COMPLICATING THE CONVERSATION: RHETORIC, SUBSTANCE, AND CONTROVERSY IN PARAPSYCHOLOGY

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In my last presidential address presented in New York City in August of 2001 (Zingrone, 2002), I talked about the lessons that can be learned from science studies and its examination of controversy in mainstream science, across the boundaries of scientific disciplines, across the boundary between mainstream and marginal science, and within parapsychology itself. In a sense this address is a continuation of that one. Instead of speaking more generally on science studies and parapsychology, I will be narrowing the focus to the rhetoric of science and to the ESP controversy that both consumed and constituted the American parapsychological community from the publication of J. B. Rhine's monograph *Extra-sensory Perception* (*ESP*) in 1934 to the publication of *Extrasensory Perception After Sixty Years* (*ESP-60*) in 1940 by J. Gaither Pratt and his colleagues. Although this particular controversy occurred more than 60 years ago, we all know that our phenomena and methodology are still misrepresented and maligned and that our community still remains under attack at the margins of mainstream science.

The main thesis of this address, then, is that relevant lessons can be learned from a re-examination of that decade of controversy. To illustrate this point, I will discuss some of the rhetorical choices embodied in, and speculate on the possible impact of these choices on the reception of, these two documents.

Privileging the Text

Before heading into an illustration of my points, it is necessary to describe what I mean by rhetoric. In the preface to the second edition of his seminal work, *The Rhetoric of Science*, Alan Gross (1996) noted that his book was meant to "alter the state of the question. To create a disciplinary space…" (p. viii) through which aspects of scientific text such as style and structure might be examined for what they can tell us about science itself. Gross's brand of rhetoric of science was based on Perelman and Olbrechts-Tyteca's recasting of classical rhetoric in their treatise on argumentation published in 1971. To illustrate what a "reconfigured" (p.

[1] This paper was presented as the Presidential Address at the Annual Convention of the Parapsychological Association in Vienna, Austria, in August of 2004.

xxi) rhetoric might contribute to the discursive terrain of science studies, Gross reviewed, among other studies, Boyd's (1979) survey of the use of metaphor in scientific theory, a topic that Carl Williams and Diane Dutton (1998) explored in parapsychological texts. Gross's own research examined the persuasive presentation of science and how the rhetoric of published scientific texts are related to day-to-day science practice. To do this, he compared Boyle's records of his experiments to the published framing of "Boyle's law" (pp. 85-91), the content of Charles Darwin's private *Red Notebooks* to the text of his *The Origin of Species* (pp. 95-100), and Einstein's laboratory notes to his published work on relativity (pp. 92-96). Gross concluded that published scientific papers instantiated a myth of logically developed scientific progress modeled on Baconian induction, in contrast to the much more complex trajectory from experiment to theory and from theory to experiment that was revealed in scientists' personal work records.[2]

In 1991 Peter Dear edited an anthology called *The Literary Structure of Scientific Argument*. Among the studies of rhetoric in science included in the volume were: T. H. Browman's (1991) review of the growth of the scientific journal as a genre, Peter Dear's (1991) own examination of the repackaging of "anecdotes and experiments" into coherent scientific reports in the seventeenth century, and B. J. Hunt's (1991) treatment of the impact of referees on the construction of scientific articles. Done by historians of science rather than rhetoricians, the chapters in Dear's anthology had similar goals to those outlined by Gross in that they attempted to show how scientific practice came to be packaged in its current narrative forms. Dear described the volume as the product of the "literary turn" in intellectual history which was sparked in part by the reinvigoration of rhetoric that was embodied in the Perelmann and Olbrechts-Tyteca (1971) text that also inspired Alan Gross.

Scholars in communications and science studies have also focused on the development and interdependence of conventions in scientific reporting and science practice. Examples of these two approaches are, respectively, Charles Bazerman's (1988, 1995) analysis of changes in style and content of articles published in the *Philosophical Transactions of the Royal Society* between 1665 and 1800, and Bruno Latour and Steven Woolgar's (1979) study of laboratory life that Peter Dear (1991) described as paying

> devoted attention to the role of "literary inscription" in the practical creation of scientific knowledge and the

[2] His findings have been supported by historians of science who have also found this disjuncture between the written depiction of a scientific work in published documents and the underlying laboratory records. See, for example, Holmes, 1987, 1991.

> way in which scientific papers, through particular textual strategies, process knowledge-claims by integrating them in stronger or weaker fashion with already accepted bodies of knowledge. (p. 2)

For the purposes of my examination of Rhine's *ESP* and Pratt et al.'s *ESP-60,* I have happily co-opted a methodology that rhetorician of science Alan Gross, science writer Joseph Harmon, and historian of science Michael Reidy developed in their 2002 book *Communicating Science: The Scientific Article from the 17th Century to the Present.* Covering scientific articles published in English, French, and German, Gross, Harmon, and Reidy conceived of their project as proceeding in "three acts: the creation of arguments for and against knowledge claims about the natural world, the artful deployment of these arguments in a text, and their representation in the syntax and semantics of natural languages" (p. vii). Adopting a version of Darwin's notion of "natural selection," Gross, Harmon, and Reidy attempted to determine how and why the modern scientific article had evolved.

