DISCIPLINING HETERODOXY, CIRCUMVENTING DISCIPLINE: PARAPSYCHOLOGY, ANTHROPOLOGICALLY

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ABSTRACT

This essay develops the study of boundary-work by moving the study of academic psychologists, as well as other scientists engaged in research on extrasensory perception or psychokinesis, from the level of ideological struggles between scientists and nonscientists to the microlevel of disciplinary mechanisms as they apply to scientists who became marked (both culturally and politically) as non- or pseudoscientists. Ethnographically, the study documents the "horror stories" of academic persecution that circulate as part of the oral tradition of the parapsychology community in the United States. Theoretically, the goal is to arrive at a dynamic approach to the problem of disciplinary mechanisms and social control mechanisms on the one side, and circumvention strategies on the other side, for taboo areas of inquiry. The conclusion of the study then directs the analysis of boundary-work regarding these "marked scientists" back to the field of science and technology studies to formulate a critical analysis of terminology and research priorities within STS. [P. 224 begins here.]

As the work of Collins and Pinch (e.g., 1979, 1982), Wallis (e.g., 1979), and other STS researchers has demonstrated, the study of controversies associated with heterodox sciences such as parapsychology can provide insight into broader theoretical problems of general interest to the STS field. One such problem is the social study of scientific boundary-work, that is, conflicts over the authority of science and the legitimacy of "pseudosciences" (Gieryn 1983). This paper develops the study of boundary-work by placing academic psychologists, as well as other scientists engaged in research on extrasensory perception or psychokinesis, in the context of what Brian Martin and colleagues have called "direct intellectual suppression" (1986): the denial of funds, advancement, job opportunities, and so on that whistleblowers and other critics of established views frequently face.

Through correspondence and interviews with approximately twenty academic parapsychologists in the United States, I document some of the "horror stories" that circulate as part of the oral tradition of the academic parapsychology community. Drawing on the materials gathered in these interviews, I construct a typology of what might be called the mechanisms for "disciplining" heterodox science and scientists in the academy, and I use the typology to evaluate the usefulness and limitations of Foucauldian frameworks for this type of problem (Foucault 1979, 1980). Then, following scholars such as De Certeau (1984) and Bourdieu (1975), I examine the strategies of circumvention that parapsychologists employ in response to or anticipation of these mechanisms. The result is to arrive at a fairly specific but equally dynamic approach to the problem of disciplinary mechanisms and social control mechanisms on the one side, and circumvention strategies on the other side, for taboo areas of inquiry. Furthermore, by adopting the method of informal interviews on life-history events, it is possible to expand the concept of boundary-work from the level of ideological struggles between scientists and nonscientists to the microlevel of disciplinary mechanisms as they apply to

scientists who became marked (both culturally and politically) as non- or pseudoscientists. Finally, I direct the analysis of boundary-work regarding these "marked scientists" back reflexively to the field of science and technology studies to formulate a critical analysis of research priorities.

In addition to presenting these theoretical arguments, the essay is also intended as a "mere" ethnographic description of one aspect of the culture of academic parapsychologists in the United States. From my perspective as a cultural anthropologist, the "thick description" (Geertz 1973; see Hess, introduction) of parapsychologists' identity as a persecuted minority is an end in itself, not a means to more general theoretical constructions. By wallowing in the ethnographic detail, so to speak, I am implicitly challenging the ideology of STS as an interdisciplinary field, where in fact there is a strong tendency to relegate historical and ethnographic description--and historians and anthropologists--to a second-class status unless they are willing to change their [p. 225 begins here] discourse and write in ways that are "theoretically interesting" rather than "merely descriptive." A truly interdisciplinary field will have to recognize the place for thick description as important in its own right, because the issues and peoples described in these studies warrant our attention and consideration, and because such analyses provide vantage points for a more profound inspection of our own cultural/disciplinary assumptions. Certainly, the details by which muted voices of any type--underrepresented ethnic groups, Third World, feminist, gay/lesbian, or even parapsychological--are pushed out of the academy warrant close attention and further study, and the case of parapsychologists provides one interesting example of how such processes work.

Background

In the world of academic science, "discipline" operates through the long periods of socialization and enculturation that students go through as they become members of a

scientific "discipline" (see Foucault 1979; Kant de Lima, this volume). In the process of socialization, students encounter disciplinary techniques or mechanisms as they are ranked, sorted, individualized, examined, and observed. However, in the case of the regulation of heterodox sciences such as parapsychology, disciplinary mechanisms converge with more explicit discourses and practices that have often been described as "social control mechanisms" or "gatekeeping functions."

Discussions of social control mechanisms in science tend to focus on the conventional gatekeeping role of scientists who regulate access to journals, professional titles, and public and private funding (see the review by Merton 1973: 521-523). Instances of gatekeeping are well-documented in the case of academic parapsychology, and cases in which orthodox scientific journals have refused publication of parapsychology research for reasons of apparent bias are too common to discuss in detail (see McClenon 1984: 114-121; McConnell 1983a: 165-177, 240-248). As a result of difficulties in publishing in orthodox science journals, parapsychologists have tended to publish in their own journals, a process which becomes self-fulfilling because editors of orthodox scientific journals may then argue that the papers should be published in a parapsychology journal (see Stevenson 1984).

