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CONTROVERSY AND THE *JP*

BY JOHN PALMER

One of the few uncontroversial statements one can make about parapsychology is that parapsychology *is* controversial. One forum where scientific controversies play themselves out is the journals, and the *Journal of Parapsychology* (or the *JP*, as it is known to insiders) is no exception. Since its origin in 1937, the *JP* has been host to numerous controversies, some major and some minor, some involving battles with outside critics, and some strictly homegrown. These controversies have often been provocative, and sometimes they have even been enlightening. They all have contributed to the history of our field.

The most prominent vehicle for controversies within the *JP* has been the section for letters to the editor. However, some of the more important controversies have appeared as series of papers in the main part of the journal. The recent Hyman-Honorton controversy consumed virtually an entire issue. Another vehicle for controversy, particularly in recent years, has been book reviews, which often evoke letters from irate authors who feel their works have been misrepresented or misunderstood by the reviewer.

This golden anniversary of the *JP* is an appropriate time to present a brief historical overview of the controversies that have appeared in the *JP* over the years. I shall not refer to each and every controversy, but I shall try to cover all the major ones and enough of the minor ones to give a flavor of the range of topics and personalities involved.

In writing about these controversies it is not my intention to contribute to them. Therefore, I shall do my best to discuss them evenhandedly, matter-of-factly, and to leave my own prejudices out of it. Also, given the limited space I can devote to each controversy, it is unlikely that I shall be able to capture all the nuances that may be important to those who actually participated in the controversies. Finally, I recognize that some of these controversies have extended beyond the *JP*, and that not all facts relevant to them have been published in the *JP*. Since the purpose of the paper is to chronicle the role of the *Journal* in these controversies, no attempt will be made to include such information.

Experimental Work

The publication policy of the *JP* in some of its formative years (1939–1941) provided a built-in vehicle for controversy. As Dr. Broughton has noted in his paper (this issue of the *JP*), experimental articles were submitted to a "Board of Review," whose critiques were actually published along with responses by the authors. The research reports that these exchanges focused on were extremely complex and involved intricate statistical analyses. Indeed, the reviewers sometimes complained that the reports were hard to follow. This complexity also characterized the exchanges themselves, and thus they cannot be reviewed substantively here. The most extensive of the exchanges concerned the drawing experiments of Whately Carington (Carington, 1942a; Sells, 1940).[1] Others concerned J. B. Rhine's experiments on precognition (J. B. Rhine, 1941a; Sells, 1941a) and salience effects (J. B. Rhine, 1941b; Sells, 1941b), and Stuart's experiment on new analyses of a clairvoyance experiment (Sells, 1941c; Stuart, 1941). Rhine was obviously not pleased with the Board's critiques of his work and at one point openly complained about the poor quality of some of the reviews (J. B. Rhine, 1941b).

The most heavily criticized experiment of this period was not, however, one of the experiments considered by the Board. This distinction went to an experiment by James MacFarland designed to demonstrate an experimenter effect by comparing a subject's calls to different target decks, one handled and scored by a supposedly psi-facilitating experimenter and the other by a supposedly psi-inhibiting experimenter. As expected, results were much better with the former than with the latter.

This experiment was attacked by two outside critics. Willy Feller (1940) used it as an example to illustrate the inadequacy of shuffling as a randomization method. John Kennedy (1939) made the embarrassing revelation that there were discrepancies in the subjects' calls as recorded by the two experimenters and went on to suggest a recording-error hypothesis that could account for the effects in the study.

Interestingly, MacFarland did not defend himself against either critique but was defended by senior staff at Rhine's laboratory. Greenwood and Stuart (1940) attempted to refute Feller's critique by noting that similarities in sequences of *targets* in two decks of

[1]To keep the reference section at a somewhat reasonable length, I generally have not cited the articles or books that prompted the exchanges reviewed. References to these sources can of course be found in the exchanges themselves.

cards does not necessarily imply similarities in sequences of the *cards* themselves. Stuart (1940), who took on Kennedy alone, was forced to concede that MacFarland's psi-facilitating experimenter made motivated recording errors, but these errors were not sufficient to account for the results. (A similar strategy was used several decades later by Tart and Dronek [1983] in defending Tart's ESP learning studies.) Stuart also attempted to demonstrate that Kennedy's recording-error hypothesis was logically flawed and its application based on faulty analysis.