After having digested three centuries of scientific writing in three languages—a project that took 10 years to complete—the authors concluded that "the current scientific article is, on the whole, an accurate reflection of the world as science conceives it, an effective means of securing the claims of science, and an efficient medium for communicating the knowledge science creates" (p. ix). That is:

> Translated into evolutionary language, selection pressures favor a style that represents science as an objective enterprise … and produces stronger, more flexible argumentative strategies. These result in either a gradual or continuous change in some feature over time—as in the general decline of personal pronouns and [in the] corresponding rise of passive voice [in scientific articles]…. (p. 259)

In the remainder of this address, as we move through the two primary documents of the ESP controversy, Rhine's *Extra-sensory Perception,* published in 1934, and Pratt, Rhine, Smith, Stuart and Greenwood's *Extrasensory Perception After Sixty Years,* published in 1940, you will see that I have drawn from these various traditions to comment on the conformance of two of our central texts to some of the existing norms of scientific writing. I will also speculate briefly on the impact of the rhetorical choices these books embodied on the controversy that ensued.

Rhine's *Extra-Sensory Perception*

For those of you who do not know Rhine's (1934) monograph, *Extra-sensory Perception,* the volume was put together by Rhine 7 years after his arrival at Duke University in 1927 and nearly 4 years after the start of the research program that would become experimental parapsychology. The volume served to introduce readers to the methodology Rhine's team had developed, the results they had obtained, and their sense of how these results fit into the scientific worldview.

As I will argue in a moment, the rhetorical choices Rhine made in the monograph were nonstandard for a scientific treatise in his time. But for our community, *ESP* is still essential reading, not only for the sense of the moment that it conveys, but also to remind us—as reading the pages of the early SPR proceedings often does—that the best ideas have been germinating since the beginning.

The monograph was divided into three parts. The first part included Rhine's characterization of the phenomena as "radical" (pp. 3-15), not only because he felt they violated physical laws in general but also because they suggested the direct agency of mind in the world in particular. Although he developed a classification schema into which he hoped all the known physical and mental phenomena of psychical research could be parsed, he limited the work of his own group to telepathy and clairvoyance, that is, to extrasensory perception as it could be attributed to the influence or intention of a living being. That such phenomena existed was not in doubt for Rhine, and to support his belief, he provided a deep introduction to the literature of psychical research that included spontaneous case collections, mediumship studies, field investigations, and early experiments. But, he noted, more was needed because unnamed skeptics remained who "… demand[ed] some measure of experimental manipulation and even some artificial control of the phenomena in question before they venture credence" (p. 20). Thus, Rhine's job, as he saw it, was to provide that experimental evidence, to focus on confirming the reality of extrasensory perception in the laboratory, and to discover lawful relationships between relevant variables and the ability of experimental subjects to produce the phenomena under test conditions.

Because the theoretical work that preceded Rhine did not impress him, however, he felt that it was his "job" to develop a clear theory that included the distinction between telepathy and clairvoyance. In addition, he wanted to improve the methods of quantitative analysis that had predated him, an intention that required his team both to develop methodology that lent itself to quantification and to invent new metrics by which the presence of telepathy and clairvoyance could be detected reliably in experimental results.

Having set out his goals, Rhine then plunged into what was essentially a lyrical, autobiographical description of the development of the research program at Duke from the early 1930s to the publication of *ESP*. He described collaborations with Duke psychology department members on early problems and in early tests.[3] He wrote about testing his children

[3] See Mauskopf & McVaugh (1980) for a clear picture of how that collaboration flowered and then wilted as Rhine's visibility on campus and in the wider world increased.

and their neighborhood friends, any undergraduate who volunteered, and laboratory members including himself. Also depicted was the trial and error development of the ESP cards, the discovery of "star" subjects, and the enlisting of members of the mathematics department to aid in the development of statistical methodology.

Although Rhine claimed that by the winter of 1931 the methodology his group used had standardized into what they considered a rigorous protocol, his decision to report his results on a subject-by-subject basis meant that precious few methodological details were in fact presented in his monograph. When available, they were unsystematically presented, spread over several chapters with differing emphases in a style of reporting that belied Rhine's claim of standardization.

One unusually in-depth description Rhine wrote of an informal series of practice tests was to haunt the Rhine team over the entire decade that followed as critics repeatedly assumed that formal studies had been conducted in the same way:

> … we have followed the policy of giving a new subject a preliminary test, the results not to be taken into the record no matter what they are. When the subject gets 3 hits in 10 or better, the record can be started on the next trial following but must be so designated at the time. If, during the performance for record, the score drops below 6 in 25, it is legitimate to quit scoring for the time. These preliminary test data have been rejected. My estimate of them, from memory and my own experience, is that they were on the whole above chance average anyhow, and probably represent only a few hundred trials with those subjects who later came into good scoring. But there have been a few subjects who have "practiced" for thousands of trials without getting above the chance expectation (np). No conclusion of this report would be changed or appreciably weakened by including these practice data. For that matter, no amount of failing to score above chance by any number of other individuals can seriously affect our judgement of the results of those who succeed, since an individual ability is in question. (pp. 76-77)

Another description that Rhine may have intended as a charming glimpse into the day-to-day life of the harried researcher was equally unfortunate in its influence on the reception of the monograph:

> I have finally a number of scraps of data for record that do not fit in anywhere. Some of them are very good and some are poor. I cannot be sure, of course, that to-morrow or

> next year I shall not find a sheet of data stuck away absent-mindedly in a book I was reading or holding at the time … [but Rhine stressed] I am fully confident that there is no batch of forgotten and unreported data that would alter the final "anti-chance" value (*D/pe*) by so much as half a unit. (p. 77)

Rhine's presentation of the formal results obtained by his team under varying conditions—which Rhine considered to be his laboratory's best work—was followed by what some critics felt were freestyle psychological musings on the personalities, habits, successes, and failures of the subjects, ranging from descriptions of their late nights revels and their lovers' quarrels to comments on their love of music or art and their outgoing natures.[4]

A number of statistical tables were included that summarized the data of individuals or of groups of individuals. But each table was customized to the chapter in which it appeared, having its own focus and structure and including different levels of information about methodology and results, thus making it difficult for readers to feel confident that they understood the overall results.

