as a scientist, he preferred to leave the door open to the possibility of some ultimate cosmology that would embrace both mind and matter and derive the properties of each from some more fundamental set of principles. In one place we find him saying: "It seems justifiable to expect to find underneath the surface of our somewhat arbitrary academic distinctions . . . a less definable but more basic reality than has been known hitherto in natural science" (Rhine, 1953, p. 164); and elsewhere, he is even more explicit in qualifying his postulate of nonphysicality, as when he says: "This distinction of nonphysicality is reasonably certain to prove to be a transient, even though a temporarily very important, point. It is important now as the essential negative boundary in the definition of parapsychology; it is necessary in order to call the attention of science to the existence of another domain of nature that is now measurably and experimentally demonstrable as a distinctive territory"; and then adds: "At least one inquirer has been urging exploration also of the positive or *common ground of nature that makes a psychophysical border necessary*; this should in due time become a principal object of scientific study for those in the field of parapsychology" (Rhine, 1960, pp. 75-76. Italics in original).

Perhaps Rhine's position could not unfairly be described as that of a would-be monist. In a letter he wrote to me in February 1964, after reading my book, *The Existence of the Mind*, written from a dualist standpoint, he explains why he is reluctant to go all the way with the dualists and more or less admits that it is partly a matter of policy. "The main difference it will make," he writes, "is on our getting together with our modern scientists in the United States and those in the U.S.S.R. They will much more quickly add a new *energetic element* than they would admit another order of nature. I am trying them out in Russia and, I think, making headway" (my italics). The fact that he speaks so frequently of psi as some new form of energy is surely revealing. Strictly, to speak of a "nonphysical energy" is as much a contradiction in terms as it would be to speak of an "unextended space." But Rhine, in his desire to promote parapsychology, wanted to have it both ways. Psi was at once the spiritual, immaterial component of the human personality and, at the same time, a new "energetic element" in nature. The confusion can also be traced, I believe, to certain common misconceptions about dualism that have been prevalent ever since Descartes himself first attempted to define the distinction between mind and matter. "One cannot even conceive the possibility" Rhine (1953) says at one point, "of two completely

different systems interacting and, yet, at the same time, constituting so manifestly unified a whole as the personality of man—without having something fundamental in common” (p. 161). It was this very point that worried Descartes and, even more, his disciples, some of whom tried to meet the objection by inventing fantastic models of parallel worlds that ran in synchrony but never interacted.

In fact, however, the objection had no force. Descartes and his followers had simply taken over a principle of scholastic philosophy according to which an effect must be of the same nature as its cause. But there is no reason in logic why this must be so. There is nothing contradictory in supposing that an immaterial entity, if that is what the mind is, could produce physical effects. There is, moreover, nothing to stop the mind creating mental representations of the external world in consciousness (what else do we mean by perception?) nor is there any reason why the material brain should not be made to conform to the will or intentions of the immaterial mind (what else do we mean by voluntary action?). The idea, still being purveyed by so many modern philosophers that there is something absurd or incoherent about dualist interactionism is without foundation. Rhine was quite right to insist that mind and matter must have “something fundamental in common” but that something is, precisely, the power to influence one another; nothing more than that is required.

2. Rhine points out (1953) that, while psychiatry gave to psychology the concept of the unconscious, “the experimental tools by which to deal quantitatively with unconscious processes have been and are being developed through the psi investigations” (p. 203). Moreover psi, Rhine believed, was unconscious in a stronger sense than the unconscious with which psychiatrists are concerned; “the operation of psi is *really* unconscious. It is unconscious in a different degree or way from experiences that are merely forgotten or repressed. . . . The operation of psi is, so far as the researches can indicate to date, irrecoverably unconscious” (pp. 203–204. Italics in original). This somewhat cryptic passage calls for elucidation. What did Rhine mean by psi being “really” or “irrecoverably” unconscious?

The word *unconscious* is used by psychologists in a variety of different senses, not all of them pertinent to the case of psi. Let us consider some of these. Perhaps the simplest meaning of the term is: not accessible to introspection. It is in this sense that a large proportion of all our cognitive processes, perceiving, thinking, remembering, etc., are unconscious inasmuch as we cannot know by introspection the information processing that must go on in the brain to make these

