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CONTROVERSY AND THE PROBLEMS OF PARAPSYCHOLOGY

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ABSTRACT: The author discusses aspects of controversy in parapsychology from the point of view of science studies and its analysis of controversy in mainstream science. Various approaches toward controversy taken by sociologists of science and knowledge are briefly reviewed to clarify some misconceptions parapsychologists have about these disciplines in particular, and about their findings concerning the nature of science practice in general. Special attention is paid to the principle of symmetry and to the social constructionist approach in science studies. The history of controversy in parapsychology is briefly outlined to give a sense of the continuous and essential nature of controversy to progress in parapsychological research. Examples from the rhetoric of science are briefly presented for the lessons parapsychologists may learn from the experiences of other fields. The article also outlines several problems that arise from the tendency psi researchers have to receive in an uncritical and undemanding manner the work of outsiders and critics, especially those who have status in mainstream science, when competence in the subject matter and methodology of parapsychology should be central to such interactions. Finally, the article suggests some ways in which psi researchers can avoid rhetorically disadvantaging themselves in exchanges with mainstream science.

In what follows, I discuss the lessons that I have learned from a reading of the science studies literature, lessons that I believe we can apply profitably to parapsychology. Because of the reaction of some listeners to this paper when I delivered a previous version as my Presidential Address in New York City in 2001, I would like to anticipate a possible misapprehension of my points and my motives. I consider myself to be a working social scientist in parapsychology, although it has been some years since I have had the pleasure of conducting research, whether experimental or survey or questionnaire-based. I have spent far too many years in this field as a social scientist to be equivocal about the existence of the natural world as a whole.

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Neither am I in doubt about the possibility that scientific progress can be made in my own small corner of the scientific enterprise, the study of the psychology of successful experimental participants and of those who report psychic experiences in their lives. I believe scientific methodology of all types possesses an unsurpassed power for amassing knowledge. I am *not,* in any way, someone who believes that scientific knowledge is entirely socially constructed. However, neither am I a person who believes that objective reality imposes itself on the scientific community in such a way as to preclude error or interpretation. Reality exists, but like raw sensation, it comes to conscious understanding through the imposition of perception, and perception—however, it happens—is an essay in complexities.

In this article, I briefly describe some of the work that science studies has done on the problem of controversy. I have tried to do the following: draw some lessons for the parapsychological community; tell some inspirational tales (because that is a function of Presidential Addresses these days, it seems); and make some cautionary statements about some social practices in which we as a community regularly engage, social practices that are, I think, debilitating to our sense of self as individual scientists and to our shared identity as scientific parapsychologists. Under no circumstances should the seriousness with which I take the work of the science studies community be construed as a negation of science and its power, nor as a negation of the importance of "the scientific method" as an ideal in parapsychological research.

The Study of Controversy in Science Studies

For some decades in all the various subdisciplines of science studies, from history and philosophy of science to sociologies of science and knowledge to the anthropology of science to the rhetoric and psychology of science, the deep examination of controversy has been a growth industry. At one point, in the prehistory of this collection of subdisciplines in science studies, scholars believed that controversy was an aberrant moment on the way to some grand consensus. This consensus was then imagined to hold sway over all practitioners of "true" science. As one psychologist of science (McMullin, 1987) put it:

> Classical theories of science, whether of Aristotle, of Descartes, of Kant, or of the positivists, all took for granted two theses: foundationalism (that science must be built on a foundation of propositions, themselves unproblematically true), and logicism (that science possesses a logical method that will allow one to determine which of two theories is the better one in any given case. (p. 50)

As the various disciplines of science studies developed, this simplistic view of scientific practice was repeatedly challenged, replaced by the understanding that controversy is itself "continual and essential" (McMullin, 1987, p. 50) to the refinement of scientific methodology and to the development of scientific knowledge. Controversy can then be defined as a "publicly and persistently maintained dispute . . . in which the difference is one of belief, of knowledge claim . . . it is held to be determinable by scientific means" (p. 51) and "it must seem to the community to be worth taking seriously" (p. 52).

For the positivists who held sway in the mid-20th century—Karl Popper (1959, 1970) among them—some of the contested questions that caused scientific controversy were "What constitutes good conjecture?" "What constitutes a good test?" "What counts as refutation, replication, and falsification?" Underlying this view was the notion that the sciences contained what philosopher Larry Laudan (1983) called "epistemic invariants" (p. 28), truths or facts that were seen to be "essential" to any form of science, that underpinned all sciences, that all sciences must contain to be recognizable as "true science." Individual sciences and individual scientists might identify or understand these invariants incorrectly, at least at first, but ultimately the "facts" and their meaning would be uncovered and understood correctly. Epistemic invariants allowed one to demarcate good science from bad, pseudo-science from real, and science from other forms of knowledge gathering and knowledge use. The presence of this self-correcting logically and rationally revelatory process in science was what made science "superior."

But even 20 years ago, after a decade of fieldwork among the hard sciences by such sociologists of science as Harry Collins (1974, 1975), Laudan (1983) and others were despairing of finding epistemic invariants in the sciences. In their examinations of science and scientific controversy, they uncovered instead an "epistemic heterogeneity of the activities and beliefs customarily classified as scientific" (p. 28) that made the Popperian notion of "demarcation" in science moot. It became obvious to Laudan and others that the variation of method, practice, interpretation, and theory within individual sciences and across the whole of the scientific enterprise made essentialist views of science obsolete. Different answers existed in different disciplines to the questions of what was relevant in terms of instrumentation, what was an acceptable level of predictability, what was an acceptable range of values in measurement, when it was appropriate to engage in ad hoc hypothesizing and when not, and so on.

Having expressed doubts about the presence of Popperian epistemic invariants in science, Laudan (1983) did not deny, however, that there were "crucial epistemic and methodological questions to be raised about knowledge claims" (p. 29), nor did he deemphasize the importance of arguing "that a certain piece of science is epistemically warranted and that a certain piece of pseudo-science is not" (p. 29). But such determinations of what counted as science and what did not had to be done on a more finely grained, more subtle, more sophisticated level, discipline by discipline, or even, as the work of Collins and his colleagues shows, laboratory by laboratory.