The final part of the monograph provided an overarching discussion of the findings and their meaning in which Rhine dealt with five alternate hypotheses that he felt could be offered as explanations. They were: chance (pp. 145-147), fraud (pp. 147-149), incompetence (pp. 149-150), unconscious sensory perception (pp. 150-153), and rational inference (pp. 153-155). Rhine rejected all of these as having been ruled out by methodological constraints or statistical procedures. Once having eliminated the counterexplanations, at least in his own mind, Rhine felt sanguine about claiming: "For those, then, who can accept proof before explanation is arrived at (i.e., for the scientifically mature) ESP is a natural fact and principle, puzzling as its explanation may be" (p.155).

By including the qualifying phrase "for the scientifically mature," Rhine in effect insulted the unconvinced in advance of some rather bold speculations on the "nature and functioning" of extrasensory perception, followed by even bolder speculations on the interface of his research with other disciplines, among which he included physics, physiology, psychology, biology, and, paradoxically, psychical research. Two appendices completed the volume, one a list of hints for successful experimentation and the other

[4] It is interesting that, at least on the surface, some of the attributes of Rhine's star subjects fit quite well with modern experimental results which suggest that participants who have a family history of psychic experiences, claim their own experiences, and are extraverted and artistic can be expected to do well in ESP tests. But without any objective measures of these personality traits or states or without the details of the personal experiences claimed by his high-scoring subjects, and without knowing what the characteristics were of the rejected subjects in Rhine's period of testing, it is impossible to tell if this apparent goodness-of-fit to modern findings is coincidental or not.

introducing a table of significances that new experimenters might use to evaluate their data.

The Reception of *Extra-Sensory Perception*

Establishing Credibility

The chair of Duke University's psychology department, William McDougall, penned the introduction to the volume. Mention was made that Rhine's research had been conducted in a psychology department and that his assistants and subjects were largely drawn from that department. Rhine himself claimed in his introduction that parapsychology was a branch of psychology and he also claimed—at least initially—that his intended audience was psychologists. It was reasonable to assume, then, that at least some psychologists who read *ESP* expected the text to be grounded in psychology. But was *ESP* a work of psychology?

In his 1996 book, *The Scientific Voice*, Scott Montgomery made the point that citation use in scientific writing establishes a sense of community (p. 39). Given Rhine's claims about the place of parapsychology in psychology, one could have expected that "the trail of citations" (Gross, 1996, p. 36) would lead from the psychological context in which Rhine worked to the content of *ESP*.

Instead *ESP* was situated by its trail of citations squarely within psychical research. Of the 87 citations made in the volume, 82 were to articles or books that dealt with aspects of psychical research, only a few of these having been published in the general academic or psychological literature. Three citations were references to statistical textbooks. The only strictly psychological reference was to McDougall's *Outline of Abnormal Psychology*, published in 1926. One other psychology-related citation referred to Carl Murchison's (1930) compilation in which Pierre Janet's autobiography appeared (p. 125). Clearly Rhine's own work was built almost entirely on psychical research. There was a disjuncture, then, between his claims of parapsychology's place in psychology and the literature on which he relied. Strictly psychological references could reasonably have been expected. Their absence was most definitely noted.

Even at this remove, it is not difficult to think of literatures extant at the time that were relevant to Rhine's work. Among those mentioned by his critics was the community of ideas literature (Willoughby, 1935). Other literature in print at the time that I believe could have been relevant includes: discussions of the use of the probable error to evaluate performance (e.g., Edgerton & Paterson, 1926), the examination of the acquisition and loss of learned skills over time (e.g., Drury, 1930), and the literature on the influence of drugs (e.g., Cattell, 1930) and of being observed (e.g., Burri, 1931) on the performance of mental tasks. None of this literature was cited by Rhine, however.

In fact, nothing about Rhine's text except his declaration that parapsychology was a branch of psychology and his superficial discussion of the psychological characteristics of his subjects *actually* tied the monograph to the field. There is some evidence that this may have complicated the reception of Rhine's monograph in general and of his speculative chapters in particular. For example, in his review, R. R. Willoughby (1935) argued that, until the research itself could be considered credible, "… we shall not regard the concoction of hypotheses of the mechanism of ESP as a profitable investment of energy" (p. 207).

Psychical researchers, on the other hand, recognized the materials Rhine drew on, and many shared his view that the phenomena had been shown to exist by the literature that preceded him. For them, then, whatever the document's other failings, at least it lay squarely within their own territory. In fact, psychical researchers on both sides of the Atlantic received the monograph with great enthusiasm (e.g., Murphy, 1934; Thouless, 1935).