possible. What we are conscious of is only the outcome or end product of such processing, the percepts, the thoughts and ideas, the memory images, etc. Now the psi process, whatever we may understand by that, is certainly unconscious in this sense. What differentiates it from normal sensory awareness is, as Rhine has pointed out, that there is no specific modality of consciousness through which it is manifested. The psi signal is recognized as such only by the circumstances in which it occurs and the veridical information it conveys. It may be received in the form of an image but, equally, as Rhine mentions, as "an outburst of emotion or a compulsion to act." In a typical routine card-guessing test there may be no intimation of any sort, nothing beyond the guessing behavior itself. In the so-called physiological approach to psi, the unconscious aspect is taken a step further by cutting out the verbal call altogether and relying on some physiological index, a fluctuation in the subject's EEG record or GSR record, to serve as the response. By this means the subject does not even need to attend to what is happening and can let his mind wander. But perhaps the ultimate development of this approach is the "disguised" psi experiment that has become widely popular with investigators in recent years. For this, the subjects need never know that they are being tested for psi; they are told that the task is one of perception, memory, subliminal perception, or whatever, while in fact the situation is so contrived that success is made dependent on their utilizing their ESP or PK in some way to achieve their conscious aim. Stanford's concept of the psi mediated instrumental response is a recognition of this idea that the entire psi process can if need be operate at a completely unconscious level.

Thus, recent parapsychology has done much to vindicate Rhine's insistence on the unconscious nature of psi. And this idea has profound philosophical implications because it provides an answer to a question that has persisted in the philosophy of mind since Descartes; namely, can an event be both mental and yet unconscious? Normal cognition could never provide an unambiguous answer to this question, for it could always have been said that whatever was not conscious, in this instance, belonged, not to the mind, but to the physical processes of the brain and nervous system. "Unconscious cerebration" was how it was described in the 19th century. In the case of psi, however, we cannot attribute the effect to cerebral activity, but neither can we doubt that we are dealing with a mental activity inasmuch as it is intelligent, purposeful, and is concerned with communication and control. Thus, we have here an example of something that is, at once, mental and unconscious. It would seem,

therefore, that the empiricists were wrong to make mind coextensive with consciousness.

The meaning of *unconscious* that we associate with Freud is very different. In this case what concerns us are the hidden motives or reasons for our actions which, because they are so discreditable, we dare not admit to consciousness or acknowledge even to ourselves. Therapy then consists in overcoming this initial resistance. The Freudian unconscious, we may note, is an abstract theoretical construct in psychology and hence is not, in principle, incompatible with a physicalistic account of our mental processes. Now, is there any analogy in parapsychology to this meaning of *unconscious*? I think that there is and it is to be found most clearly in the phenomenon of psi-missing about which Rhine had plenty to say (Rhine, 1952, 1969). Psi-missing has been variously interpreted as due to an unconscious wish to avoid the target, whether because we are skeptics and so are anxious lest we add to the positive evidence for psi, or because we are secretly fearful lest we ourselves possess occult powers, or because we want to spite an experimenter who has bullied us into doing a task that we find irksome, or for any other such hidden reason. The closest analogy to psi-missing in sensory psychology is the phenomenon of "perceptual defense" in subliminal perception. Thus it has been shown that the threshold for recognition of emotionally charged words using tachistoscopic presentation is higher than for emotionally neutral words. Now the interesting point about perceptual defense is that, in the logic of the situation, there must be an unconscious recognition of the word prior to our capacity to articulate this: otherwise we could not know that it was a threatening stimulus. Likewise, in psi-missing, we must postulate unconscious recognition of the target; otherwise target avoidance would not be possible.

A third and final meaning of *unconscious* that I wish to discuss is that which devolves on our control over our sensorimotor skills. It cannot be too strongly emphasized that all the skilled activities of our daily life depend on a vast amount of "tacit knowledge" which never enters conscious awareness at all. It is sometimes said to be "preconscious." Moreover, as our skills become more fluent and proficient, so they become more automated and hence unconscious. As beginners, we are all too painfully conscious of each movement we make, but with practice this awareness fades away and, although we can, by an effort of attention, bring some particular component of the skill back into the focus of consciousness, this normally involves disrupting its smooth operation. Nevertheless, an essential feature of any normal skill that we have acquired is that it always remains under voluntary

control so that we can at all times deploy it as and when we require. This stands in marked contrast with the case of psi. So much so, indeed, that many philosophers have questioned whether it is proper to call our potentiality for using psi a skill or ability in any meaningful sense. Perhaps it would be less contentious if we called it simply a "gift." Now a gift can be cultivated, but whether it can be acquired or trained like any normal ability is more doubtful.

