

HISTORICAL PERSPECTIVE AND PARAPSYCHOLOGY¹

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ABSTRACT: Two historians of science who are working on a history of parapsychology discuss their approach to their study and the types of historical questions they are interested in answering. Their training and recent trends in the historiography of science enable them, they believe, to treat the history of parapsychology as the history of more orthodox sciences would be treated; that is, with relatively little concern about ultimate judgments of "truth" and with the attempt to see parapsychology as an integral part of the general scientific culture of its period.

As historians, they are interested in ascertaining what the background was to the major modern synthesis of experimental parapsychology, the work of J. B. Rhine at Duke. What were the continuities of this work with earlier attempts at experimentation? What were the discontinuities and how did they arise? How was Rhine's work received? In their interviews with parapsychologists they have often found that the pattern of the development that they perceive on the basis of studying published papers and archival material strikes the parapsychologist, initially at least, with surprise. This they attribute to the fact that details of early development in this field, as in other scientific disciplines, have often been suppressed or conventionalized in the light of subsequent developments. Specific examples of these differences of perspective are given.

In this brief article we mean to suggest something of the differences between the way in which the historian—and, in particular, the historian of science—sees the field of parapsychology and the way in which the parapsychologist himself sees it. For, as we have found in the several years during which we have been preparing a history of parapsychology, the historian tends to look for things which from the working scientist's point of view are totally unexpected. It may seem surprising that someone caught up in scientific research can be so close to his own problems that he will have no idea that, twenty or thirty years hence, historians will be raising questions that to him now must seem quite irrelevant, or at best peripheral. We hope to show why this is so, and to alert para-

¹ This paper was delivered at the Southeastern Regional Parapsychological Association Conference, Chapel Hill, N. C., on January 7, 1974, and is a considerably enlarged version of talks given earlier at the Erasmus Club, Duke University, and at the Psychological Research Foundation, Durham, N. C.

psychologists to the classes of material that a historian of the subject is likely to find important.

I

We should first underscore the fact that we have both been trained as historians of *science*. The goal of this professional training has been to teach us to look analytically at the internal development of a scientific tradition and at its external relations with other groups—the scientific community, for example, or society at large. It is this same approach to the history of science that we try to pass on to our own undergraduate and graduate students. From this standpoint, it seems to us perfectly natural to study closely the history of what we may call “experimental parapsychology”—that is, the attempts to place the study of psychical phenomena in a laboratory situation, where controlled conditions and quantitative techniques can be applied. We have been working for some time on a study that begins by examining the establishment of parapsychological research at Duke about 1930 within the context of the psychology department that William McDougall then headed; it will go on to examine in some detail the elaboration of research in this country and abroad during the 1930's through World War II. We mean to treat the extension of experimentation from the original phenomena of telepathy and clairvoyance to the domains of psychokinesis and precognition, as well as the attempts to correlate psychological data with success or failure at “extra-sensory perception,” as J. B. Rhine denoted it in his first book. During these same years, of course, the Duke work was “received” by the academic and professional communities, giving rise to much discussion—often quite heated—over its use of experimental and mathematical methodologies, and these disputes will form a central part of our study. Finally, we shall be concerned to follow the attempts to institutionalize and professionalize the field of psychical research—attempts which are still being carried on and which still run into much of the same sort of hostility as in the 1930's, although there may be signs of some change in general attitude now. It seems to us at the moment that all these strands that we will be tracing out come together in the late 1940's. By this time the major lines of

"classical" experimental procedure and domains had been formed; the principal independent corroboratory experiments had been carried out (e.g., in England, 1941-43); the initial spate of reaction and discussion had taken place; the acceptance by at least a few professional psychologists who went on to do their own research had occurred; and what to us at the moment seems like a plateau in the institutionalization and professionalization of the field had been reached. So we plan to pause in our study there (ca. 1947) temporarily, although we have every hope of eventually treating the subsequent quarter-century of parapsychology in another study when historical perspective becomes possible—when we can see how to ask historical rather than scientific questions.