The reception of *ESP* was also complicated by a lack of conformance to the conventions of scientific writing in use at the time. Charles Bazerman, Scott Montgomery, Alan Gross, Joseph Harmon, and Michael Reidy, among others, all found a progression from the personal and subjective to the abstract and "objective" in science writing from the seventeenth century to the twentieth. The typical seventeenth-century report—in which the social standing of witnesses was as important as the description of the methodology—gradually evolved into the typical twentieth-century report in which scientists attempted to establish the "presence" of nature as "the only real agent … a reality independent of its linguistic formation" (Gross, 1996, p. 17).

The achievement of this "abstraction" came through a variety of structural and stylistic changes. One of the most important of these was the evocation of what Montgomery (1996) has called the "death of self" (p. 21) in which the scientist strives to be a blank slate upon which nature writes its facts. The act of conducting research, rhetoricians have noted, is itself highly personal, an engrossing activity that the scientist both shapes and experiences. But when research is written up, there is a sense that credibility can not be evoked in the reader unless there has been a "banishment of one's personal experience," unless, in the narrative, the narrator—the scientist-as-person—"is lost" (p. 31).

One way to test this movement toward the banishment of personal experience is to quantify the use of "I," "my," "we," "our," and proper names in the text. Using this method, analysts have shown that "the literary nullification of the self" was well under way by the end of the eighteenth century and well established by the beginning of the twentieth century (p. 106).

Gross, Reidy, and Harmon (2002) found, for example, a continuous drop in the use of personal pronouns in scientific texts. The trend stabilized at less than one instance of personal pronouns per 100 words of text during the period from 1901 to 1925. That is, from the seventeenth century forward, scientific language had "evolved" to distance the scientist from "nature" in the narrative through a paring away of the personal from the report. As this happened, the authors claimed, there was a concomitant increase in the social credibility of the report and the efficiency with which the scientific content was conveyed. Scientists who returned to the personalized style of writing more typical of the eighteenth and early nineteenth century suffered the consequences: "[The] infusion of personal, descriptive style [came to be seen as] … 'bad' scientific prose, or in less pejorative terms, science on holiday," a style which is not only unpersuasive but which does not "communicat[e] … science effectively" (p. 167).

How does Rhine's prose in *ESP* fare when analyzed for its conformance to the conventions of science writing in the 1930s? Rather than being couched in a language that was data-driven, that promoted the "nullification of self," Rhine's text was what Montgomery (1996) would call "fervid" or "sermonizing" (p. 108). It was shot through with names and personal references, for example:

> We seldom ran over 20 trials per day per subject. Mr. McLarty did; as did also Mr. Mann. . . . Among this group were 100 trials by Dr. William McDougall . . . 150 by D. K. Adams . . . our greatest gain was the discovery of Cooper, who got 38 correct in 90 trials. . . . I must note that in these trials I did not myself supervise Cooper but asked another student, a friend of his, Mr. Harriman, to do it. Mr. Harriman, himself, got only 1 correct in 10, with the reverse arrangement. But if there were any doubt of Cooper's and Harriman's honesty, the further work of Cooper under supervision, reported later in this chapter, would adequately satisfy it. . . . (Rhine, 1934, p. 70)

A direct comparison of the text of *ESP* to the findings of Gross, Harmon, and Reidy (2002) underscores this departure from the rhetorical norm: Rhine's monograph averaged over three times the norm, that is, an average of 3.3 usages per 100 words.[5]

[5] Gross, Harmon, and Reidy (2002) sampled 10-line passages from over 500 articles drawn from highly cited journals published during the twentieth century (p. 241). The personal pronouns and names were counted and an average usage per 100 words was found. Using their method as a guide, I counted the total number of words on the first page of the monograph (Rhine, 1934, p. 3), and on every 10th page after that to page 221 in the conclusion section. In addition I counted the number of times personal pronouns or names were used on each of these pages. (Words appearing in footnotes or tables were not counted). Once that was completed I calculated the total number of words in my sample (7,153) and then the number of times personal pronouns or names were mentioned over all

Other conventions evolved over the modern history of scientific prose that signaled "abstraction." Among these were the ways in which methodological detail was reported and the positioning of specific structural elements in the texts. In their empirical study of scientific documents, Gross and his colleagues (2002) found that experimental, observational, and theoretical sections became separated from one another. While this evolution of type, content, and position of elements in a scientific report in the physical and natural sciences fluctuated from the seventeenth century to the early twentieth, by the second half of the twentieth century, the modern list of sections had standardized into the following: abstract, introduction, methods, results, discussions, and references (p. 189-190).

Charles Bazerman's (1988) examination of the "codification of structure" was conducted exclusively with psychological research reports. He placed a similar level of emphasis on the structure of articles, arguing that psychology had adopted the conventions used in the physical and biological sciences.

Conventions flowing from these traditions were defined first on experimental psychology and then influenced the development of structural prescriptions for all areas of psychology (pp. 257-277), spreading more slowly in psychology than in other literatures. As late as the 1920s, many psychology articles still followed what Bazerman identified as a nineteenth-century style: that is, they began with common everyday problems, and the resulting scientific examination read as "continuously reasoned arguments" written in a philosophical style. The audience for whom such early articles were intended also varied. Rather than being aimed always at a specialist audience, quite a number of articles published in the first two decades of the twentieth century were intended for "a wide range of people interested in the workings of the mind" (p. 268).