Since then, controversies have been characterized in a variety of ways. For Thomas Gieryn (1995), they are boundary disputes, negotiations over the territories of phenomena, method, training, and funding, as well as over what constitutes a "fact," who is qualified to make that determination, and at what point along the way. Gieryn regarded the complex landscape of point and counterpoint as an exercise in the cultural cartography of science, the drawing and redrawing of existing "maps," the moving of boundaries, the modification of features, and the reification (however temporarily) of research programs and disciplines into scalable features of the scientific landscape, unchanging enough to act as reference points and to become identifiable "repertoires of characteristics" available for the next cartographer in line (pp. 405–407).

If the Popperian and even the Kuhnian notion of science were "essentialist" (Gieryn, 1995, p. 407), in the less essentialist view of science, controversy is everywhere. At each of the myriad stages in scientific practice, there is room for dissent, for varying worldviews based on what seemed to be, at first glance, unproblematic truths about the natural world. Add in the profound influence of such nonepistemic variables as historical, political, social, and psychological factors, and controversy can easily arise. Once established, controversies twist and turn toward resolution in exceedingly complex ways. Among the specific, complicating nonepistemic determinants of controversy that have been identified are the influence of disciplinary socialization; the political status of disputants; the power and pervasiveness of networks of advocates and counteradvocates; personal motivations that have little to do with the work at hand and more to do with the constraining impact of everyday life, whether it be everyday life in the laboratory, the department, the university, the corporation, or at home; and, of course, personal differences in intellect, temperament, and experience. Nonepistemic confounds and epistemic arguments can be expanded and unpacked indefinitely. They can erupt, extinguish, intertwine, or fly away in opposite directions, pulling apart communities of scientists and derailing progress and prediction. The hope is, of course, that controversy will lead to improvement in practice, theory, and prediction, but the trajectory toward that goal is seldom direct.

To put it a little less ponderously, controversy flows from a "truth" that encapsulates the ease with which both prosecuting attorneys and defense attorneys can always find a crucial and credible scientific expert to testify on behalf of their own case and against the crucial and credible scientific expert hired by their opponents. The truth is this: "For every PhD there exists an equal and opposite PhD." Robert Procter (1995) borrowed this truth, called Gibson's Law, from public relations research to characterize his observation of the antics of dueling scientific experts in cancer research. The anthropologist of science David Hess (1997) then used Gibson's Law as a rhetorical tool to characterize the impact that motivated interests have on scientific practice (pp. 93–94), a topic that has long been dear to the hearts of science analysts (e.g., Barnes, 1977; Gieryn, 1983; Pickering, 1982).

The observation that underlies this truth is not trivial, however. It embodies the widely varying opinion and practice that can result even when disputants in a controversy share similar education, similar research experience, and even similar disciplinary identity. But if Gibson's Law is the heart of controversy, and controversy is at the heart of the scientific enterprise, how can the chaos of argument be structured to produce progress?

Scientific Norms and Antinorms

As in all social groups, science has developed norms. First described in the 1940s by the sociologist of science Robert K. Merton (1973), scientific norms are essentially social norms but they are also moral norms. The Mertonian norms of science are communism, universalism, disinterestedness, and organized skepticism.

Gieryn (1995) described Merton's norms in this way:

> *Communism* asks scientists to share their findings, and the institution promises "returns" only on "property" that is given away. *Universalism* enjoins scientists to evaluate knowledge claims using "pre-established impersonal criteria" (say, prevailing theoretical or methodological assumptions), so that the allocation of rewards and resources should not be affected by the contributor's race, gender, nationality, social class, or other functionally irrelevant causes. The norm of *disinterestedness* does not demand altruistic motivations of scientists, but channels their presumably diverse motivations away from merely self-interested behavior that would conflict with the institutional goal of science (which is the extension of . . . certified knowledge). *Organized skepticism* proscribes dogmatic acceptance of claims and instead urges suspension of judgment until sufficient evidence and argument are available." (p. 398)

As Hess (1997, pp. 56–58) and others (e.g., Gieryn, 1995; Mulkay, 1975) have noted, the reality of the situation is that these *norms* are used as ideals to which science aspires and should *not* be construed as synonymous with the norms our community understands. That is, Merton's four scientific norms do not *describe* the behavior of scientists in the way that a psychological norm might be thought to describe the prevalence of a specific personality trait in the population. Rather they *prescribe*. They exist as ideals, as important touchstones against which scientific behavior can be measured, especially in the context of controversy.

One can imagine that the perceived violation of Merton's norms can lead to controversies. In my experience in parapsychology, there have been fairly public controversies over the refusal to share data (e.g., Blackmore, 1987; Sargent, 1987) or over the perceived misuse of shared data in the eyes of the scientists who collected it (e.g., Berger, 1989; Blackmore, 1984;

Markwick, 1990; Spinelli, 1989). Controversies have erupted when it seems that personal criteria have heavily influenced the evaluation of knowledge claims (e.g., Beloff, 1968; Eysenck, 1968; Hansel, 1961a, 1961b, 1966, 1968; Honorton, 1967; Medhurst, 1968; Pratt & Woodruff, 1961; Rhine & Pratt, 1961; Shapiro, 1968; Slater, 1968; Stevenson, 1967, 1968; West, 1968) and when barriers have been raised for women or minority scientists (Keller, 1983; Rossiter, 1982), for scientists from laboratories outside the Anglo-American world (Shrum & Shenhav, 1995), or for scientists who are perceived to be of low status.[1] Controversies have erupted when there is obvious self-interest in the methodology set up to test a scientific question or in the interpretation of research results (Bem, Palmer, & Broughton, 2001; Milton & Wiseman, 1999, 2001; Storm & Ertel, 2001). Controversies have erupted over "scientific pronouncements," that is, when there is dogmatic acceptance or rejection of any phenomena or finding, or when announcements of results are couched in statements that are not properly open ended and not properly cautious in terms of what is or can be known at that point in our scientific development. Controversies have also erupted when scientific judgments have been made prematurely, and when the rules of evidence and argument have been purposely distorted in the service of politics and power rather than in the service of science.

Gieryn (1995, p. 398) noted that norms are endowed with a moral authority, and the prose of those who describe the breaking of norms often does so with a sense of moral indignation. As parapsychologists, we have frequently been on the receiving end of such moral indignation,[2] and we are painfully aware that the determination of whether a norm has, in fact, been broken, or should, when broken, be ignored or acted on is a socially motivated process. We know that a double standard frequently exists where we are concerned; that there are times when scientific norms are wielded like clubs against us, in service of goals that seem to us to have little or nothing to do with the overarching effectiveness of science as one of society's primary methods of knowledge gathering and knowledge work.