At the present time there is much discussion among research workers as to whether a psi ability can be trained or developed using the well-tried procedure of practice combined with instantaneous feedback. The evidence would suggest to me, at least, that the wrong analogy is being used. It could be that a closer analogy would be some such process as falling asleep. Thus we cannot, alas, will ourselves to fall asleep; it is something that happens to us rather than something that we do. Nevertheless, we are not completely helpless in this regard. We can discover by trial and error what are for us the most favorable conditions to facilitate the onset of sleep, what diet or exercise to pursue, or how best to compose our minds so that we may become sleepy. Psi-hitting seems to be as little under conscious control as falling asleep. When it comes off, it does so spontaneously. This, however, need not discourage us from searching for effective psi-conductive conditions or for techniques and disciplines both mental and physical that might enhance the probability of hitting. Even the possibility of finding a psi-conductive drug comparable to the existing sleep-conductive drugs need not be ruled out. The point is, however, that if Rhine is right when he warns us that psi is irrevocably unconscious in its manifestations, then we can at best resort to these oblique methods in our attempts to gain some degree of mastery over it.

3. It is now common knowledge that Rhine set his course firmly against research on post mortem survival. It is therefore ironical to discover that it was the survival problem that first brought him to Duke University. Thus we read (Rhine & Associates, 1965) that:

The special mission which brought the Rhines to Duke, although it did not measure the entire range of their interest in psychical matters, had to do with the claims of mediumistic communication with discarnate personalities, the question of spirit survival. (p. 6)

And yet, by the time Rhine (1951) came to deliver his Myers Memorial Lecture to the Society for Psychical Research in London on "Telepathy and Human Personality" in May 1950—one of the most definitive statements of his career—he had reached the conclusion, which he

never subsequently saw fit to modify, that the problem of survival, along with the problem of pure telepathy, must, as things stand, be reckoned among the insoluble problems of parapsychology on which no wise parapsychologist would henceforth waste any further time or effort. Here, by way of illustration, are a few characteristic passages:

A hundred years of more or less scientific consideration of the survival question has left the scientific professions more unconvinced and more indifferent to the claims today than ever. (p. 25)

Or again:

The question of whether the spirit survives bodily death depends first on whether there is anything like a spirit in man at all, or whether the belief that there is stands entirely without foundation in fact. (p. 26)

Or yet again:

So long as we are ignorant as to whether there is a distinctive spiritual component in the living individual, what sort of a subdivision it is if there is one, how independent and possibly separable such an element may be within the total personality, and what its properties are, we cannot expect to be able to design a *crucial experiment* to test the hypothesis that such a spiritual portion of personality survives the destruction of the body. (pp. 26-27. My italics.)

What had brought Rhine to this negative conclusion? The logic of his reasoning becomes plain enough if we view the matter historically. Myers, who coined the term *telepathy*, argued that, if we could demonstrate telepathy between the living, we would then be in a position to say that there was at any rate a known process that could, in principle, serve as a vehicle of communication between the living and the dead. Accordingly, the supposition that mediumistic communications might be inspired by discarnate agencies, using telepathy, would no longer seem absurd. As it turned out, however, experimental parapsychology demonstrated that we had to reckon, not just with telepathy but equally with clairvoyance, with a very general ESP faculty in effect. Even more disconcerting, while there was some evidence for pure clairvoyance, untainted by telepathy, the attempt to demonstrate pure telepathy untainted by clairvoyance ran up against certain insuperable methodological difficulties. At all events, once we grant that the medium in question could have obtained all the veridical information found in her communications by virtue solely of her own ESP powers, recourse to a discarnate intelligence is no longer warranted.

Rhine (1956) never denied that survival was a theoretical possibili-

ty; privately he may well have believed that it was true. Indeed, the gist of what he is saying is that, by demonstrating this nonphysical component in personality, parapsychology had made more plausible the hypothesis that we survive the dissolution of our brains and body. Nor did he regard the problem as unimportant. "Everyone," he writes, "no matter what nor how extreme his position, will recognize that, for most of the critical thinkers in the world, it will be highly important to find out, on the basis of incontestable evidence, just what the post mortem destiny of personality really is" (p. 30). But, as he here insists, it must be "incontestable evidence"; he saw no value whatever in continuing along the lines developed by the spiritualist movement.

I share Rhine's agnosticism as to whether we do in fact survive but I must question the validity of the arguments that he brings forward to dismiss the existing evidence that purports to demonstrate survival. He was, it seems to me, the victim of a false notion of scientific method. He uses, repeatedly, expressions such as "crucial experiment," "incontrovertible evidence," "conclusive proof," and so on. Yet, in point of fact, as Karl Popper has been at such pains to stress, in science there can be no finalities. At each step it is always a question of deciding on the most reasonable interpretation of the data, and it is only in the more favorable situations that anything approaching a general consensus of informed opinion can be expected. In parapsychology one is virtually never in this happy position. Once this is understood, Rhine's rejection of the survivalist claims is as arbitrary as his rejection of telepathy. Thus, if, in a given case of ESP, there is reason to think that the agent plays a critical role, this in itself is justification for invoking the concept of telepathy. Likewise, in the case of mediumistic communications, if it looks as if the initiative came from the discarnate agent—as, for example, in the case of the famous cross-correspondences scripts of the Society for Psychical Research—that would be justification for retaining the concept of post mortem ESP or, to use Roll's neater expression, "theta psi." There will, no doubt, always be those who would prefer to posit a "super-ESP" on the part of the living medium than posit theta psi on the part of the deceased entity just as, no doubt, there will always be those who would prefer the clairvoyant interpretation to the telepathic one and vice versa. However, once we are no longer beguiled by the false quest for certainty this need no longer worry us.