Regarding the other major gatekeeping mechanism, funding, parapsychologists believe they suffer prejudice in general from foundations and governmental agencies, and this prejudice has deepened after a 1987 report by the National Research Council, whose members included the skeptical psychologists Ray Hyman and James Alcock. Parapsychologists argue that the report was biased because it "had attempted to suppress a committee-commissioned report by noted psychologist Robert Rosenthal which was [p. 226 begins here] highly favorable toward parapsychology" (Berger 1988: 14; see also Palmer et al. 1989). The conclusion of the NRC report, that there is "no scientific justification from research conducted over a period of 130 years for the existence of

parapsychological phenomena" (ibid.), will undoubtedly adversely affect future funding efforts.

Even before the NRC Report, parapsychologists experienced what they viewed to be unfair treatment from government funding agencies. For example, Professor A, a leading parapsychology researcher, wrote to me with the following anecdote:

> I once applied for a grant to do some parapsychological research to the National Science Foundation (it could have been the NIMH; I'd have to go back into old records to see). The grant was denied. When I requested an explanation, I was sent summaries of what were touted as the reports of all the evaluation committee members. There were many negative comments, many of them, in my judgment, technically incompetent and representing prejudice.
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> It was only by accident a year or so later that I ran into someone at a professional meeting who turned out to have been on the reviewing committee and to have given very favorable comments. The agency seems to have "lost" his comments and forgotten his existence altogether in their count of how many referees there were. I supposed if I had wanted to put in a huge amount of energy, this situation could have been turned into a scandal, but it wasn't worth my time. Bias in journals and granting agencies is, in my personal experience, all too common.

Another recent case, British rather than American, reveals how funding may be given, but only after the heterodox aspects of the funding proposal are stripped away:

> Parapsychologist Dr. Serena Roney-Dougal applied for support from Britain's Enterprise Allowance Scheme, proposing to do research and teaching and write a book about psychology and the paranormal. Six months and numerous letters later she was told that her proposal is "unsuitable for public support because it covers subjects such as

clairvoyance, the occult and various psychic phenomena." She was finally accepted, but only if she restricts her work to teaching about topics other than the paranormal ("Teaching Restricted," *Parapsychology Review*, Sept./Oct., 1988, p. 13).

Since these formal, gatekeeping mechanisms are already well-known and discussed in the STS literature, in this essay I will focus instead on the problem of how disciplinary mechanisms operate at an informal or microlevel, and how in the process they affect individual life histories, career trajectories, and decisions influencing advancement and job placement. I have used the Foucauldian term "discipline" to emphasize the similarity to the rationalized technologies of power that he discusses in *Discipline and Punish*. Of particular relevance is the discussion of the "means of correct training," including the analysis of the distribution of actors into ranks of grades that serve as a means [p. 227 begins here] of reward and punishment (1979: 181). By bringing Foucault into the discussion of "intellectual suppression," and by examining the role and importance of circumvention strategies, it will be possible to contribute, in a modest way, to current evaluations of the relevance and limitations of the framework of "disciplinary technologies."

Method

To investigate how the informal and microlevel disciplinary mechanisms operate, I began by writing a letter to thirteen North American academic parapsychologists (or psi researchers). I chose these parapsychologists because they were prominent (based on publications) in the field in the United States, and because most of them hold or held positions in colleges or universities. I asked them to describe to me anecdotes about how doing psi research 1) affected their experiences in the academic job market; 2) affected their position within their profession of origin, such as posts on journals and officer titles within professional organizations; 3) led to social and even political ostracism among

their colleagues in the university setting; and 4) affected decisions of promotion and tenure. I also asked them what strategies they adopted in order to mitigate the stigmatizing effects of doing research on extrasensory perception or psychokinesis.

Of the thirteen researchers selected for this preliminary survey, two did not respond to the letter (one of whom I later interviewed), three declined to participate (two for time considerations, and one because such material was already available in printed form), two said that their experiences were not relevant because they were now outside the academy, and the remaining six gave some very detailed and helpful answers. I then followed up this set of questions with interviews at the August, 1990, meeting of the Parapsychological Association in Washington, D.C. At this conference, I spoke informally with twenty persons who hold academic or research positions in the United States and who have completed research in or expressed interest in the field of psi research (that is, I spoke with nearly everyone who attended the conference and met these criteria). Of these twenty researchers, thirteen reported some cases of prejudice because of their interest in the topic, and a few had dramatic stories to tell. Of these thirteen, I made more detailed, tape-recorded interviews with eight. Although the number of responses (either by letter or at the conference) is a small one, it represents a large percentage of those persons in the United States who hold academic or research positions and who engage in or show an open interest in psi research. I should also point out that because I have had nearly ten years field experience with the parapsychology communities in Brazil and in the United States (see Hess 1991), I had fairly good rapport with many of the people whom I interviewed, [p. 228 begins here] and I was able to draw people out based on informal conversations that I had heard previously.