By the 1930s, the psychology article was becoming more standardized in structure. In psychology reports, however, the methods section became the position in the scientific report in which the researcher assured his audience that his experiment had been conducted properly and established the reliability and validity of the results. In the natural and physical sciences, the plausibility of the method-as-described as a proper vehicle for obtaining the results constructed the "factness" of the underlying natural phenomena and gave credibility to the idea that contact with "objective truth" was being displayed in a scientific report (Gross, Harmon, & Reidy, 2002). In psychology, on the other hand, the purpose of the methodology section was rather more personal; it was the credibility of the scientist and his or her ability to follow the rules that was at issue. Whatever the differences between these disciplines, method was,

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my sample pages (234). I calculated the average citation per 100 words by using the simple equivalence formula 234 over 7,153 is equivalent to X over 100, and solved for X. The average obtained was 3.3 per 100 words, with a range per sample page from 0 citations to 9.9 and a standard deviation of 2.6 citations per 100 words.

in both the natural and the social sciences, the vehicle by which science was communicated, with the scientist-competently-doing-method at the forefront in psychology and an objectified method-as-depersonalized-science-practice at the forefront in the physical sciences (Bazerman, 1988, pp. 274-275).

When the initial hypothesis was controversial, as Rhine's defense of extrasensory perception most surely was, an author needed to be more careful in conforming to the "rules." Gross and his colleagues (2002) have argued that such an author needed to be mindful of that which was potentially controversial in his or her report, taking care to justify such elements by "presenting and 'impeaching' any plausible weaknesses" that the reader might find in the report (pp. 207-208). Speculation needed to be argued in an exceedingly careful fashion, using inductive means that were classically Baconian, moving from the most conservative points that were "closest to the facts" to the points that were more conjectural (Gross, 1996, p. 96).

Whether Rhine was conversant with the structural elements that were necessary to make a persuasive scientific case in the natural and physical sciences, or whether he agreed with or opposed the evolving conventions in report writing in psychology—or the different requirements for potentially controversial research—is a matter for speculation. But Rhine's subject-based structure coupled with his heavily personal prose may well have undermined the persuasive potential of his message to a great extent, just as it may well have undermined the establishment of his competence as a scientist.

That the reception of Rhine's monograph was mixed at best is apparent in the 100-plus articles of criticism that were published in mainstream psychology journals and in the *Journal of Parapsychology* and elsewhere from 1935 to 1939. Some critics focused on the lack of methodological detail and the difficulty of reconstructing individual experiments. Rhine's group was dismissed by some as incompetent psychologists and by others as merely disconnected from mainstream psychology. The content of these and other criticisms suggest that the critical community of psychologists were at least to some extent susceptible to the deviations in Rhine's prose from what was expected.

Another element, which speaks more of interdisciplinarity than rhetoric, is the fact that most of Rhine's psychology-based critics set aside the psychical research literature review that began the monograph as absolutely immaterial to the task at hand. For them, the experimental testing of extrasensory perception began with Rhine. For others, *credible* experimental testing was yet to be done, awaiting the meeting of methodological and statistical criticisms. Where Rhine saw a trail of evidence over 50 years old, many of his colleagues in psychology saw a trail that was at best less than 10 years old or at worst less than 1 year old.

Rhine predicted the mixed reaction that the monograph and the research program it described would receive. In his introduction and in other writing, he indicated that he knew how unusual it was to conduct psychical research in a university context and how likely it was that his research would be met with criticism and disbelief. Given that awareness, it is a shame that Rhine chose to present the data in his monograph in such a nonstandard way. Not only was the problem under study controversial but the style in which it was presented was controversial as well.

Extrasensory Perception After Sixty Years

Over the course of the critical response to Rhine's monograph, the influence of two conventionally trained laboratory staff members became very apparent. They were Charles Stuart, who had come to the Rhine laboratory and the psychology department from the Duke mathematics department, and J. Gaither Pratt, who had been trained as a comparative psychologist. Both of these men became involved early on in the substantive debate over the mathematical assumptions that underlay Rhine's work, and over Rhine's methodology.

Both Stuart and Pratt had more luck than Rhine in publishing some of their rebuttals to critical articles in mainstream psychology journals, and both seemed more able to understand the necessity of rising to the methodological and mathematical challenges that were laid before them. Mathematicians Joseph Greenwood and later, T. N. E Greville also became regular contributors to the criticism that raged from 1934 to 1939, devising ways to solve the analytical problems that new methodologies posed. A number of the critics, whose exasperation with Rhine's own arguments could at times be almost palpable even in print, found Stuart and Pratt to be worthy colleagues.

Stuart and Pratt could be counted on to hear an argument and make a cogent counter-argument, not merely to reiterate that research had been carried out competently or that here were revolutionary results that needed to be addressed, as Rhine frequently did.

Louisa Rhine (1983) has said in her autobiography that as the critical period unfolded, especially after the peak year of 1938 when a number of stressful symposia and seminars were held at psychological conventions, word filtered back to the laboratory of public ridicule being heaped on Rhine's team by such dogmatic critics as B. F. Skinner and Hans Rogosin at conventions the Rhine group did not attend. The burden of criticism, she thought, poisoned the atmosphere at the laboratory. She claimed that J.B. often expressed nostalgia for the old days before the publication of *ESP*, when enthusiasm reigned. Stuart, Pratt, Greville, and Greenwood, on the other hand, seemed—at least in print—to be thriving on the debate. They seemed to have the sense that many scholars now have, that the period of criticism after the publication of Rhine's monograph was the most important period of methodological development in the history of parapsychology.