Undoubtedly the best example of a question which turns out in the course of time to be of scientific but not of historical interest is: Do psi phenomena exist? Over and over again, university colleagues who misunderstand our motives have asked us: Do you "believe" in ESP? Belief or disbelief in the subject can certainly produce historical literature of a sort; most of the literature on the field, including what historical literature there is, has been motivated by a concern either to prove or disprove ESP.² But we have consistently tried to exclude this as a topic of concern, precisely because of our training in the history of science. For this training consists in reorientating one's conception of scientific development to get away from the "truth content" viewpoint. That is, we have been trained to look at the science of an earlier period, *not* from the retrospective position of later science, not to select out the "right" guesses and ignore the "wrong" ones, but rather, trained to consider earlier science comprehensively and for its own sake. Thus we can treat the fourteenth-century development of Aristotelian physics seriously, as possessing an internal history of its own, without any concern whatsoever as to whether Aristotle's laws of motion were "right." The application to parapsychology is, we hope, obvious. We see no historical merit in either defending or discrediting ESP; we feel that the history of the attempts to isolate,

² For example, C. E. M. Hansel's *ESP: A Scientific Evaluation* (New York: Scribner's, 1966). A welcome exception to this generalization is Alan Gauld's *The Founders of Psychical Research* (London: Routledge and Kegan Paul, 1968), which treats the first years of the British Society for Psychical Research.

control, and replicate psychical phenomena has an interest and an internal logic similar to that of the history of any other scientific development, regardless of what may eventually prove to be the "truth."

But it is not merely the intrinsic interest of the history of parapsychology that has led us as historians of science to take it up. What we are after is not simply a chronology of books and discoveries and techniques—Zener cards, Basil Shackleton and precognition, Gertrude Schmeidler and the sheep-goat distinction, and so forth. We feel that the history of parapsychology may have scope for illuminating the more general nature of the "mainstream" science which was and is contemporary with the development of parapsychology. For what we are dealing with here, fundamentally, is the attempt by an unorthodox field to become scientific and break into scientific respectability. Once again, the recent historiography of science has provided the orientation that we believe is valuable. We refer here to those studies which have taken seriously the impact of peripheral, pseudo-, or even anti-scientific movements on the development of mainstream science. The two principal foci for these studies have been Renaissance occultism and hermeticism, and early nineteenth-century German *Naturphilosophie*. Whereas earlier generations of historians of science had ignored or condemned these movements, more recent historians have come to sensitize themselves to the influence of these movements on the recognized scientists of these two periods. The general assumption now current is that one cannot have a comprehensive and balanced picture of the science of any period by focusing only upon its positive achievements (again, "positive" only in retrospect). It is impossible, now, to treat Kepler or even Newton as merely the discoverer of certain laws of celestial mechanics: both are imbued with "occult" tendencies that guided their work.

What sorts of questions is the historian of science likely to ask himself, then, about the history of parapsychology that he would ask about any field? One might be this: What sort of men, what sort of scientific personalities if you will, choose to devote their careers to this particular field? What are their approaches to science? Their world-views? It is a truism, at least to those familiar with the subject, that some of the most eminent scientists of the

late nineteenth and twentieth centuries have been interested in psychical research—Oliver Lodge, William Crookes, Pierre Janet, Charles Richet, and Hans Driesch, to name some. To separate this interest off from their better-known scientific work as mere “aberration” is surely to beg the question of why they were interested in this subject, and what relation their interest bore to their more “mainstream” work. Similarly for the full-time researchers in the field: to label them frauds on the one hand or prophets on the other is just as surely to beg the interesting questions about their personalities and motives for trying to make this subject scientific.

Another set of questions deals with the reception of the field. Why do so many scientists react so vehemently against the very *right* to pursue the investigation of psychical phenomena? Against the very suggestion that parapsychology might be a valid subject? What do these reactions have to do with the scientists' own image of what science is, where it is going, and what impels the scientific enterprise? In the period of acute controversy—the mid- and late 1930's—what was the pattern of reaction? Were the psychologists, for example, significantly *more* hostile to this field than other scientists—or other academicians? Was there a difference in the ways in which different schools of psychology reacted to parapsychological research? It is questions like this that make us feel our study has wider application than simply to parapsychology, and which convince us that the peculiar perspective of the historian of science is of value in looking at this field.