As noted previously, these early controversies tended to focus on statistical issues. These issues were sometimes dealt with in the abstract as well as in the context of particular experiments. The statistical consultants to the *JP*, Joseph Greenwood and Thomas Greville, played a prominent role in these exchanges. Although a wide array of statistical issues were addressed, ranging from pooling of data to independence of trials, the most extensively debated topics proved to be optional stopping and suppression of negative results. The optional stopping issue was first raised by Vernon Lemmon (1939), who contributed a paper to a symposium on ESP methods sponsored by the Southern Society for Philosophy and Psychology. The exchange was subsequently published in the *JP*. Lemmon attempted to argue that stopping experimental subunits at optimal points could bias the results, whereas Greville (1939, 1940) maintained that only the total number of trials in the experiment need be considered. Unfortunately, Lemmon offered his paper in absentia and was not present to respond to Greville. However, his argument was later picked up in the *JP* by Feller (1940), who also maintained (without evidence) that only positive results were included in the overall analysis of ESP card-guessing data published in *Extrasensory Perception After Sixty Years* (Rhine et al., 1940/1966). In responding to Feller, Greenwood and Stuart (1940) defended the data base by noting, among other things, that 170,000,000 chance trials would be necessary to reduce it to nonsignificance. Readers familiar with the present-day applications of meta-analysis will quickly identify the relevance of Greenwood's argument to modern strategies, for example, those by Honorton (1985) in his defense of the ganzfeld data base. Finally, an article by R. and S. Bugelski (1940), which demonstrated how data selection could yield spurious significance, evoked an angry retort from Carington (1942b), who resented the implication that data selection had actually occurred.

A statistical issue that gradually emerged over the years was the appropriateness of the *CR* method itself when compared with parametric statistical tests. It first cropped up in an exchange between

Betty Humphrey (Humphrey & J. B. Rhine, 1944) and S. G. Soal (Soal & Goldney, 1944) concerning whether a theoretical or empirical variance estimate should be used in evaluating Soal's data for position effects. Robert Thouless touched upon the issue in suggesting the use of ANOVA to evaluate Schmeidler's sheep-goat experiments (Thouless, 1958, 1959; also see Greville, 1959) and an experiment by Nash (Thouless, 1961). In the latter case, Thouless's suggestion was challenged by the Editors of the *JP*, who published a reanalysis of Nash's data using a chi-square method. However, the matter was not attacked directly until the late 1970s, when an extended exchange appeared in the *JP*. Greenwood and Greville (1979a, 1979b) argued that parametric statistical tests are inappropriate for psi data because such tests assume a distribution of psi the existence of which is unknown owing to the unreliability of psi test data. Donald Burdick (1979), who is presently the statistical editor of the *JP*, challenged this thesis, pointing out that we never know in practice that data come from an underlying distribution. James Kennedy (1980) echoed Burdick's point and suggested that following Greenwood's prescription would stifle research. Finally, Dick Bierman and Brian Millar (1980) suggested that appreciation of psi-based experimenter effects might serve to reconcile the two positions.

Perhaps the most prominent controversy to achieve representation in the *JP* was the one concerning suggestions of fraud in the research of Rhine and Soal. It began with a paper in *Science* by George Price, who used Hume's essay on miracles as a basis for suggesting that the fraud interpretation of psi data should be preferred on a priori grounds. He went on to suggest that a committee dominated by hostile skeptics should be set up to undertake a fraud-proof experiment that would settle the matter once and for all.

A large portion of the December 1955 issue of the *JP* was devoted to this matter (J. B. Rhine et al., 1955). The Editors published an abstract of Price's article along with abstracts of correspondence published in a subsequent issue of *Science*. These included replies by Rhine and Soal, along with comments by the eminent positivistic philosopher P. W. Bridgman and the not-so-positivistic scholars Paul Meehl and Michael Scriven. The Editors then published, in full, letters that were either submitted to *Science* but not published or submitted directly to the *JP*. (It is not indicated which were which.) Rhine added an introduction and postscript to the exchanges.