In 1939, a friend of Rhine's who worked at the publisher Henry Holt in New York City suggested that the team pull together a new monograph that would review all the original experiments, all the criticism, and all the response, and report on all the new experiments that had been devised to meet the criticisms. That book project, completed in 6 months of concerted effort by the entire Duke parapsychology team, resulted in what Charles Honorton (1993) once called "the central classic of experimental parapsychology" (p. 195), *Extrasensory Perception After Sixty Years* (Pratt, Rhine, Smith, Stuart, & Greenwood, 1940).

The Style and Structure of *ESP-60*

The structure of the volume may have ensured that *ESP-60* took this central position. Three elements especially may have led to its importance for future generations of researchers. The first was the six chapters devoted solely to a comprehensive review of substantive criticisms of ESP research, responses to those criticisms, further commentary by the most active critics, and responses to that commentary (pp. 70-242). The second was the inclusion of appendices devoted to statistical methods and to the comprehensive listing of the studies that were included, a listing which allowed for comparisons of methodology and results (pp. 363-420). The third was a glossary of terms.[6]

But was there a significant improvement in the rhetorical elements of *ESP-60* that helped establish the credibility of its authors, and the "factness" of the phenomena under study? The network of authority established by the "trail of citations" in *ESP-60* was only somewhat different from that of *ESP*. Of the 267 references cited, 230 were to psychical research or parapsychology articles. Although 51 citations were to articles published in psychology journals, 18 to statistics and mathematics journals and 13 to philosophy journals, all of these citations were to articles directly involved in the ESP controversy. The only exceptions were a few references to general problems in probability and statistical analysis that had relevance to other fields (e.g., Huntington, 1927, 1937, 1938). If knowledge or methods existed in psychology that could have impacted on the research that was being conducted at Rhine's laboratory—and there were as many relevant areas in general psychology in 1940 as there had been in 1934—they were not evident in the text. Clearly *ESP-60*, like *ESP*, was a contribution to the literatures of parapsychology and psychical research and not to psychology per se.

An additional problem that was somewhat unavoidable was that a significant number of citations were to popular magazines in which some aspect of the debate had been published. A description of the complications

6 Although this may seem to be an unimportant inclusion, Alvarado and I (Zingrone & Alvarado, 1988) have argued elsewhere that attempts to standardize terminology in parapsychology were important to the professionalization of the field.

caused by the enthusiastic endorsements of the science editors at *The New York Times*—especially their insistence that Rhine's work was psychology as usual—is beyond the scope of this paper. Equally, a description of the complications caused by Rhine's publication of a popular book on his team's research in 1937 and the media fanfare that accompanied its release is also beyond the scope of this paper. Suffice it to say, though, that the course of the controversy was very much influenced by the popular debate and not in a positive way.

Did *ESP-60,* which was largely team written initially and then worked into a cohesive whole by Gaither Pratt, differ in use of language from Rhine's monograph? To provide a partial answer, I examined the document for the presence of personal pronouns and personal names. Unlike *Extrasensory Perception,* which had more than three times the number of personal pronouns and proper names per 100 words than was standard in scientific prose in the period from 1926 to 1960, *Extrasensory Perception After Sixty Years* had averaged 1.78 instances per 100 words. When the analyses of the two volumes were compared using a Mann-Whitney U statistic, the difference was statistically significant (*ESP* Median = 2.68, *ESP-60* Median = 1.53, U = 600, $z = 2.65$, $p < .008$, two-tailed).

It can be said then, using this indicator, that the prose in *Extrasensory Perception After Sixty Years* clearly conformed more closely to the scientific norm in the distance it placed between the individual scientist and the "work" than Rhine's earlier writings had done. But it should be noted that while *ESP-60* was significantly less different from the rhetorical norm, the mean number of instances per 100 words was still nearly twice the number Gross, Harmon, and Reidy had found in the physical and natural sciences of the day. Thus, while there had been movement toward the norm in science writing in the style of *ESP-60* when *ESP* was taken as the starting point, the journey was far from complete.[7]

As for the structure of *ESP-60*, Gaither Pratt and his colleagues (Pratt et al., 1940) described their intent as follows:

> [T]he authors have attempted to condense into a reasonably compact form: (a) all the experimental and evaluative methods by which the research has been done and by which its adequacy must be judged; (b) all of the results obtained … grouped, classified, and analyzed so as to enable them to be assayed critically from the point of view of all possible alternatives; (c) a thorough digest of the criticism, both constructive and otherwise; and (d) all of these as they bear upon the clarified question about which the research is concerned, with as much an answer

[7] An indication of the remaining difference in prose was the assignment of only the less "polemical" chapters to Harvard undergraduates as required reading in the introductory classes in 1941 (Mauskopf & McVaugh, 1980, p. 357, note 62).

to that question as the assembled materials permit. (p. vii)

The contents and structure of *ESP-60* went a long way toward establishing the credibility of the research by emphasizing criticism and methodology, by describing results on a study by study basis and not as narratives centered on "star" subjects, by parsing the available experimental material by its perceived evidential quality, and by reporting on research in progress that incorporated the most reasonable of the criticisms raised. That the writing team saw fit to make determinations as to which criticisms were reasonable or not also indicated that a great deal of knowledge—both substantive and technical—had been gained over the course of the controversy. Pratt and his colleagues felt perfectly competent to judge for themselves which methodological prescriptions were necessary and sufficient to steer their science practice toward epistemic progress.