When norms are wielded for political and social purposes, it is often to do boundary work (Gieryn, 1995, p. 400), to establish a hierarchy of disciplines, and to separate scientists from the nonscientists, the powerful

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[1] Of course simply working in parapsychology is sufficient to completely destroy any status a scientist may have accrued from a conventional degree or a conventional place of scientific or academic employment. See, for example, Alcock (1979), in which he used the term *parapsychologist* to denigrate the qualifications of Karlis Osis and Erlendur Haraldsson in his discussion of their research into death-bed visions (p. 29). Alcock also used the term to dismiss unfairly the work of John Palmer (p. 33). Many other examples of this particular ploy are available in the skeptical literature.

[2] Virtually all of Martin Gardner's works and James Randi's works contain statements in which the parapsychological community is battered rhetorically by a brandishment of idealized scientific norms and misconceptions of scientific practice in prose that fairly drips with moral indignation. See, for example, Gardner (1957, 1981) and Randi (1980).

from the powerless. It is also in boundary work that what have been called *antinorms* come into play (Mitroff, 1974). Organized dogmaticism is one of the antinorms of which parapsychology has seen entirely too much in its history. Organized dogmaticism is the antithesis of organized skepticism: If organized skepticism is the institutionalization of doubt, organized dogmatism is the institutionalization of belief. While the discourse of the hardened skeptical community reveals that they assume they are acting in the best tradition of organized skepticism (see, e.g., Kurtz, 1978a, especially pp. 16, 21, 27, 29; Kurtz, 1978b), skeptics' publications also reveal a reified belief system, a hotly defended dogmatism (e.g., Alcock, 1979, p. 40; Kurtz, 1978a, p. 14) that amounts to faith in a particular mechanistic, reductionistic, compartmentalized worldview. By defending its worldview, the hardened skeptical community situates itself, building a group identity as valued gatekeepers at the boundaries between mainstream and marginal science.

Social psychologists have frequently found that identity-building discourse oversimplifies the beliefs of the in-group just as it stereotypes the beliefs of the out-group (Billig, 1987; Gilbert & Mulkay, 1984; Judd, Park, Ryan, Brauer, & Kraus, 1995). However, such findings provide little comfort to those of us on the outside looking in. As heretics condemned by the organized dogmatism of hardened skeptics, we are painfully aware of the often-severe social, political, and cognitive consequences of being "out there," beyond the boundary between science and pseudoscience. I return to this point later in this discussion.

Approaches to Controversy in Science Studies

Before this discussion gets more specific, I would like to move back to the general for a moment. In an early edition of the *Handbook of Science and Technology* (Jasanoff, Markle, Petersen, & Pinch, 1995), a section of the volume is devoted to controversy. The article by Martin and Richards (1995) describes the four main approaches to controversy in science studies: the positivist approach, the group politics approach, the constructivist approach, and the social structural approach.

The Positivist Approach

In the positivist approach, Martin and Richards (1995) noted:

> The social scientist accepts the orthodox view . . . of the scientific content of the controversy and analyzes the interchanges of the disputants from the standpoint that there is a correct position and an incorrect one. The debate is held to be legitimate and the social scientist attempts to determine if the controversy has been caused by incomplete or contradictory evidence, and then looks for resolution. If the orthodox view of the problem under dispute holds that the evidence has spoken and a proper interpretation is

> already known, the problem then becomes how to explain continued dissent. Legitimate questions for sociological research on the controversy under this approach are "Why do the critics persist in the face of the evidence? Who are the critics and what do they gain from persisting in their views? How do they relate to the wider forces [at work in society], such as corporations, governments and groups of 'true believers'?" (p. 510)

Scientists who argue in defense of the orthodox scientific view of the knowledge claim underlying the controversy are not very interesting to the analyst who uses this approach because such scientists have simply adopted the correct interpretation of the evidence. The interesting actors in the controversy are the dissenters. Examining these disputants under the positivist approach leads to what Martin and Richards (1995) called "a sociology of error." There is an asymmetry in the analysis in that those who hold to the accepted "truth" are not studied, and the dissenters are examined using all the "familiar social science tools . . . [to analyze] individual psychology, belief systems, social roles, vested interest groups, and the like" (p. 510). The analyst is asking, in effect, why the dissenters are so determined to be wrong and stay wrong.

The "sociology of errors" that the positivist approach to controversy produces is familiar to us. We in the parapsychological community, as dissenters to the mainstream scientific worldview, have frequently been subjected to this form of analysis—not so much from sociologists of science as from hardened members of the skeptical community. You can also see the outlines of this type of argument applied to experiencers in some of the work Jerome Tobacyk had done on belief in the paranormal, in a series of studies Harvey Irwin (1993) called the *cognitive deficits approach* to the study of belief in the paranormal. Irwin and others who study the psychological correlates of belief within the community of parapsychology know that the topography of belief is much more complicated than Tobacyk's (Tobacyk & Pirttilae-Backman, 1992) "psychology of error" approach would allow (and indeed, even Tobacyk's more recent research provides support for this complexity). Likewise, many science analysts assert that the topography of controversy is much more complicated than a sociology-of-error approach would allow.

The Group Politics Approach

Martin and Richards (1995) characterized the second approach used by science studies analysts as the "group politics approach" (p. 511). This approach, pioneered by Dorothy Nelkin (1971, 1972, 1975, 1992, 1995), "focuses on the groups involved in the controversy (governments, laboratories, disciplines)" (Martin & Richards, 1995, p. 511). From this approach, the resolution of controversy is "a process of conflict and compromise involving various groups contending in a political marketplace" (p. 511). Martin and Richards also wrote, "There are a number of theoretical frameworks for proceeding with a group politics study. A commonly used one is resource mobilization, in which the focus is on how different groups mobilize and use a range of 'resources,' including money, political power, supporters, status, belief systems, and scientific authority" (p. 511). In this kind of analysis, the epistemic content of a scientific controversy is merely one more tool used by the combatants to bring closure to the controversy and to restore or overturn the balance of power, retaining or reallocating resources.

Analysts who use this approach buy into Merton's third scientific norm, which claims that the average scientist is fundamentally disinterested, therefore—it is implied—objective. When specific interests are identified as operating in the controversy at hand, the group politics analyst will talk about the scientist as having been drawn into the "politicization of expertise." Studies of this sort usually focus on scientific disputes that occur in the realm of public policy (see, e.g., Nelkin, 1995) or within the courtroom where a focus on politics and power to the exclusion of all else can be useful. Applied to a specific scientific controversy occurring within a discipline or across local boundaries of related disciplines, however, the group politics approach does not seem to be quite as useful, particularly if it is used to the exclusion of other approaches.