Rhine took a rather upbeat tack, noting that Price had served to bring parapsychology to public attention and had conceded the inadequacy of the earlier methodological and statistical criticisms. Soal used the opportunity to respond to Price's detailed speculations about how cheating could have occurred in his experiments, and he suggested that achieving replicability would be more likely to settle the controversy than the illusory "fraud-proof" experiment. Bridgman said no conclusions could be drawn until lawful regularities were uncovered. Meehl and Scriven attacked the validity of Price's appeal to a priori arguments, noting that if Price's committee ever affirmed psi in its experiment, it would be preferable to assume the committee lied, according to Price's logic. (Price's reply to this comment was to suggest that the committee be enlarged!)

All but one of the letters published in full in the *JP* were from psi proponents. They were long and quite hostile to Price. The one short letter was from a psychologist, Kendon Smith, who identified himself as a skeptic. He nonetheless characterized Price's paper as "combin[ing] obvious misinformation and apparent malice" (J. B. Rhine et al., 1955, p. 266).

As we all know, the Price controversy did not end the matter. In the June 1961 issue, the *JP* published two articles by C. E. M. Hansel addressing respectively the Pearce-Pratt and the Pratt-Woodruff experiments, each article followed by a rebuttal from the experimenters. Hansel adopted Price's philosophical position while offering specific hypotheses about how fraud could have occurred in the two experiments. Briefly, regarding the Pearce-Pratt experiment, Hansel (1961a) suggested that Pearce could have cheated by surreptitiously observing the recording of the target orders during checkup and changing his calls accordingly. Rhine and Pratt (1961) countered that this was precluded in one series in which two experimenters were present in the room where the recording took place. (The arguments about the lab layout did not come until later.)

In the Pratt-Woodruff matching experiment, Hansel (1961b) suggested that Woodruff (the junior experimenter) could have cheated by misplacing target cards in the wrong piles, provided he knew the order of the key cards. Pratt and Woodruff (1961) replied. The debate centered to a large extent on whether the key cards had been sufficiently randomized to preclude Hansel's hypothesis. Hansel provided internal evidence from the data of the highest scoring subject to support his hypothesis, but Pratt and Woodruff interpreted this as a psi effect. In a rekindling of the debate more than a decade later, George Medhurst and Christopher Scott (1974) provided data supporting Hansel's hypothesis for the four other high-scoring subjects, which, they contended, weakened a psi interpretation. Pratt (1974) raised questions about their analyses and continued to defend the psi interpretation.

Michael Scriven (1961), who it will be recalled was among those who attacked the article by Price, was less warmly received by parapsychologists when he used the occasion of a PA banquet address to suggest that parapsychology was flirting with extinction and, in particular, that Hansel's attacks raised legitimate questions about the experiments he criticized. The address was published in the *JP* (which was the practice from 1961 to 1964), and it evoked a number of critical letters from parapsychologists. Among them was Woodruff (1961), who chastized Scriven for confusing criticism with propaganda and failing to do his "homework." Scriven (1962a) stood his ground, but he did reiterate that he thought the parapsychologists' rebuttals met Hansel's criticisms *on balance.* The overall status of the evidence for psi was challenged again in the *JP* a decade later by John Beloff, who stressed the lack of replicability and theory rather than fraud. The context was an exchange of letters with Rhine, who maintained that replicability was an unfair demand because of the unconscious nature of psi (Beloff & Rhine, 1973).