The decision to include the table of experiments was a particularly effective one as the results were presented in a single format with all of the available information about each study encapsulated in a standardized form, allowing for comparisons between studies and pooling across the entire study list. Equally effective was the inclusion of the appendices designed for advanced researchers, ensuring that the volume would not only have a life as the capstone of the ESP controversy but also as a reference work for future researchers. The presence of these appendices, even for the unconvinced, coupled with the willingness of the writing team to present and entertain further criticisms in a respectful way, also signaled a commitment to the give and take necessary for science to be "self-correcting."

The Reception of *ESP-60*

Citation, style, and structure all worked together to enhance the effectiveness of *ESP-60* as a potentially persuasive document in the scientific sense. Although a much deeper analysis of both *ESP* and *ESP-60* is possible, given the time constraints of this address, I wish to add only that whereas some have claimed that *ESP-60* was widely reviewed in the scientific press, the majority of the reviews were once again published in popular venues. The scientific reviews that did appear, however, even when written by authors who were largely unconvinced by the arguments, were respectful and careful, taking the method and the theory seriously. Henry Garrett's (1941) review in the *American Journal of Psychology* was an example of this. Garrett took the time to review the methodology and arguments in some detail, endeavoring to make the reader aware of the advances that had been made in control and evaluation since Rhine's (1934) original monograph.

The reception of *ESP-60* was quite different from the reception of *ESP*. The style and structure signaled—at least to some readers—that here was a competent team of scientists, evaluating carefully a progressive research program upon which a scientific future could reasonably be built. Whether or not its persuasiveness “caused” the ending of the ESP controversy soon after its publication is, however, beyond the scope of this address. Suffice it to say that the controversy did abate for slightly more than a decade after the publication of *ESP-60,* but archival research is needed to discover why that was. Were the critics merely tired of the argument? Because the controversy had shifted to the pages of the *Journal of Parapsychology*, was it just that psychologists and other scientists were no longer faced with the debate in their own literatures? Whatever the reason, from 1940 until 1955 when G. R. Price’s critique appeared in the pages of *Science*, the battle seemed to have been won.

The Rhetorical Choices We Make

Over the course of this address, I have examined briefly some of the rhetorical choices that were made by J. B. Rhine in his monograph and J. G. Pratt and his colleagues in *Extrasensory Perception After Sixty Years*. What kind of recommendations can the modern parapsychologist take away from this brief examination of some of the rhetorical choices made by Rhine in 1934 and by Rhine and his team in 1940?

First, do not attempt to orient a book or an article to one discipline while allowing its trail of citations to establish your credibility in another. By doing so you tell your intended reader that you are disinterested in the problems that energize them and that you have found nothing of value in their research. That is not a good foundation for a conversation.

Second, do not pay lip service to substantive criticisms by accepting them verbally but then setting them aside at the theoretical or speculative stage. I did not have time here to give examples of how Rhine repeatedly, in *ESP* and in the articles he wrote between 1934 and 1940, set aside experiments he agreed were of low quality and then slipped them back into his theorizing as if they were unproblematic. Quality matters. If you accept a criticism in print, make sure you remember that as you pen new reports.

Third, if you are serious about making scientific progress, work within the scientific community. Rhine’s mistake was not just failing to craft his 1934 monograph so that it was of interest to his intended audience, psychologists, but he also lost substantive opportunities by not being conversant with the relevant psychological literature of his time. Many of us still write as if parapsychology exists in a vacuum. It does not now, and it never did. There are findings, even if they underscore or emphasize conventional explanations, that are important to our work. It behooves us to know what they are.

Fourth, if you are serious about making scientific progress, do not complicate the conversation that you have with other scientific colleagues by encouraging, prompting, or engaging in public promotion of your research unless you are willing to live with the serious consequences this kind of straddling of the public/scientific divide can bring. Scientific credibility is inversely related to public fame in most cases. That may be unfortunate, but that is the way it is. Part of the heat generated in the debate over *ESP* flowed from the support given to it by the science editors of various New York newspapers. Even more heat was generated by the appearance of Rhine's popular book in 1937. The greater rhetorical success of *ESP-60* may have been due to the conservatism of such Rhine team members as Pratt, Stuart, and Greenwood regarding where they fought their substantive battles. Unlike Rhine, they kept their attention focused squarely on the scientific audience.

Fifth, always state the weaknesses of your work and anticipate criticism insofar as possible. Pay attention to past criticisms when you design or describe new research. Although parapsychologists today do this, it bears repeating. This willingness to make criticism a central component was one of the great strengths of *ESP-60*.

Finally, do not assume that silence after publication means acceptance or capitulation. Your opponents may turn away, as some of Rhine's may have done, out of fatigue or a shift of interest, or because they made a pragmatic decision that enough was enough. If you have not persuaded your opponents, you will be having the conversation again.

The bottom line is that rhetorical choices matter. In order to achieve closure in a scientific debate, there have to be substantive as well as epistemic gains. As scientists we want to come closer to a clearer understanding of the natural world. That is our job. But to do our job effectively, to make substantive gains, we must be able to talk to each other and to our critics. The rhetorical choices made in Rhine's monograph obscured the content, especially for the unconvinced. The rhetorical choices in *ESP-60*, on the other hand, underscored and emphasized the content, even for some of the unconvinced. By emulating the craft that went into *ESP-60*, even now, we can increase the probability that our message will be heard. That is a valuable goal.

References

Bazerman, C. (1988). *Shaping written knowledge: The genre and activity of the experimental article in science.* Madison: University of Wisconsin Press.