II

There is another level at which the perspective of the historian proves to be quite different from that of the working scientist, one which can best be understood in the light of some specific historical problem. Let us then sketch out as background the substance of an article we expect to be publishing soon concerning the relationship of J. B. Rhine's initial book of 1934, *Extra-Sensory Perception*, to previous work in experimental psychical research. There is, of course, a very considerable earlier history to experimental parapsychology. As far back as the 1880's members of the then newly-formed British S.P.R. were attempting

many of the same sorts of things that Rhine and his colleagues were to carry out at Duke: the reduction of the study of psychic phenomena to a controlled situation of card-guessing (rather than simply the collection of psychical experiential anecdotes), the use of statistical methods *very* early (1884) to quantify the degree of extrachance results, and the distinction between different types of psychical phenomena—in particular, telepathy and clairvoyance. In addition, these same men were concerned to identify the active and passive roles of those involved in an experiment: that is, did the agent send out beams of signals that were passively received by the subject, or did the subject in some manner actively “scan” his psychic target? In terms of more general scientific context, the investigators of the 1880’s and 1890’s were particularly sensitive to the relation this type of phenomenon might bear to the physical, biological, and psychological sciences. Did thought-transference involve some sort of radiant energy transfer (as Crookes, after the discovery of X-rays, tried to hypothesize), or did it seem to go against normal modes of transfer of energy? What was the evolutionary significance of psychical abilities? Were they some kind of pre-linguistic atavism, or did they indicate some kind of nascent higher evolutionary step? What was the relation of psychical abilities to that array of strange mental states in which pre-Freudian psychologists took so much interest: hypnosis, hysteria, multiple personality? Such questions recurred again and again and were still very much alive in England, France, and America in the 1920’s and early 1930’s, even though they were not the dominant problems concerning the field.

What we have argued is that the Duke work of the early 1930’s brought to bear a sustained concentration upon one aspect of psychical research, the experimental aspect. Without actually inventing anything radically new, without introducing concepts that couldn’t be found somewhere in the earlier literature or raising questions that had not previously been discussed, Rhine did devise an elegantly simple method of experimentation and statistical evaluation of extrachance results for studying ESP through card-guessing. The work at Duke was far more sustained and comprehensive than anything done previously in experimental psychical research; the results were published in an ordered and systematic form, with plenty of

open questions for further research; and, most importantly, Rhine claimed extraordinary success in discovering and nurturing good subjects. It is our contention that, in a phrase, Rhine created a paradigm for experimental psychical research, and that a large part of the success of Rhine's work among other researchers was due to the fact that—rather than introducing any shocking innovations—it built upon and synthesized the very real if sporadic tradition of this sort of experimentation which predated his work by half a century.

We have discussed at some length one particular subject on which after forty years some sort of historical perspective is possible, so as to illustrate against that background the kinds of issues with which the historian but not the parapsychologist is likely to be concerned. Let us take up three in some detail. First, what we may call the *discontinuities* of history. To someone working in a scientific discipline, it invariably seems that the work he is doing is an immediate outgrowth of the past, that the intellectual history of his work has been continuous—as of course properly it *has* been. But the historian, making judgments, categories, about qualitative variation in history, must look for breaks—moments where in retrospect it becomes clear that the nature of the science changed decisively, transforming the nature of what followed. When the break is big enough, scientists themselves recognize it: already in 1945 the English parapsychologist Whately Carington was recognizing that “the modern era of experimental parapsychology began in 1934,”⁸ mirroring our own conclusions, and men like Gardner Murphy and Robert Thouless come to much the same judgment in recent books, speaking of the “novel” or “pioneering” or “revolutionary” nature of Rhine's first work. Bear in mind, however, that in our analysis, while Rhine's book is a watershed paradigm, it is not truly “novel” or “pioneering” or “revolutionary”—the historian may well see the historical discontinuity differently from the parapsychologist. Furthermore, there are often historical discontinuities of quite a minor nature that the historian will perceive but that will never matter to future parapsychologists, and that therefore go unnoticed by them. Such a one is the shift that took place in the minds

⁸ Carington, W. *Telepathy*. London: Methuen, 1945. P. 20.

of English and French psychical researchers from taking the *agent* in thought-transference as the active element, to the *percipient*. Once they began to think of the percipient as active rather than passive, sometime between 1915 and 1925, work like Rhine's became much easier to plan out or to accept when met with. Here, then, is another important break in the history of the science—but one that only the past-minded historian, not the present-minded parapsychologist, is likely to identify.