The next fraud controversy I could find in the *JP* had a markedly different character. Ramakrishna Rao (1964a) used a review of a book discussing research by the Indian investigator H. N. Banerjee to bring to light anomalies in data from some of Banerjee's experiments that could easily be construed as suggesting fraud. Banerjee (1964) defended himself with support from Gaither Pratt (1964), who observed some of the experiments in question, and from Ian Stevenson (1964). Although Pratt and Stevenson dissociated themselves from the fraud interpretation of Banerjee's studies, their letters focused primarily on the alleged impropriety of Rao's perceived accusations of fraud and his not sharing his review with Banerjee and Pratt prior to its publication. Rao (1964b) defended both his conduct and the validity of his comments concerning the experiments. He denied actually accusing Banerjee of fraud, but he said he felt a responsibility to bring the facts to light so readers could draw their own conclusions, especially since the research had been sponsored by Rhine's laboratory.

PK experiments received their share of attention beginning in the 1950s. Robert McConnell (1958) resorted to the *JP* to reply to psychologist L. G. Humphreys's critique of his dice experiments because the *Journal of Experimental Psychology*, where the research was originally published, refused to publish his reply in their pages. Carroll Nash (1956, 1960, 1962) and Haakon Forwald (1956, 1960) engaged in an extended debate primarily devoted to the nature of the physical forces that might be involved in the latter's PK placement experiments. In recent years, less orthodox PK experiments came to be represented—and criticized—in the pages of the *JP*. When Robert Brier published a plant PK experiment based on the work of Cleve Backster, letters followed by Harold Cahn (1970) and by Rex Stanford and Gaither Pratt (1970) that were strongly critical of Brier's methodology and statistics. Brier's (1970a, 1970b) replies were generally conciliatory. More recently, George Hansen (1982a, 1982b), W. E. Cox (1982), and John Richards (1982) debated the validity of macro-PK effects allegedly produced in a spiritualistic context by a group identifying itself by the code name SORRAT.

Process-oriented ESP research received some critical attention after the 1970s, but this generally was restricted to studies that seemed to provide strong psi effects apart from the independent variables. Charles Tart admitted presenting the results from his learning ESP experiments in a way that would be maximally provocative, but he may have been more provocative than he intended. Although the extended controversy over this work appeared primarily in the *Journal of the American Society for Psychical Research*, a *JP* review of Tart's book by Dennis O'Brien (1976) did evoke a rather lengthy response from Tart (1976). The debate focused on the status of Tart's data as evidence for learning.

A more protracted exchange in the *JP* occurred between Irvin Child (1977, 1978) and Hans and Shulamith Kreitler (1977) over the latters' experiments on ESP in the context of subliminal perception. One of the findings reported by the Kreitlers was that letters of the alphabet detected relatively infrequently when presented subliminally to subjects *without* an agent sending them were perceived more frequently when presented *with* an agent sending them. The controversy was devoted primarily to the validity of Child's assertion that this finding is attributable to statistical regression artifact.

Last but not least in this section is the recent and very elaborate exchange between Ray Hyman (1985) and Charles Honorton (1985) concerning the evidentiality of ESP ganzfeld research. This is the first time since the Hansel-Pratt exchanges of the early 1960s that a prominent outside critic has debated a parapsychologist in the pages of the *JP*. Unlike most previous exchanges, this one has focused on a group of experiments rather than on one individual experiment, and the application of the newly popular meta-analytical techniques has played an important role. Both the statistical significance of the aggregate data base and the attributability of whatever significance there may be to procedural flaws have been addressed. The debate is scheduled to continue in forthcoming issues.

Spontaneous Cases

The controversies devoted to spontaneous cases have focused primarily on the different philosophies about the role of such cases in parapsychology as represented by Louisa Rhine on the one hand and those who favored a more traditional approach on the other. To L. E. Rhine, spontaneous cases are not evidential in their own right; they serve primarily as sources of hypotheses for experiments and to help elucidate the processes by which psi experiences are cognitively mediated. To the traditionalists, good spontaneous cases can be as evidential as good experiments, and, moreover, they can provide evidence relevant to other important issues such as the survival question.