Rhine may well have been justified in thinking that survival research was not a good investment for parapsychology at the present time, but the theoretical reasons he gave for this were misconceived.

As a result he failed to appreciate the very real advances in survival research that were being made during the '60s and '70s, most notably, of course, the scholarly studies of cases of the reincarnation type by Stevenson, but also the phenomenon of "near-death experiences" to which Moody drew attention, or the accounts of deathbed visions assembled by Osis and Haraldsson. Rhine, no doubt, would have pointed out that we are dealing here with spontaneous phenomena where we are necessarily dependent on the veracity of human testimony with all its uncertainties. For, although he often mentions the spontaneous case studies of his wife, Louisa Rhine, he was emphatic that such evidence has no scientific status in its own right; its value consists in the hypotheses it suggests for experimental research. But dare we any longer assume that a laboratory investigation automatically takes precedence over the study of some real-life phenomenon? Consider the following passage in which Rhine (1953) comments on the investigation by the English parapsychologist, S. G. Soal, of his special subject, Basil Shackleton:

It is difficult to do justice to so extensive an experiment as this. Only those who have laboured for years under the strain of equally complex precautions can come anywhere near appreciating the evidential quality of these results. Such a person surely is entitled to wonder why anyone asks for further evidence. After all, what more could further evidence add to the assurance that under certain conditions ESP does occur? (p. 68)

I quote this passage not, heaven knows, to mock Rhine, for I have said as much myself on this topic, but to drive home the point that, no matter how impressive the precautions, no investigation considered in isolation is any more secure evidentially than the integrity of those who were responsible for it. We now know, alas, thanks to Betty Markwick, that this historic investigation was, in fact, worthless. The superiority of experimental evidence arises only when one has a clearly repeatable experiment that no longer rests on the trustworthiness of individuals. Until such a time a well-attested spontaneous case has as much claim on our credence as a laboratory report.

4. Rhine was unsure as to how far back in evolution to assign the origins of psi; but he believed that it was most probably something that we shared with much of the animal kingdom and almost certainly a common possession of humankind. It might, indeed, be more salient in certain individuals, with certain personalities, with certain groups or societies; but he was convinced (1947) it was there potentially in every one of us. "Most experienced investigators . . ." he writes, "have come more and more to accept the view that while



individuals differ greatly in their potentialities, most people—probably all—possess some of these parapsychical abilities to some degree” (p. 138). He was particularly insistent that psi should not be regarded as any sort of freakish abnormality. What gave him his confidence? I think he was impressed, in the first place, by the fact that, in so many of the experiments he discusses, psi shows the same sensitivity to psychological conditions as we would expect to find in testing for any normal skill or ability. Thus, the inhibitory effects of stress, distraction, boredom, frustration, etc., seem to operate as much with respect to psi performance as they do with respect to any more or less delicate psychological task. And the same holds good of such positive influences as encouragement, excitement, challenge, and so forth.

In the second place, Rhine took full cognizance of the important findings of Gertrude Schmeidler which showed a relationship between the pattern of scoring, psi-hitting versus psi-missing, and the beliefs and attitudes of the subjects involved. Her so-called “sheep-goat effect” has, in fact, stood up to replication as well as anything in the parapsychological field. Third, he draws our attention to the kind of salience effects, the U-shaped curves and suchlike, that crop up alike in ESP testing as they do in tests of recall. There are, of course, some striking contrasts. Most normal abilities show learning; psi, notoriously, shows decline effects. But there was, Rhine argued, sufficient lawfulness in the manifestations of psi to justify us in looking on it as a universal function or faculty.