The Constructivist Approach

The third approach to the study of scientific controversies, the constructivist approach, is the most misunderstood both by scientists and by the public at large. Born in the Science Studies Unit at the University of Edinburgh in the 1970s, and nurtured in various other centers of study including the University of Bath where the sociologist Harry Collins worked for many years, the constructivist approach has been at the center of an almost surreal debate (for a recent example, see Koertge, 1998; and in response, Edge, 1999). In popular parlance, this debate has become known as "The Science Wars" and the constructivist community has paradoxically been branded "Science Haters" (e.g., Leavitt 1999). On the surface, such a reaction to work of the science studies community would seem to be outrageous. When has an anthropologist ever been deemed a hater of indigenous cultures simply because she studies them, or a psychologist a hater of people and their minds and behaviors because his experiments are designed to test them? It is not surprising then that science analysts generally find the charge that they are "science haters" to be ludicrous. Beneath the surface of the Science Wars are important methodological, disciplinary, and even temperamental issues that, once exposed, have provoked serious negotiation important both to the progress of science and to methodological refinement in science studies itself.

For our purposes here, however, it is sufficient to note that the constructivist approach to scientific controversy allows for the influence of a variety of social forces and processes on the development of scientific knowledge. This approach takes as a given that a natural world exists (Latour, 1999, especially pp. 1–23). Several of its main proponents maintain it has always done so. A careful reading of the canonical texts by Barry Barnes, David Edge, and others (e.g., Barnes & Edge, 1991; Barnes, Bloor, & Henry, 1996; Bloor, 1976/1991) will attest to this fact, although some second- and third-generation constructivist analysts do write as if they were naive idealists opposed to any kind of knowledge gathering that presumes the existence of a "real world." Still, the fundamental point here bears repeating: The classical constructivist position, in the main, believes that the world is real, that nature exists, but that the shape and movement of the natural world—its dimensions, its causes, its laws—can be interpreted imprecisely. Further, this imprecision arises, at least partly, from the state of the art of current-day science, that is, from present-day limitations in theory, method, mode of observation, and measurement. But—and this is the key point that the constructivist analyst makes—the imprecision also arises from the sometimes profound influence of social, political, and personal variables on the scientist herself or himself at the point of measurement and at the moment of interpretation (among other loci), that is, on scientific practice itself.

To put it more simply, sometimes the shape of the natural world and the social–psychological–political surround of the scientist combine in equal measure to determine what is taken as a scientific fact. Sometimes when method, theory, and knowledge are more developed, the contour of the natural world is more obvious, and something akin to "pure" knowledge determines the production and application of new facts. However, sometimes when method, theory, and knowledge are not so developed, or when the social–personal–political surround is overwhelming, the contour of the natural world becomes lost and extrascientific, nonepistemic factors determine the production of knowledge. The skeptical community has often said that the overwhelming of nature by nonepistemic variables is what *always* happens in scientific parapsychology; that no matter how sophisticated our methodology or argumentation becomes, our will to believe *always* distorts our scientific practice. We believe, however, that the topography of our "error"—and thus of our "truth"—is much more complex than that.

Essentially then, what the social constructivist is trying to say is that, at different levels of what is *already known,* epistemic and nonepistemic factors vary as determinants in the production of what is coming *to be known.*

The Misconstrual of the Constructivist Approach. This seems like a conservative point to me: that we see with our own eyes, think with our contextualized and socialized brains, and moderate our talk according to the company we keep. But for some scientists, the mere suggestion that they might actually behave at the laboratory bench like the fallible human beings they truly are is so alarming that they misconstrue the constructivist enterprise completely; they become blind to evidence and deaf to argument. This misconstrual has happened even here, in our own community, which is, to be honest, quite amazing to me. If anyone should understand how elusive and chameleon-like the natural world can be, and how delicate method, observation, and theory are in the face of ambiguity and belief, it should be us. If anyone should understand the power of complex perception to confound the understanding of even simple raw sensation, it should be us. Yet, the blind and the deaf are among us.

My point then is this: Science studies analysts who use the constructivist approach have been unfairly accused of denying the existence of the real world, and of claiming that nature herself has no impact whatsoever on scientific knowledge or scientific progress. The classical constructivist approach seeks merely to show that scientific knowledge claims are negotiated entities that contain glimpses of nature moderated by the social, the political, and the psychological. As these glimpses of nature become more precise, clearer, more testable, and more predictable, the amount of social, political, and psychological distortion decreases. This last assertion sounds to me like an alternative way to state the old positivist saw that science is self-correcting. Social constructivist approaches underscore the point, however, that the much-relied-on self-correcting mechanism of science is infinitely more complex and infinitely more susceptible to derailment than we had previously understood it to be. This is a profoundly important message for the working scientist, it seems to me. As Martin and Richards (1995) noted:

> Accounts are not directly given by nature but may be approached as the products of social processes and negotiations that mediate scientists' accounts of the natural world. The study of . . . controversies have the further advantage that these social processes, which ordinarily are not visible to outsiders, are confronted and made overt by the contending disputants. (p. 512)

What could be more important to us than the uncovering of confounding variables? How can we expect to control for, or even eliminate, confounds if we refuse to admit they exist?

The Principle of Symmetry. This brings me to the principle of symmetry. For most of the history of the constructivist approach, both the Edinburgh School and the Bath School have applied what is known as the principle of symmetry; that is, the methodology adopted in their brand of science studies required that scientific knowledge claims made by each side in a controversy needed to be balanced in the analysis (Bloor, 1976/1991). In the study of scientific controversy, those who conformed to the orthodox scientific worldview were not to be privileged over those who dissented. Beginning in the 1970s, Harry Collins, Trevor Pinch, and others conducted a number of important studies of parapsychological research using this approach (Collins & Pinch, 1979, 1982). We and other dissenters whose scientific production were studied in this way benefited from the principle of symmetry because our knowledge claims were taken as seriously as were those of the orthodox view (a happy by-product of the principle of symmetry), or, once our knowledge claims were bracketed or set aside with the knowledge claims of the orthodox community as beyond the scope of the analysis, the structure of our enterprise was analyzed as though we were not working beyond the margins of the mainstream—again, a happy by-product that, we thought, allowed us to be seen as we ourselves see us, as "real" scientists and not as pseudo- or marginal scientists.