L. E. Rhine's first debating partner was the sociologist Hornell Hart (1957a, 1957b, 1958), who responded to each of her three papers published in the 1950s in which she presented analyses of her case collections. A major point she made in her papers was that the percipient, not the agent, seemed to play the active role in telepathy and that there was no evidence from such cases to support the survival hypothesis. Hart's first letter was extremely polite, but his tone became increasingly strident as the debate progressed. He complained, in particular, that L. E. Rhine had failed to consider concepts and data from other investigators (including himself) that contradicted her thesis. L. E. Rhine's (1957a, 1957b, 1958) responses to Hart were quite brief, which may have contributed to his irritation. She considered Hart's hypotheses speculative and irrelevant to her study. Alluding to the lack of evidential value of spontaneous cases generally, she maintained that she was not drawing conclusions herself, but Hart challenged this.

In the 1970s, Ian Stevenson took up where Hart left off, again responding to a review of her own work that Rhine had just published. This debate focused on the issue of sampling. Stevenson (1970) claimed that Rhine's cases were not as representative as she implied, because people would only be likely to submit cases that fit in with what they perceived to be her interests. The possible role of the agent, he said, would not clearly emerge because most such cases were submitted by percipients. Finally, many of the cases were likely to be nonevidential and one cannot assume that the nonevidential cases would be randomly distributed among the various categories. L. E. Rhine (1970) argued that the role of the agent was prevalent in early case collections because the investigators focused on them and may have found them easier to corroborate. She attributed the lack of active-agent cases in her collection to cultural and generational differences. Although she never made this point explicitly, it is my impression that in both her exchanges with Hart and with Stevenson she appealed primarily to the parsimony principle in support of her active-percipient theory of spontaneous psi: i.e., this theory can account for all the cases, whereas the active-agent theory cannot account for clairvoyance cases or cases where the agent was clearly passive.

An earlier exchange between Rhine and Stevenson concerned L. E. Rhine's (1966) review of Stevenson's book *Twenty Cases Suggestive of Reincarnation.* Rhine considered the cases anecdotal, claimed they could be explained by ESP, and that the birthmark cases assumed Lamarckianism. Stevenson (1967) defended the evidentiality of his cases, noting that the ESP hypothesis failed to account for the selectivity of the perceived information and that only a handful of the birthmark cases occurred within the same family, the only context in which the Lamarckian thesis could apply.

L. E. Rhine was not the only person against whom Stevenson needed to defend his attitudes toward spontaneous cases in the *JP*. Such cases were one of the targets of Michael Scriven's (1961) PA banquet address, discussed earlier. An exchange of letters ensued concerning Scriven's claim that spontaneous cases could be attributed to chance coincidence (Scriven, 1962b; Stevenson, 1962a, 1962b).

Philosophy and Sociology

The metaphysical implications of psi were always important to J. B. Rhine, and discussions of these implications were frequently represented in the pages of the *JP*. This matter was approached most directly in a "symposium" on the "physicality of psi" (Scriven, Broad, Pratt, & Burt, 1961), which really consisted of papers invited by the Editors. The debate continued in the correspondence section. The principal combatants turned out to be Scriven and Pratt, Scriven maintaining that psi was indeed physical and Pratt maintaining that it might be nonphysical. The issue seemed to boil down to whether the definition of physics should be restricted to present-day physics and what is the best tactical approach in relating to other scientists.

The issue reemerged 22 years later in the form of a debate between Gerd Hövelmann (1983a) and I. W. Mabbett (1983) over the proper definition of the term *paranormal.* Hövelmann suggested that to define it as unexplainable by science implied an unacceptably metaphysical orientation to the subject; he preferred a more neutral definition, in effect equating *paranormal* with *anomalous.* Mabbett replied that he agreed with the spirit of Hövelmann's remarks but noted that *paranormal* events may well be explained by some future science.

Some subsequent debates concerned attempts to give substance to a physical approach to psi. The esteemed Dutch physicist J. M. J. Kooy addressed the problem in terms of our conceptions of space and time, but his views were challenged by the Indian philosopher C. T. K. Chari (1958; Kooy, 1958). Sixteen years later Chari (1974a, 1974b) again took on the role of critic, this time challenging a theory by physicist M. Ruderfer (1974), which purported to explain psi in terms of neutrinos and (in the case of precognition) tachyons.