Can we say that he has now finally been vindicated on this point? Alas, like almost every issue in this perplexing field it remains unresolved, as can be seen by the fact that at the 1980 Parapsychological Association convention a special roundtable was held to discuss the “distribution of psi” at which widely differing views were voiced. On the one side there are those currently working on the relationship between psi and personality who assume the validity of the universalist position and see their work as lending it further support. On the opposite side are those who rarely if ever obtain positive results and are acutely aware of the fact that certain experimenters consistently do so. They have raised the specter of the “psi experimenter effect” and ask whether these successful experimenters might not represent the self-selected few who use their own psi to achieve the results which they then attribute to their unselected volunteer subjects. Perhaps, they suggest, Rhine was misled by his sheer good luck in having among his early entourage a number of these psi-positive experimenters? Rhine, himself (1947, pp. 137–138), did indeed recognize early on

that some of his experimenters, like Margaret Price or Margaret Pegrarn, could obtain positive results where others failed but, at the time, he naturally attributed this to their social skills rather than to anything as extraordinary as their own psychic powers.

For my part I am still unsure where I stand on this issue. My own lack of success makes me sympathize with those who see psi as something very rare and exceptional. On the other hand, it is important to keep in view the distinction between psi performance and the potentiality for psi. There may be good reasons why we refrain from using psi except on very special occasions. For most purposes it pays us to rely on our sensorimotor system both for extracting information from the environment and for executing our intentions with respect to that environment. Most of us, I suspect, are so comfortably integrated with our brain and nervous system that we can no longer dispense with their aid. The crux of the problem when it comes to psi, as I see it, is how to induce your subject to forego these psychological crutches.

This completes my brief examination of Rhine's philosophy of psi. It is, as I have tried to suggest, a working philosophy for parapsychologists, not a finished or integrated theoretical doctrine that must be accepted or rejected *in toto*. Ever since its inception, there have, one can say, been two main views as to what parapsychology is really about. According to one school of thought parapsychology is a science with no subject matter of its own; it takes as its field of inquiry those puzzles and anomalies that have been disowned by the other sciences. According to the other school of thought parapsychology is an integral part of psychology, perhaps its most fundamental part—at any rate that part that deals with the mind-matter interface. I would say that Rhine's most important contribution to the philosophy of parapsychology is the impetus he gave to this latter point of view.

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## THE WORK OF J. B. RHINE: IMPLICATIONS FOR RELIGION

By JAMES A. HALL

The work of J. B. Rhine has clear implications for the study of theology and the practice of religion, although there has been little serious acknowledgment of this relevance. The scientific field of parapsychology owes the current definition of its borders to Dr. Rhine, and these borders clearly touch both the carefully cultivated fields of the sciences and the misty shorelines of the religions, shorelines with little stable definition but supporting vast shifting images of numinous awe and power. If science is the art of asking answerable questions, J. B. Rhine has posed the most imposing questions that have as yet been asked to either of these vast domains of human enterprise and endeavor. As Carl Jung wrote to J. B. Rhine on November 5, 1942 (Adler, 1973, Vol. 1, p. 322):

I quite agree with you that once we are in possession of all facts science will look very peculiar indeed. It will mean nothing less than an entirely new understanding of man and the world.

J. B. Rhine (1945b) indicated quite early in his writings an awareness of the importance of parapsychological findings for the field of religion, particularly in reference to dualism (Rhine, 1945a), ethics (Rhine, 1947), and the possibility of human survival after bodily death (Rhine, 1960). He later made two major coordinated presentations of these concerns (Rhine 1975b, 1977-1978), so that there is no doubt that in his own mind the field of parapsychology has a relation to the field of religion that is analogous to "that of physiology to medicine, and physics to engineering" (Rhine, 1945b).

I first became aware of Dr. Rhine's deep concern with this relation of parapsychology and religion when he made a major address at the annual meeting of the Society for the Scientific Study of Religion (Southwest) at Phillips University in Enid, Oklahoma, in 1976. We talked a great deal at that conference and made basic plans for him to participate in an academic course on the psychology of religion at Perkins School of Theology, Southern Methodist University. In 1977, he generously spent an entire week at SMU, presenting the weekly convocation address in Perkins Chapel, consulting with members of

the theological faculty, and addressing the Texas Society for Psychical Research on "Parapsychology and Religion," a meeting that fell with seeming synchronicity on Easter Sunday of 1977 (Rhine, 1976-1977).

Because of the deep and moving impression created by Dr. Rhine's visit, the sponsoring organization, the Foundation for the Study of Theology and the Human Sciences, began plans for a more focused symposium for the following year, one in which Dr. Rhine's concern about parapsychology and religion could be presented by him in a discussion forum with a select group of theologians, religious leaders, and representatives of the scientific community, particularly from the fields of psychiatry and psychology (Rhine, 1977-1978). This symposium on parapsychology and religion was held February 17-19, 1978, at Perkins School of Theology, SMU, convened by Albert C. Outler, Research Professor of Theology, and involved twenty discussants (Hall, 1977-1978a).