Recently, there has been a certain amount of backlash in the science studies community against the principle of symmetry (Scott, Richards, & Martin, 1990). Main journals in the field, such as *Social Studies of Science,* have published discussions about the importance of the cognitive content of scientific work and about the necessity of enlisting and acknowledging scientific expertise when analyzing controversy (Collins, 1999a, 1999b, 1999c; Edge, 1999; Koertge, 1999; MacKenzie, 1999a, 1999b). In my reading of these discussions, I see a connection between this concern about scientific content in general and the fact that parapsychology was given what we have always thought was a "fair" reading, particularly in the work of Collins and Pinch (1979, 1982). Perhaps for some science analysts, symmetry was fine as long as the controversy studied took place within the boundaries of orthodox science and not across the divide between "us" and "them." Even Collins and Pinch have noted in their recent books, *The Golem: What You Should Know About Science* (1993/1998a) and *The Golem at Large: What You Should Know About Technology* (1998b), that the principle of symmetry seems to have served, in effect, to aid the enemy, that is, to bolster the status and persuasiveness of those whose lines of research were labeled by orthodox sciences as failed or erroneous.

As proponents of a science that is considered to be failed or erroneous by mainstream science, we as parapsychologists can understand the need for constructivists to propose a corrective to the sociology-of-error approach positivists adopted. Paradoxically, however, like the hard science combatants in the Science Wars who are particularly unnerved by the principle of symmetry, we parapsychologists *as* scientists can resonate with the notion that the strength, validity, and reliability—the "trueness" of the scientific knowledge underlying a controversy—is extremely important to any real understanding of what is going on. After all, whether or not there is demonstrable, replicable ESP or PK in our data, whether or not spontaneous case experiencers *actually* experienced something paranormal or are merely misattributing paranormality to some normal event, is of prime importance to us. As scientists, we are working toward a level of descriptive and predictive understanding of the natural world that is as close to "true" as it can possibly be. So, as working scientists, we understand and applaud the efforts positivist detractors of science studies have made to get science analysts to realize that the content of science is *at least* as important as the context of science, if not more so.

Still, we, as parapsychologists, are not above "capturing" the rhetorical advantage the principle of symmetry can convey on us. For example, it was music to our ears when, talking about scientific parapsychology, Collins and Pinch (1979) declared that in the subtitle of their article "nothing unscientific is happening here." We perceived that statement and others like it to be a validation of our enterprise evidence that even if the mainstream scientific community has marginalized us, we do not deserve to be. Collins has found that we are not the only community who is willing to co-opt science studies in this way (Collins & Pinch, 1993/1998a). But I digress.

The Social Structural Approach

The fourth approach to controversy Martin and Richards (1995) described is the social structural approach (p. 514), which looks at scientific controversy from the point of view of such macrosocial structures as class, the state, and patriarchy. Marxist and feminist sociologists of science have generally used these approaches with widely varying degrees of success. Among the most important of these types of analyses, to my mind, are those that have been done on gender and science (Haraway, 1991; Harding, 1986; Keller, 1985), a topic that has found some resonance in our community as well (Coly & White, 1994; Hess, 1988; Zingrone, 1988, 1994).

A Multimethod Approach

The bottom line here, Martin and Richards (1995) maintain, is that a method which integrates one or more of these four approaches is needed to properly understand scientific controversy. Such a method has, they feel, a significantly better chance of providing really useful answers to such questions as "Why do specific scientific controversies erupt?" "Why do some controversies persist?" "What counts as closure in a scientific controversy?" and "How does closure occur?"

I agree. It is crucial to acknowledge the essential importance of the cognitive underpinnings of scientific debate, to recognize that there are always cognitive winners and losers whose relative positions in the debate are meaningful and must not be set aside. It is also important to understand the politics in which the cognitive debate evolves and persists. Without such an understanding, the analyst may forget that the attributions, which divide winners from losers, may be resource-based and not representative of the strength, utility, or "trueness" of the underlying knowledge claims. An integrated approach to science studies requires the analyst to remember that the knowledge claims themselves, and the process by which a controversy erupts and persists, are multiply determined and complex and may result from a symphony (or a cacophony) of forces, processes, and positions, with the contours of the natural world more or less obscured. Just as we understand that, in parapsychology, a multivariate approach requires empirical and theoretical sophistication, Martin and Richards (1995) believe future science analysts will need to be more careful about what voice, what observation and what depiction are privileged as their analysis proceeds. Their position on the development of science studies methodology makes sense to me, as I stand on the periphery of their discipline, ready to bolt back to parapsychology proper as soon as I can. But, as in all other fields, Martin and Richards are only two voices in the controversy over method in science studies. Like all other scientific and academic communities, there are those who disagree.

Studies of Controversy as Inspirational Texts for Beleaguered Scientists

Even though science analysts disagree on methods, they all agree that scientific controversies, in general, provide a particularly fruitful locus for research into the development and refinement of scientific "fact," method, argument, and practice. But beyond the understanding one can draw in a general sense from this line of research, there are other insights in the literature on scientific controversy that I, as a working parapsychologist, find personally heartening. One example is Jeanne Fahnestock's (1997) article, "Arguing in Different Forums," published in the anthology *Landmarks Essays in the Rhetoric of Science* (Harris, 1997). In her paper, Fahnestock outlined the controversy that has raged between scholars who believe that modern humans migrated to the North American continent around 14,000 years ago and those who push the date much farther back. Among the many parallels to the problems parapsychology faces are the following: the rhetorical methods used by combatants in the controversy; the varying use and relative merit of popular versus academic publishing outlets; the difficulties involved in making convincing arguments out of exceedingly ambiguous or emotionally highly charged data; and the seemingly never-ending search for the next tangible piece of evidence.

Another set of studies that I found fascinating and oddly comforting was published in Greg A. Myers's (1990) book, *Writing Biology: Texts in the Social Construction of Scientific Knowledge.* Myers analyzed, among other texts, successive drafts of ultimately successful research proposals of two biologists and the trajectory of original articles and their revisions that the biologists wrote on similar topics (one of them had an article rejected by *Nature* but published in *Science,* and the other had a similar article rejected by *Science* but published in *Nature*). Myers also analyzed the impact of "outsider" status on the two biologists' ability to have their work heard in the scientific communities to which they belonged, and on their ability to find collaborators and funding (one made his "big claim" as a graduate student, the other crossed disciplinary boundaries late in his academic career to make his "big claim"). Myers's analyses of the experiences of these two biologists reminded me greatly of what many of us have faced.