The second problem area that we will take up is the *genesis* of historical events: the problem of the roots of discovery, in particular. To a researcher caught up in the movement of events, a discovery may seem spontaneous (Kekule's discovery of the benzene formula came to him in a dream) or inevitable (as physicists have regularly taken [wrongly] Einstein's special theory of relativity to be a natural outgrowth of the Michelson-Morley experiment). The historian, however, by profession, tends to distrust such simple answers and to examine much more broadly the intellectual context of the event for impinging factors that could have helped give rise to it. Thus, to move to parapsychology: the English parapsychologist S. G. Soal attributed to Whately Carington the impulse that made him look for evidence of displacement in Basil Shackleton's test scores (in 1939; this was the first independent English confirmation of Rhine's work).⁴ This is all very well, but we would like to see if there might not be additional factors involved that could have made Soal willing to follow up such an idea at that time. He had already had, after all, some direct experience of the possibility of precognition. To give another example: from our perspective, the most interesting figure in the British S.P.R. in the 1920's is a woman named Ina Jephson, who in 1928 published in their proceedings a long account of a series of card-guessing experiments she had performed.⁵ She used a statistical technique devised especially for her by the statistician R. A. Fisher⁶ in evaluating her results, which were above chance

⁴ Soal, S. G., & Bateman, F. *Modern Experiments in Telepathy*. London: Faber & Faber, 1955. Pp. 123-24.

⁵ Jephson, Ina. Evidence for clairvoyance in card-guessing. *Proceedings of the Society for Psychical Research*, 1928-29, **38**, 233-71.

⁶ Fisher, R. A. A method of scoring coincidences in tests with playing cards. *Proceedings of the Society for Psychical Research*, 1924, **34**, 181-85.

but exhibited a decline curve that she commented upon. In a great many respects Miss Jephson's work suggests the techniques and conclusions that characterize the Duke work of the 1930's, and yet there is general agreement among parapsychologists today that her work did not make much impression upon the field. We cannot be content merely to ignore her work, or mention it only in a footnote; instead, we feel compelled to try to fit her work and the reception it received into the pattern of intellectual genesis that we are working out. It may seem perverse to give so much attention to an apparently unimportant development, but such a stubbornness can be immensely fruitful, even though it often involves us in raising questions that the parapsychologist is convinced are irrelevant. One final example: we find very interesting the attention that parapsychologists gave to the history of science in the 1920's. Gardner Murphy's *Historical Introduction to Modern Psychology*, for example, was first published in 1929. And when J. B. Rhine came down to Duke and began to teach, the first course he gave was one in the history of science. We find these facts interesting because we know of our own experience that research or teaching in this field will reflect a man's developing understanding of what science is or can be. Consequently, we are curious to know, for example, what books Rhine's students read in the history of science, and what topics he chose to lecture on. Now it might seem understandable to a parapsychologist that we would want to know what *psychology* courses he taught—but the intellectual connection between his teaching the history of science and the subsequent paradigm for parapsychology is probably *not* something that would occur to the parapsychologist.

The final problem we may call the area of *fact* or *truth*. A scientific tradition inevitably stresses the cumulative nature of the model of reality: earlier discoveries are taught as self-evident truths, and the initial difficulties standing in the way of their acceptance as "truth" are lost to sight. A practicing parapsychologist may take for granted, treat as facts, certain conclusions which in earlier days did not seem factual at all. For example: it seems clear to us from scraps of evidence that the review of Rhine's book of 1934 delivered to the S.P.R. stirred up a wide and violent debate *within* the Society as to whether there could be as high a proportion of individuals with

psychical powers in society as Rhine claimed to have found. We feel quite confident that we can explain this skepticism: English psychical research had never attained this sort of success in 1935; it took pride in its amateur status and was somewhat mistrustful or suspicious of men who would do this kind of research full time, for a living; and it was somewhat upset at the thought that an important contribution of this kind might come out of *America*. But the point we want to stress is this—there is today no memory at all among the participants that there was this deep a division, this intense a discussion of *Extra-Sensory Perception*—and we have asked Robert Thouless, who gave that review to the Society in 1935. Here is an example of how a scientific tradition inevitably suppresses the difficulties that a "fact" has in getting established as factual, and how the historian must look for evidence of events that the parapsychologist cannot suspect ever took place. Comparable to a degree is the role of spiritualism in the background that produced the work at Duke and the present-day experimental tradition. A small percentage of people may perhaps be aware that the possibility of post-mortem survival was important in the genesis of the Duke work, but not that it was of fundamental importance, really the directing motive in some sense. The experimental-scientific tradition, as it developed in the 1930's and 1940's, found the survival question irrelevant (or unanswerable) and therefore forgot its antecedent. Even Rhine's initial publication of 1934 fails to suggest the extreme influence of the survival question. So here is another case where the historian must recover a habit of mind of which the practicing parapsychologist may now be oblivious.