As we drove Dr. Rhine to the airport after this successful symposium he spoke of his sense of pleasure at having said what he wished to say about parapsychology and religion in an appropriate forum. I was also painfully aware that the symposium had been a strain on his remarkable physical energy and endurance. That he insisted on continuing the planned symposium in spite of recent health problems was a measure of the importance of the subject to his own deep purposes. He was unconcerned about any criticism that his presentation on religion might evoke in the scientific world. Several years before on the way to another airport he had said that his scientific critics never bothered him at all because he was always more critical of himself than any of them could possibly be. J. B. Rhine also encouraged the participation of Dr. K. Ramakrishna Rao in a subsequent symposium on parapsychology and religion (Rao, 1980), a symposium that took place only a few days after Dr. Rhine's sudden and unexpected death. We seriously considered postponing the symposium but concluded that "J. B." himself would have advised us to go right ahead with it.

In his 1977 Easter Sunday address to the Texas Society for Psychical Research, which was transcribed for the *Journal of the Texas SPR* (Rhine, 1976-1977), J. B. Rhine traced the changing emphasis of Easter in his own experience:

When I was a child I remember the concept of Easter as being a day of the promise of the literal resurrection. By the time I got into high school, it was a symbolic resurrection. By the time I got to college, where I was a pre-ministerial student, the concept was something else; not resurrection but of a promise of a spiritual existence of some kind that wasn't very

clear. Further on in my college career, I had to throw the whole thing out. I didn't know what to accept, I couldn't see the basis for any of it in my studies in the sciences to which I was drawn very strongly, especially psychology and biology. Still further on, by this time a graduate student, I heard about mediums and psychical research and the idea that life after death was something, an existence with which you could come to terms—you could do something about it. (pp. 9–10)

In the same address, Dr. Rhine reviewed the early work with mediumship at the Duke Parapsychology Laboratory, primarily experiments with Eileen Garrett, the famous medium whose work with the Parapsychology Laboratory was financed by one of her own chief supporters. But the anticipated evidence for communication with disincarnate personalities who had survived death was defeated by Mrs. Garrett's also scoring well on ESP tests that presumably required only her own psi powers, not the help of any incorporeal personal agency (IPA). Dr. Rhine (1976–1977) said: "We decided to put the question of Survival on the shelf as most every science has to do with its problems in earlier stages of its research, waiting for a time when methods are further developed" (p. 13). He wrote the chief supporter who had financed Mrs. Garrett, asking for funds to develop the basic methods needed if mediumship were to be thoroughly tested. As he succinctly put it: "She gave us the time but no more money" (p. 13).

Dr. Rhine was clearly not discouraged, however, about the possibility of re-opening the shelved question of survival. In 1975 he had indicated that he shared with William Perry Bentley an interest in investigating possible mediumistic evidence for survival (Rhine, 1974–1975, pp. 9–10). Rhine (1976–1977) suggested investing another fifty years of research on the survival question, adding: "I don't see how it could lose in bringing an answer to this question [of survival], whatever that answer is to be" (p. 15). He proposed two lines of parapsychological research that he felt might be fruitful in dealing with the question of survival (Rhine, 1976–1977, p. 14). These were (1) developing a tracer method that would "enable the subject to say not only what the message is, but where it came from . . . a test of assurance, a confidence test," and (2) an animal experiment that would make the question of survival a biological question "as it properly should have been long ago." The general outlines of the proposed animal experiment would "let us ask if there is something about an animal that as its vital processes decline and death approaches keeps right on going as if it weren't dependent on the vital processes." Dr. Rhine pointed out that it would not be necessary to bring the animal to the point of actual death to theoretically test the proposed

hypothesis. Even a demonstration that the simplest form of psi ability could function independently of the state of physical health would open the whole question of survival as a possibility.

Speculating on the possible findings of "what is really bedrock," Dr. Rhine (1976-1977, p. 19) said that he was leaning toward the view that the mind and body are a unity, "something like Spinoza's theory of the mind-body relationship," implying that "mind and body together make something that neither one of them possess" when separated. He labeled this possibility as like "the old idea of 'emergence.'" The terse, clear summary of his position (Rhine, 1976-1977) was the conclusion that "the scientists had the methods but not the problems and the theologians had the problems but not the methods" (p. 10).

J. B. Rhine published two papers explicitly dealing with the question of parapsychology and religion. The first (Rhine, 1975b) was virtually identical to the address given at Phillips University and primarily presents parallels between religious forms (as prayer) and various types of psi. These thoughts were expanded and elaborated in the second paper, that given at Southern Methodist University (Rhine, 1977-1978).