What is heartening in these cautionary tales for us as marginal scientists is that other lines of research and other findings, now consensually endowed by orthodox science as "true," have, in less contested realms than the paranormal, faced staggering social and political obstacles but have, ultimately, achieved "factness" just the same. For this reason, these case studies lift the spirit. But perhaps these tales are also heartening not only because they suggest ways in which parapsychology might rhetorically, socially, or evidentially improve its lot but also because they provide proof that, well, we are not alone. Even in our particular experience of controversy, "nothing unscientific is happening here" (Collins & Pinch, 1979).

Living With Controversy

My enjoyment of the controversy case study literature aside, it is still true that that which gives a science analyst great joy—a juicy controversy to investigate and interpret—can be a tiresome but necessary evil for a working scientist. We can intellectually understand the appeal of studying at close range the actual negotiation process by which an observation becomes a fact, but being caught personally in that negotiation, with its endless rounds of argument and counterargument, of criticism and response, can become quite daunting. What the working scientist laboring under the cloud of controversy looks for is a little closure, a little peace. It is not easy being a dissident, especially in a persistent controversy.

I have spent approximately half of the last 8 years working on what seems to be the never-ending doctoral dissertation. Titled "From Text to Self: The Interplay of Criticism and Response in English-Language Parapsychology," the contours of the thesis have changed drastically at least three times. One of the early shocks that forced a reassessment of the enterprise was that the bibliography of criticism and response in the English-language literature was huge, even though I had decided from the outset to ignore the rather significant number of controversies that played out solely in the correspondence sections of the main journals. Initially, I thought that limiting the bibliography to controversies published in English would collapse the terrain sufficiently to make some comprehensive handling possible. (There are, after all, an enormous number of important controversies in the French, German, Italian, and Spanish literatures of psychical research and parapsychology.) The resulting bibliography was, of course, still too large—over 2,000 items—for deep analysis in a single dissertation. The breadth of it is instructive in and of itself, however. Listed so far are books, articles, book chapters, and book reviews, published between 1820 and 1998, with the *Journal of the Society for Psychical Research* and the *Journal of the American Society for Psychical Research* only incompletely canvassed between 1906 and 1936 and no references taken as yet from the last 3 years. To give the reader a flavor of the riches the bibliography represents, it is helpful to focus on the depth of one of the controversies included, the debate that extended from the publication in 1934 of Rhine's monograph, *Extra-Sensory Perception* to the publication in 1940 of Pratt, Rhine, Smith, and Greenwood's *Extrasensory Perception After Sixty Years.* This controversy alone comprised over 100 articles, involved virtually every member of Rhine's laboratory, and took up a considerable amount of time of a number of mainstream psychologists, not to mention virtually all of the pages of the *Journal of Parapsychology* over the intervening period and not an inconsiderable number of pages in the *Journal of General Psychology,* the *Journal of Experimental Psychology,* the *Journal of Abnormal and Social Psychology,* and *Psychological Bulletin.* More than 140 journals and periodicals were involved in the "ESP" to "ESP-60" controversy.

Over the whole of the bibliography, there is a wonderful range of publications, from the obscure *Psychological League Journal* and *Pedagogical Seminary* to such well-known general science outlets as *Nature, Science, New Scientist,* and *Scientific American,* and from such general audience publications as *Atlantic Monthly, Scientific Monthly,* and *The New Yorker* to the intriguingly named *Unpopular Review.* Specific controversies swirled around a variety of mental and physical mediums, a number of controversial field investigations, among them Borley Rectory, and around a number of such colorful researchers as Cyril Burt, Harry Price, S. G. Soal, and Carl Sargent, to name a few. Dice tests and metal-bending have been the subject of extended debates, as have laboratory methodology, sensory cues, security procedures, and the relative merits of selected and unselected volunteers as experimental participants. I have in my controversy coding list nearly 90 individuals who made repeated contributions to various controversies, among them such luminaries as Frank Podmore and Edmund Gurney, E. G. Boring and Joseph Jastrow, John L. Kennedy and Charles Kellogg, Ian Stevenson and William Braude, and Charles Honorton and John Palmer.

Once the terrain became apparent, I decided to focus on specific controversies in the dissertation and to draw on the methodologies of science studies in doing so. Hence the research that underlies the present article. That is, it became obvious early on that the history of English-language psychical research and parapsychology *is* the history of controversy. The topics debated, of course, have changed over time as research interests have shifted. But the predominant emotion that rises in one's throat as the bibliography scrolls by is longing for closure, for consensus.

Being in a state of continual controversy in a marginal science is a very peculiar experience, even if controversy is, in and of itself, continual and essential in science as a whole. As a working scientist in this discipline, it is obvious to me that we have made an enormous amount of scientific progress since the founding of the Society for Psychical Research in 1882, particularly given the persistent lack of funding, institutional support, and personnel. There are those in the field today, however, who would chalk all that progress off to a lack of discernment, to wishful thinking, and to a continued tradition of ineffectual scientific practice. I agree with Henry Sidgwick, and with Dean Radin who quoted Sidgwick a few years back in his Presidential Address: The time when we needed to debate whether or not the phenomena we study exist is long past. There *is* an anomaly here. The shape of the natural world that is embodied by that anomaly is becoming clearer and clearer with every methodological refinement, every theoretical advance. The day is coming when the social, psychological, and political surround will not be able to distort the process of observation or the resulting interpretation.

On the other hand, my bibliography has also instilled in me a sense of caution. Unlike Radin, I am not going to put a number on it. There is a real possibility that a hundred years from now somebody else will be standing in my place, evoking the names of some of us here and saying, "Like so and so, I believe the time is long past . . ." Closure slips in and out of our grasp. But closure also slips in and out of the grasps of more mainstream scientists than we, and the mysteries of that process energize the same science analysts who work so hard to understand the process of controversy itself. But how does closure happen?

The Case of Cold Fusion

Before we look at what some psychologists of science have to say about closure, let us look at a case study of a field that has suffered what appears to be closure and is considered by Anglo-American mainstream science to be absolutely, completely, and utterly dead: cold fusion.