A related issue is the implication of precognition for our notions of cause and effect. A long-standing debate on this matter between the philosophers Antony Flew (1953) and C. W. K. Mundle (1952) achieved representation in the *JP.* According to Flew, the proposition that an effect can precede its cause is a logical contradiction, whereas Mundle maintained that Flew's approach begs the question. (The debate was also entered by John Whittlesey [1953], who made the point that quantum mechanics provides for time reversal, thus anticipating the observational theories that emerged two decades later.) When Bob Brier published his book proposing a philosophical basis for precognition, Flew (1975) attacked his thesis in a review. John Beloff (1975) came to Brier's defense, although Brier did not respond himself.

An issue of particular importance to J. B. Rhine was the ontological status of the subspecies of ESP—particularly telepathy and clairvoyance. His view was that while the existence of clairvoyance had been established by the experimental research, the status of telepathy was unresolved and likely to remain unresolved given the currently available methods. Following the publication of a paper expressing these views, a number of parapsychologists, all but one from Britain, wrote dissenting letters to him. In a subsequent article in the *JP*, J. B. Rhine (1946) excerpted, integrated, and responded to these letters. The main arguments by the British dissenters were that clairvoyance was logically inconceivable and that evidence for true telepathy was provided by mediumship research and the Soal-Shackleton experiments. Rhine countered that saying clairvoyance is presently incomprehensible is nothing more than saying it is paranormal, and that the alleged evidence for telepathy does not rule out other explanations.

A number of years later one of these British parapsychologists, Robert Thouless, resumed the debate with Rhine (J. B. Rhine & Thouless, 1972; Thouless, 1973, 1974). Thouless made the point that we can consider telepathy to have been established if we define it operationally rather than metaphysically. As for mind-to-mind communication, he contended that it was a metaphysical issue that could not be resolved by science in principle. The irony here is that his position was in fact *more* extreme than that of Rhine, who maintained that mind-to-mind communication might be verified at some later date.

Thouless also stood up for survival research, which Rhine considered another "bad-risk problem" unresolvable by current methods. Thouless put forth his cipher test as capable of providing good if not perfect evidence for survival, whereas Rhine, who seemed to feel only conclusive evidence was worthwhile, pointed out alternative interpretations.

Since the publication of the seminal writings of Thomas Kuhn, philosophy of science has sometimes come to have a sociological flavor. When Gerd Hövelmann warned parapsychologists not to jump on the Kuhnian bandwagon, he found himself embroiled in a controversy with Trevor Pinch over the accuracy of his representation of Kuhn's position (Hövelmann, 1984; Pinch, 1984). In the same spirit, Hövelmann (1983b) questioned the appropriateness of Charles Tart's paper suggesting that some critics may be motivated by fear of psi. Tart (1983) said he was not trying to be ad hominem, just encourage research.

Book Reviews

Finally, a few words should be included about controversies engendered by book reviews. Perhaps the most extensive of these to appear in the *JP* concerned the anthology *Psychic Exploration.* Several writers (Honorton, 1975; Mitchell, 1975; Rogo, 1975) complained that Pratt's review of the book focused too heavily on the weak contributions at the expense of the stronger ones. Pratt, however, seemed more concerned with the fact that, in his view, Edgar Mitchell should not have been listed as the author of the book (Pratt, 1975; White, 1975).

In recent years, spice has been added to the *JP* by the book reviews of Douglas Stokes, who approaches his task with a critical eye and a sharp pen. Not surprisingly, he frequently evokes responses from wounded authors (e.g., Irwin, 1981; Stokes, 1981a, 1981b). Frequently, the main issue in these exchanges has not been the *merit* of what the author said but what it in fact *was* that the author said. Such clarifications of positions are of course an important function of all controversies and may show that the combatants agree after all!

Conclusion

We have seen that the *JP* has played a prominent role in promoting exchanges by leading parapsychologists with each other and with scholars from other disciplines. Whether they adequately resolved the items they debated notwithstanding, that there were debates at all has sensitized readers to important issues and allowed them to draw their own conclusions. Controversy is a necessary and healthy part of the scientific and scholarly process. Let the debates continue!

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Institute for Parapsychology
Box 6847, College Station
Durham, NC 27708