In the more expanded paper, Dr. Rhine discussed how parapsychology had already brought forward impressive evidence against the merely physicalist theory of man, thereby indirectly aiding the religious vision of mankind. He stressed again, as he had done in 1975, the similarity between religious forms and types of psi (Rhine, 1975b): PK (related possibly to omnipotence), ESP (related to omniscience), clairvoyance (possibly related to the "all-seeing eyes" of a divine being), precognition (similar to prophecy, or knowledge of things to come), and all the various forms of psi relating to religious "miracles." "Thus," said Rhine (1977-1978), "on the whole, the types of psi that have been quite independently outlined by laboratory research closely resemble the kinds of exchange that religious men have assumed in the theologies that arose out of human experience long before the laboratories of parapsychology began their work" (p. 6).

Conditions of psi experience (most notably relaxation, sleep, trance, and other dissociated conditions) were seen as similar to religious traditions about the state of consciousness appropriate for contacting a divine order, as through a sacred oracle. Speaking of a "common foundation" of psi and religious experience, Rhine (1977-1978) said:

Yet it now appears today that the chart of findings on psi communication fits rather closely into the pattern of interaction assumed in the major

religions of mankind. Indeed, no matter what one thinks about the theological claims of these religions, he can now at least see that their founders must have built those great cultural systems on a rather good acquaintance with the same powers that have now been independently established as parapsychical. (pp. 7-8)

Dr. Rhine then considered three questions from the point of view of the parapsychology of religion, seeing them as areas of probably fruitful future research:

*First*, is the question of whether a person can exercise some volitional control over his situation. Is he in any meaningful sense (and in even the slightest verifiable degree) a free moral agent—free, that is, of the substituent deterministic forces operating in and through his organism?

*Second*, is the problem of man's post-mortem destiny, whether or not death is the end of the personality as an individual agent.

*Third*, is the question of the kind of universe it is in which we live. Is it in any verifiable way a *personal universe*, with a type of intelligently purposive agency within it to which man can with rational confidence turn for helpful communication in the midst of the trying emergencies of life? (p. 9)

He clearly indicated that these important questions are much more than problems for parapsychology alone, but added: "It does now seem safe to say with some confidence that this branch [of science] can continue to make advances into problem areas of religion where the footprints of no other science have ever been left" (p. 9).

J. B. Rhine thus clearly saw the relevance of parapsychology to the questions and beliefs that had been carried since the emergence of human consciousness by the forms of religious belief and worship. Without lessening his dedication to the principles of science, he yet saw parapsychology as offering a way to possibly reconcile the unnatural split between mankind's greatest achievement, which is science, and mankind's greatest hopes and aspirations, which are still embodied in religious forms. J. B. Rhine thus was deeply concerned with the neglected field of natural theology, the concern that our study of the universe and our relation to it is also a way toward a sense of divine order that the great religions have believed on the authority of inspiration and revelation.

The evidence at this point is clearly inconclusive (Rhine, 1977-1978, pp. 21-22); there is enough evidence, however, to encourage parapsychology to pursue a deeper understanding of religious experiences and possibilities without sacrificing any of its dedication to the purest forms of science. The fact that neither the present community of science nor the present religious communities of faith sufficiently



appreciate the deep importance of parapsychology is most likely a passing historical misunderstanding. As science, natural theology, and persons with religious commitment explore the largest meanings of human experience they must all increasingly appreciate the field of parapsychology. They also then will appreciate the gentle, honest vision of J. B. Rhine, which will increasingly be seen as a pioneering attempt to find the true place of mankind in the encompassing mystery of the universe.

In his Gifford Lectures on Natural Theology at the University of Aberdeen in 1951-1952, Michael Polanyi began a reassessment of scientific meaning and practice which has spoken both to science and to an increasing number of theologians. Polanyi (1958) spoke of opening horizons of religious inquiry through the deepening of scientific knowledge: "The greater precision and more conscious flexibility of modern thought, shown by the new physics and the logico-philosophic movements of our age, may presently engender conceptual reforms which will renew and clarify, on the grounds of modern extra-religious experience, man's relation to God." Polanyi added: "An era of great religious discoveries may lie before us" (p. 285).

In other Gifford Lectures at Edinburgh in 1979, still another scientist, Sir John Eccles (1980), Nobel laureate and distinguished researcher in neuroscience, turned his gaze in a similar direction:

Man has lost his way ideologically in this age. . . . I think that science has gone too far in breaking down man's belief in his spiritual greatness . . . and has given him the belief that he is merely an insignificant animal that has arisen by chance and necessity in an insignificant planet lost in the great cosmic immensity. . . . We must realize the great unknowns in the material makeup and operation of our brains, in the relationship of brain to mind and in our creative imagination. (p. 251)

Sir Karl Popper and Eccles (1977) had previously worked out a dualist-interactionist model of mind-brain relationship, a model that opens the possibility of the "relative sort of dualism" that in 1945 J. B. Rhine spoke of as an implication of the findings of parapsychology.