III

We might conclude by saying something about how the historian's different perspective on parapsychology affects the course of his research, particularly since he must confront the practicing scientist about his past. It is a commonplace about science that "90% of all scientists who ever lived are alive today"—but surely 99% of all experimental parapsychologists are alive today. This means that in studying this field we are not limited to written materials, letters, and published papers, and that we can carry on as well what is

known as "oral history"; that is, we can speak with the participants themselves about their work and its context. Obviously, this can be very rewarding, since conversations of this sort can provide insight into the circumstances of scientific work that could not come from published papers. In the summer of 1972 we spoke in London with Mrs. K. M. Goldney, who took part with S. G. Soal in the experiments of 1941 with Basil Shackleton on precognition, and we made some casual remark about the somewhat sporadic nature of the experimentation. With some amusement, she reminded us that that had been wartime, and described the ways in which bombing raids over the city and the curtailment of public transportation had made it difficult for individuals engaged full-time in war work (as she was) to find the opportunity for parapsychological experiments. This, of course, was something which simply had not occurred to us.

On the other hand, "oral history" inevitably is a little frustrating, precisely because of the difference between the perspective of the historian and that of the parapsychologist with whom he is talking. Events which seem to us to be of importance in retrospect turn out to have left no impression at all on the minds of the participants; no amount of interviewing can make the event more understandable. Let us offer one last example. Gardner Murphy has spoken with us at some length, answering our questions about his career in the 1920's. One of the things we were very interested to hear more about was a period of several weeks in the summer of 1929 which he has described as having been spent with Rene Warcollier, at that time the leading and perhaps only figure committed to *experimental* psychical research in France: "There in Brittany, at the old town of Pont-Aven, we discussed for hours at a time, in the huge orchard, the questions which Janet, Grasset, Myers, and others had raised regarding the subconscious, and the implications of Henri Bergson's philosophy for the problem of transcending time and space. During a part of this visit, S. G. Soal joined us from Great Britain and eagerly participated in the discussion of experimental and quantitative methods in telepathic research."⁷ Soal (who of course later went on to verify Rhine's techniques) was at this time deeply engaged in an ambitious attempt to replicate Ina Jephson's

⁷ Warcollier, Rene. *Mind to Mind*. New York: Creative Age Press, 1948. P. xvi.

card-guessing work. *Why* this meeting interested us should now be obvious—here were three of the most important experimental parapsychologists representing three different national traditions discussing the substance and structure of their field in the years just before Rhine's *Extra-Sensory Perception* drew it together. What particular issues arose in their discussion? How did they feel about the validity of quantitative methods in research? Did they feel that experimentation should address itself to the demonstration of psychical phenomena, or to trying to delineate the circumstances that give rise to them? These and similar questions fascinate us, who are trying to understand the direction taken by experimentation in the 1930's; but Dr. Murphy was able to say nothing more specific about the details of the discussion. This happens to be an episode which from his perspective turned out to be relatively unimportant.

Of course, this argument cuts both ways. Just as it may be difficult for the working scientist to immediately appreciate the perspective of the historian, it is not always clear to the historian what attitude the scientist feels towards historical research. In our enthusiasm for our work—which deals, after all, with contemporary history—we have spoken impersonally and analytically about the work of many active parapsychologists without at first appreciating the discomfiting feeling this might produce—a feeling, as one put it, of being a frog on a dissecting table. We are now more sensitive to this point, even though it seems to us inescapable. It is only our concern to present an objective account of our subject that leads us to speak in this way, and we would like to extend our warmest thanks to the many people who have discussed the past fifty years of parapsychology with us. From their contributions we have learned just how illuminating (to mix a metaphor) a sharing of perspectives can be.

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