In the February 1999 issue of *Social Studies of Science*, Bart Simon (1999), a member of the sociology department of Queen's University in Ontario, Canada, published an article called "Undead Science: Making Sense of Cold Fusion After the (Arti)fact." Simon obtained his doctorate at the University of California at San Diego with a dissertation on cold fusion, the subtitle of which was "The Hauntology of the Technoscientific Afterlife." What interested Simon most was the disjuncture between what the mainstream scientific community knows about cold fusion and what the cold fusion research community knows about itself. In the United States, Simon pointed out, mainstream scientists "know" that 12 months after the March 1989 press conference in which two University of Utah chemists, Martin Fleischman and Stanley Pons, announced that they had discovered cold fusion, an interdisciplinary conference "proved" that the announced results had been spurious, an artifact of science practice that was, at least, wrong-headed, and at worst, incompetent. Once this "fact" was known, mainstream science withdrew its moral and financial support, the original claim was declared null and void, and institutional barriers against further research were erected. Science declared cold fusion "dead."

Even though mainstream scientists now "know" that cold fusion is impossible or, at least, not found by Pons and Fleischman and their supporters, even though the Department of Energy no longer funds such research, and even though no patents have been given in the United States for devices that are in any way related to the notion, Pons and Fleischman report to work each morning at a state-of-the-art research facility in Nice funded by a subsidiary of Toyota Corporation for the sole purpose of continuing their work. Even though no self-respecting Ivy League university in the United States would openly pursue such a dead and discredited research problem or allow a graduate student to waste a dissertation on it, every year about 200 researchers who are still actively pursuing the research, mainly in countries other than the United States, meet to share research results and otherwise move their science forward. Although "dead" in the eyes of the Anglo-American research establishment, bereft of traditional resources and traditional mainstream political credibility, the cold fusion research community is—as Simon says with his tongue planted firmly in his cheek—"undead." It is a community that continues to do its work.

As Simon analyzed the situation, he began to understand that while what he calls the central fact about the cold fusion research community is that they "died" to mainstream science in 1990, 10 years later the community is still very much alive. Although, by the yardsticks of mainstream science in the United States, the cold fusion research community may not be faring so well in its afterlife, that afterlife does exist. It is a place where funding is obtained from alternative sources, where patents are entertained by other governments, and where alternative ways of bestowing political capital have been developed, including the opening of the community to less traditionally trained and less traditionally employed researchers. Hence, Simon used the term "hauntology" in his dissertation subtitle because he sees the cold fusion research community as relegated by a "closure" that may or may not have been warranted, to a netherworld between life in mainstream science and the "true" death of a research question and its community. There, in this afterlife, members of the cold fusion research community exist like ghosts, hidden from public view, but continuing to work toward the moment when they will, they hope, cross back over the threshold into the world of the living again, or, as they fear, toward the moment when even the afterlife they have fashioned will no longer be scientifically possible or socially viable.

As a science analyst, the presence of the cold fusion community provoked Simon to ask deeper questions about the nature of controversy, consensus, and closure. For the working parapsychologist tired of controversy, the article provides a perverse kind of comfort. No matter how difficult our scientific lives are, the research life the cold fusion community has carved out for itself seems a little bit harder to live with than our lot.

Seeking Closure

Psychologist of science Ernan McMullin (1987), with whose definition of classical notions of science I began this article, identified three methods by which scientific controversies achieve closure (p. 6): resolution, closure, and abandonment.

For McMullin, resolution is a kind of closure that flows from rational argument, a closure based on merit and on fact. Simple closure, on the other hand, flows from social, political, and psychological considerations, and abandonment is simply that—the setting aside of the research problem. McMullin warned that controversies that achieve closure through the application of nonepistemic factors and are not in fact epistemically resolved will inevitably be reopened on rational grounds. I would warn that research topics that are abandoned for nonepistemic reasons will also be reopened on rational grounds.

McMullin, like many current psychologists of science, oversimplified science and its attributes. Closure, like controversy, is a complex and varied terrain. In his defense, though, even the great philosopher of science Thomas Kuhn has been accused of "black-boxing" the related concept of consensus, which presumably is an essential element in his concept of paradigm shift (Gieryn, 1995). For Kuhn, at least the early Kuhn, there was an inevitable movement in normal science toward the conversion experience—the paradigm shift—in which the consensus as to what constituted the fundamentals of science changed profoundly (Kuhn, 1970, 1977). This conversion experience then rippled through science, changing the boundaries of disciplines, reallocating resources, and so on. The paradigm shift was followed in Kuhn's scenario by another long walk through normal science toward the next sea change, the next paradigm shift.

Thomas Gieryn (1995), who I also mentioned at the beginning of this article, criticized Kuhn for not unpacking the term *consensus* and instead simplistically noting that this magical point of accord would be reached at the point at which the paradigm shifted. Consensus, Gieryn (1995) noted, is far more problematic than that.

> Scientists must themselves solve three interpretative problems as they consider what consensus means and whether their field has achieved it. First, they must decide the limits of membership of their research community, for inclusion or exclusion of certain individuals could easily affect their conclusions about the extent of consensus. Second, they must reach judgment on the changing beliefs of other scientists in regard to their subject matter . . . . Who accepts it, and when did their conversion to the new framework occur?; and Third, scientists must decide the cognitive content of the new view because . . . if there is consensus, one needs to know just what does the community agree on? (p. 404)

To achieve closure, to move beyond a state of controversy, these three things must also be in place, and for parapsychology all three of these things have always been in dispute, within the boundaries of the field and outside. Even if we can agree that the phenomena exist, are they paranormal? Are they as yet unpacked physical phenomena that will fit within an expanded understanding of the natural world? Do they have something to say about the relationship of mind to body? Do we as a community even believe that the mind–body question is worth asking, or are we walking toward a kind of hybrid idealistic point of view, where the natural world is configured so differently we might as well consider ourselves to be creatures fundamentally constructed of thought rather than of matter? Are our experiments related to our musings on the ultimate questions of humanity? Is there an afterlife? Is there a soul? Are we ready to talk about such things in public? Is mainstream science ready for us? (Like the members of the cold fusion community Bart Simon interviewed, I suspect we believe we have always been ready for them.)

"Science," Gieryn (1995) wrote, is "a kind of spatial 'marker' for cognitive authority, empty until its insides get filled and its borders drawn amidst context-bound negotiations over who and what is 'scientific'" (p. 405).