Natural theology is the most focused area in which the work of J. B. Rhine may (I think *will*) influence religious thought through the gradual and responsible exploration of the possible meanings of the divine/human interaction as observable in the world as we explore it scientifically. If this is indeed a religious universe, there is no intrinsic reason that science should not demonstrate such qualities (Hall, 1977-1978b, p. 40); if it is *not* such a universe, that too should

ultimately come to awareness. In this scientific endeavor to find the meaning (if not the origins and limits) of mankind's place in the universe, a crucial factor is the faith that the community of science places on the integrity and reliability of individual scientists. The entire edifice of science ultimately rests on this basis of trust; and the trust of the community of science in parapsychology is built primarily on the foundation laid by the work of J. B. Rhine. In any future synthesis of science and religion, it is J. B. Rhine's unswerving loyalty to truth (Rhine, 1974, 1975a) and his "unquestioned integrity that has made parapsychology a respectable pursuit in the scientific world" (Hall, 1974-1975, p. 36).

Much remains to be done in parapsychology, and much additional work in other areas to begin a responsible investigation of the parapsychology of religion. Professor Frederick Streng (1977-1978, p. 32), a participant in the symposium at SMU, listed at least three things: a sensitivity to the assumptions now used to understand various forms of existence; the development of a vocabulary which allows mind, intuition, and will to play a role in the understanding of reality; and a continuing effort to accurately describe the phenomena of both parapsychology and religion, both in the laboratory and in ordinary life, and both in the past and the present. Albert Outler (1977-1978, p. 48) added the need for a "really fruitful awareness of the paradox . . . of the dialectic between the radical fact of mystery as the context in which we live and die and our insatiable hunger for intelligibility, verification, and scientific rigor." The mutual understanding of science and religion for the *human* origins and destinies of each field may point toward something that C. G. Jung anticipated: empirical indication of an ultimate unity of all existence, a goal Jung articulated through use of a term from medieval natural philosophy—the *unus mundus* (Von Franz, 1975, p. 247).

A significant honor was bestowed upon J. B. Rhine by the Society for Psychical Research, which had elected him to its presidency in the year of his death. In 1891 E. W. H. Myers, a founder of the SPR, reviewed a book that was then new, William James's *Varieties of Religious Experience*. Myers, (1891-1892) spoke of the important and impartial work that had been begun, words that are equally applicable to the work of J. B. Rhine on the parapsychology of religion:

. . . it will be found that we have mainly concerned ourselves with such questions as, while admitting of statistical or experimental treatment, do nevertheless promise to throw some light, one way or the other, upon those deeper controversies as to the existence or character of a spiritual

principle in man which have hitherto been mainly conducted on metaphysical than on empirical lines. In this task we have started—as I at least conceive our position—entirely without presupposition or prejudice. (p. 112)

The impartiality of Dr. Rhine in considering the significant questions which parapsychology reopens is shown no better than in his answer to a question concerning his own conclusion about his own personal existence beyond the point of bodily death (Rhine, 1976–1977). Speaking to the Texas Society for Psychical Research on Easter Sunday of 1977, he said:

As a matter of fact, I seldom ever thought about it [survival] in a personal way, and still less so since I quit conducting experiments with mediums. However, even in the days when we were working with mediumship, I never got deeply into the groove of thinking how this Spirit Survival might be because I didn't want to be carried away emotionally by such speculative thinking while the evidence was so inconclusive. (p. 16)

In closing the symposium on parapsychology and religion at Southern Methodist University in 1978, Dr. Albert Outler spoke words of thanks to J. B. Rhine that also are an appropriate ending to my own present remarks. Dr. Outler (1977–1978) first summarized the accomplishments and problems brought into clearer focus by the dialogue of J. B. Rhine with the theologians and scientists who participated in the symposium. Then turning to Dr. Rhine he said:

Most of all, and of course, we are *heartily thankful* to Dr. Rhine: for what he has contributed over the years, for his presence in our midst this week-end, for the inspiration he has given us for a continued search for truth—for that truth that lies within the perspective of science and rationality, but also that truth that reaches beyond those limits on out to those deeper and higher truths about human existence which hitherto have been too narrowly enclosed. For this and more, Dr. Rhine, we are, and will always be, in your grateful debt! (p. 51)

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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<sup>1</sup> 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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<sup>1</sup> 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).<sup>2</sup> 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 hocery and special pleading for the truth and the glory of animal magnetism. Hare, after his

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<sup>2</sup> 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 pre-paradigm 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.<sup>3</sup> 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).

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<sup>3</sup> 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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