Bazerman, C. (1995). Reporting the experiment: The changing account of scientific doings in the philosophical transactions of the Royal Society, 1665-1800. In S. Jasonoff, G.E. Markle, J.C. Peterson & T. Pinch (Eds.), *Handbook of science and technology studies* (pp. 169-186). Thousand Oaks, CA: Sage Publications.

Boyd, R. (1979). Metaphor and theory change: What is a “metaphor” for? In A. Ortony (Ed.), *Metaphor and thought* (pp. 356-408). Cambridge: Cambridge University Press.

Browman, T.H. (1991). J.C. Reil and the "journalization" of physiology. In P. Dear (Ed.), *The literary structure of scientific argument: Historical studies* (pp. 13-42). Philadelphia: University of Pennsylvania Press.

Burri, C. (1931). The influence of an audience on recall. *Journal of Educational Psychology,* **22***,* 683-690.

Cattell, R.B. (1930). The effects of alcohol and caffeine on intelligent and associative performance. *British Journal of Medical Psychology,* **10***,* 20-33.

Dear, P. (Ed.). (1991a). *The literary structure of scientific argument: Historical studies.* Philadelphia: University of Pennsylvania Press.

Dear, P. (1991b). Narratives, anecdotes and experiments: Turning experience into science in the seventeenth century. In P. Dear (Ed.), *The literary structure of scientific argument: Historical studies* (pp. 135-164). Philadelphia: University of Pennsylvania Press.

Drury, S.M. (1930). Periods of arrested progress in the acquisition of skills. *British Journal of Psychology,* **1***,* 1-28.

Edgerton, H.A., & Paterson, D.G. (1926). Table of standard errors and probable errors for varying numbers of cases. *Journal of Applied Psychology,* **10***,* 378-391.

Garrett, H.J. (1941). Review of Pratt et al.'s *Extrasensory perception after sixty years. American Journal of Psychology,* **54***,* 449-453.

Gross, A.G. (1996). *The rhetoric of science.* (2nd ed.) Cambridge: Harvard University.

Gross, A.G., Harmon, J.E., & Reidy, M. (2002). *Communicating science: The scientific article from the 17th century to the present.* New York: Oxford University Press.

Holmes, F.L. (1987). Scientific writing and scientific discovery. *Isis,* **78***,* 220-235.

Holmes, F.L. (1991). Argument and narrative in scientific writing. In P. Dear (Ed.) *The Literary structure of scientific argument: Historical studies* (pp. 164-181). Philadelphia: University of Pennsylvania Press.

Honorton, C. (1993). Rhetoric over substance: The impoverished state of skepticism. *Journal of Parapsychology,* **57***,* 191-214.

Hunt, B.J. (1991). Rigorous discipline: Oliver Heaviside versus the mathematicians. In P. Dear (Ed.), *The literary structure of scientific argument: Historical studies* (pp. 72-96). Philadelphia: University of Pennsylvania Press.

Huntington, E.V. (1927). The notion of probable error in elementary statistics. *Science,* **66***,* 633-637.

Huntington, E.V. (1937). Exact probabilities in certain card-matching problems. *Science,* **86** , 499-500.

Huntington, E.V. (1938). Is it chance or ESP? *American Scholar,* **7**, 201-210.

Latour, B., & Woolgar, S. (1979). *Laboratory life: The social construction of scientific facts.* Sage Library of Social Research, vol. 80. Beverly Hills, CA: Sage Publications.

McDougall, W. (1926). *Outlines of abnormal psychology.* New York: C. Scribner's Sons.

Mauskopf, S.H., & McVaugh, M. (1980). *Elusive science: The origins of experimental parapsychology.* Princeton: Princeton University Press.

Montgomery, S.L. (1996). *The scientific voice.* New York: Guilford Press.

Murchison, C. (Ed.) (1930). *A history of psychology as autobiography.* Worcester, MA: Clark University Press.

Murphy, G. (1934). Review of J. B. Rhine's *Extra-sensory perception. Journal of General Psychology, 11,* 454-457.

Perelman, C., & Olbrechts-Tyteca, L. (1971). *The new rhetoric: A treatise on argumentation* (Trans. by J. Wilingen & P. Weaver). Notre Dame: Notre Dame University Press.

Pratt, J.B., Rhine, J.B., Smith, B. M., Stuart, C.E., & Greenwood, J.A. (1940). *Extrasensory perception after sixty years.* Boston: Bruce Humphries.

Price, G.R. (1955). Science and the supernatural. *Science,* **122,** 359-367.

Rhine, J.B. (1934). *Extra-sensory perception.* Boston: Bruce Humphries.

Rhine, L.E. (1983). *Something hidden.* Jefferson City, NC: McFarland.

Thouless, R.H. (1935). [Review] Dr. J.B. Rhine, *Extra-sensory perception. Proceedings of the Society for Psychical Research,* **43**, 24-37.

Williams, C., & Dutton, D. (1998). Metaphors and lay theories of psi experiences. *Journal of the American Society for Psychical Research,* **92,** 52-68.

Willoughby, R.R. (1935). A critique of Rhine's *Extra-sensory perception. Journal of Abnormal & Social Psychology,* **30***,* 199-207.

Zingrone, N.L. (2002). Controversy and the problems of parapsychology. *Journal of Parapsychology,* **66,** 1-30.

Zingrone, N.L., & Alvarado, C.S. (1988). Historical aspects of parapsychological terminology. *Journal of Parapsychology,* **51***,* 49-74.

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