Learning Lessons

What can parapsychology learn from science studies? I am not the least bit apologetic about co-opting the insights science analysts have amassed so far. I am perfectly happy to continue to "capture" whatever is useful to us as working scientists in a discipline that has been called marginal and pseudoscientific. I believe what we, as parapsychologists, do is defensible as good, solid science practice. Moreover, I believe that science studies offer us lessons that can be learned to our cognitive and social advantage. If the resolution of controversy and the formation of consensus are both social and cognitive processes, then it behooves us to learn how to manage effectively those processes. To do so requires that we think more deeply about our own personal and social motivations (as deeply as we think about our theory, our methodology, and our analytic and interpretive tools), about our personal reactions to findings and theories, and to the work of our colleagues and our detractors. We should question everything, but especially those attributions and interpretations, which appear to us to be visceral, automatic, and facile.

There is one seemingly automatic response that we have labored under too long. When a critic raises his or her head among us, as a community we typically endow them immediately with political capital. If the critic comes to us from a more mainstream discipline, or from a professional group we have been taught to believe we need, we are even quicker to give that person the floor. We listen more intently, but we also use our own critical faculties less forcefully. The endowment of political capital in outsiders is ratcheted up again when the "someone" who arrives on our scene appears to have political power in some other scientific or public venue. Because of the emotion our subject matter evokes in mainstream science, because of our continued devotion to a discipline that does not reward us with jobs, credibility, or even the minimal resources needed to make reasonable scientific progress, we are already disadvantaged speakers in any controversy. When we cower in front of the mainstream community and ask of them less competence and less familiarity with our subject matter than we would ask of ourselves, what do we signal to ourselves and our critics?

Case studies of scientific controversy have shown that rhetorical disadvantage can lead to severe social and even cognitive disadvantage, that closure can come from capitulation, that winners and losers in scientific controversies do not always obtain those attributions through anything that resembles a fair contest.

It has been my experience that we, as parapsychologists, tend to value the skeptic—especially when he or she comes from outside our community—more than we value any proponent, even when we ourselves are acting as proponents. We value those among us who sound like critics in public and like proponents in private. Is there not a contradiction in valuing the person who, after a lifetime of research, proclaims publicly that psi is just a hypothesis when in private they are adamant that the research proves that ESP and PK exist? Is there not a contradiction in valuing the "real scientist" who comes among us clandestinely and would never, for one moment, expose himself or herself to the loss of career mobility, mainstream scientific capital, or personal credibility that open support of, or research in, parapsychology would bring? We have to be aware that tactics, attitudes, and habits that begin as rhetorical or social defense mechanisms can quickly become maladaptive, both socially and scientifically.

Science analysts have shown that even in scientific controversy the subtext of a debate can be easily understood, both overtly and unconsciously. The critic and the outsider observe the process by which we pay homage to them at the expense of ourselves. They understand that we are giving them more power to wield among us, more protection from criticism, more latitude with methodological and analytical tools, more status in our community, whether their science practice deserves it or not. They see by our behavior and our rhetoric that characterizations of us as marginal and methodologically weak scientists must not be far off the mark, because we appear to be fundamentally shame-faced about who we are and what we do and what we have concluded. We are signaling that we do not see ourselves as equal partners in the controversies that surround us, that we ourselves are ready to abdicate our right to broker closure and to build consensus.

Yet, what I see in our convention hall behavior flies in the face of what takes place on private chat lists, and in sessions devoted strictly to our internal scientific questions. We have, as a community, a great deal of methodological competence and theoretical subtlety. We have made scientific progress in spite of the obstacles we have encountered in our paths. I have said earlier that I see actual progress being made in our field. The disjuncture between our public face and our private face arises from a failure of presentation, not of a failure of substance. In a sense, that is a hopeful fact: Failures of presentation can be more easily managed, provided we are willing to see ourselves clearly and act on what we see.

Where Do We Go From Here?

What I think we can learn from science studies and its abiding interest in scientific controversies is that from a sociological point of view, from a psychological point of view, and from a rhetorical point of view, we must not, under any circumstances, signal any kind of ambiguity about who we are and what we know. Nor should we allow our boundaries to be endlessly and mindlessly permeable on the border that lies between us and mainstream science, and mindlessly impermeable on the border that lies between us and the nontraditionally trained, the nontraditionally employed, the experiencer, and the practitioner. Management of our movement toward consensus, even toward a paradigm shift if there is such a thing, can be done consciously, even self-consciously.

What science studies has done is to flesh out the social, psychological, rhetorical, and literary dimensions of science practice, just as the history of science has fleshed out the organizational, institutional, and otherwise contextual dimensions. There are those who see the science studies enterprise as the setting of a dialectic, an either/or metric in which science is either a positivist, realist enterprise that has as its touchstone knowable nature or a complete illusion, a fictional construction multiply determined by all the elements that drive imaginative, socially-minded, heavily cultural humankind. To see science studies in this way is to misunderstand fundamentally what this collection of disciplines comprises. The cartography of science that science studies has developed is variegated, as complex and shifting as nature itself. We know, as working scientists—and science studies only serves to provide support for this point—that there is no either/or in science. The practice of science *is* a positivist search for the truth in a real world, in an out-there, knowable, objective natural world that reveals itself little by little for better or for worse. But like any human enterprise, science is also an interpretive soup of social, moral, cultural, and personal fashions, of obligations, understandings, foibles, strengths, and weaknesses, all impinging on our ability to trace accurately, and understand completely, the contours of the natural world. As consensus builds, we hope—and I believe—nature will become more really "real," more revealed, more "usable," more "understandable," but no matter how far science develops, waiting out there will always be a natural world that is even more "real," one in which the interpretive overlay of our current and near-future foibles have been whittled away even more.

I would like to make one final comment on the intertwining of the history of controversy with the history of parapsychology. Controversy has been the life of the parapsychological community from the beginning of our attempts to build a science of the paranormal. Controversy has been our life's blood as well. We have struggled with controversy, but we have also thrived on it. While we may long for closure, for the peace of a solidified consensus, what we really know in our heart of hearts is that science is a process. Science in general and parapsychology in particular are monumentally engaging, inspiring, infuriating, and full of twists and turns and differing perspectives. The scientific methodology of parapsychology is particularly well suited, as it now stands and as it is evolving, to deal with both the questions our anomalies raise and with the wider mysteries that are just beyond the horizon. Controversy may be a growth industry in the social sciences, but in parapsychology, controversy is also the engine that drives our progress. It is ubiquitous, frustrating, exhilarating, and unavoidable. If we add an understanding of the social, political, and rhetorical surround to the methodological and analytical tools we already have on our research bench, progress in parapsychology is inevitable.

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