Regarding methodology in general, my goal is not to evaluate the extent to which social control "works" in the case of academic parapsychology; rather, my goal is the more modest one of developing a double typology that will provide a dynamic picture of how disciplinary mechanisms and circumvention strategies fit together. However, as

stated in the introduction, this essay is meant to be more than a presentation of a sociological argument (which could be accomplished in much less space); it is also an ethnographic description of one aspect of the culture of a contemporary heterodox science. Thus, the material gathered is presented in a way that gives some room to the parapsychologists' self-descriptions of their experiences, a procedure which is consistent with a movement in anthropology that calls for greater room for the voices of the informants (see my discussion in the introduction to this volume).

Because the method is case-oriented and anecdotal, I promised to keep confidential any information which the researchers requested that I do not quote, and to this end I have used a letter (A, B, C, etc.) to protect the identity of the parapsychologist whose testimony I am using. This necessity makes it difficult to develop a "dialogical" type of paper, that is, an essay in which I discuss the nature of the dialogue I had with the informants or their relationship to me, topics that could contribute to revealing their identity (see my introduction to this volume). Thus, here is one case where the goals of reflexivity and critique are in conflict, and I have opted to focus on their statements rather than my relationship to them. The latter research question would in any case have been more interesting in a longterm, complete ethnographic project.

I have made a few other minor changes in the identification and texts of the parapsychologists interviewed. To further protect identity and confidentiality, I mailed a copy of an earlier version of this report to the researchers whom I interviewed, so that they had an opportunity to change or delete any passages they did not wish to have quoted. Most made only very minor changes, but one person pointed out that because there were so few women parapsychologists, I might also protect their identity by using the same gender for all parapsychologists. Although the majority of the persons described here are men, I have chosen to represent them all as women. This gender designation was originally suggested to me by one of the male parapsychologists, and it has the advantage of drawing a parallel with the experiences of women in the academy,

who often have their research and credentials undervalued by men in ways similar to the experience of parapsychologists (see Harding 1986: 64). Finally, some of the persons interviewed are not psychologists and others do not regard themselves as "parapsychologists" but instead as physicists, biologists, or psychologists who do some research on "psychic," "paranormal," or "psi" phenomena. [P. 229 begins here.] Consequently, I have used the designation "psi researcher" when referring to specific individuals, but I sometimes retain the more commonly used term "parapsychologist" when referring to them as a collectivity. This flexible and somewhat interchangeable terminology is consistent with usage in the community.

A further note on method regards the question of the categories used within the broader confines of "disciplinary mechanisms" and "circumvention strategies." The discussion of the material in terms of these two processes emerges from the theoretical frameworks discussed above. However, categories within each of these two processes, such as "hindering advancement" or "snickering and ostracism," are concepts that psi researchers themselves recognize in some form, and consequently they may be considered "native categories." However, psi researchers do not themselves consciously state that there are, for example, "four main categories of disciplinary mechanisms." In other words, I have built up a set of categories that represents the broader knowledge of psi researchers as a whole--of the "parapsychology culture"--and not necessarily the individual knowledge of any single psi researcher. This is one task of the anthropological description of a culture: to build up a second level of analysis from the categories of individual members within a culture. However, in order to avoid giving the misimpression that all members of the culture carry with them a reified set of categories (a kind of ecological fallacy or "synoptic illusion," Bourdieu 1977), I also keep track of the individual members of the culture, their own voices, and how these categories are put into action in their life experiences.

Disciplinary Mechanisms

The oral tradition of parapsychology is replete with stories of people whose careers have been adversely affected by their interest in psi research. The few exceptions seem to be explained by extenuating circumstances, as in the case of psi researcher B, whose experience is primarily with non-Western culture:

> Fortunately for me I have not experienced in my professional career of thirty-five years so far any significant adverse effect because of my fairly deep involvement in psi research. It did not affect my advancement in the academic hierarchy. I became a full professor at the age of thirty-five, which was quite unusual in [X, foreign country] at that time when each department had only one professorship. It did not work against me in my advancement in the administrative hierarchy either. I was selected to head Y University as [a high administrative post. This post] in Y university is comparable to that of a university president in this country. I was advisor to the state government on higher education and a member of numerous high-power committees. It did not affect me adversely in achieving professional recognition either. I was elected twice as the President of the X Academy of Applied Psychology...
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> There are various reasons for this state of affairs. First, I grew up in a culture different in its perspective on paranormal phenomena from the Western culture. Second, I never [p. 230 begins here] sought any nonparapsychological positions in the West. Third, I was fortunate to achieve recognition independent of my contributions to the field of parapsychology. It is true that some friends have occasionally expressed the view that I could have made more significant contributions to education and psychology [in X]. I am doing what I want to do most. I am occasionally depressed that the progress in the field is not

> commensurate with our expectations and commitment. But this is a risk one has to take in attempting to explore difficult areas such as parapsychology represents (Psi researcher B).

Although this person's interest in psi research has apparently not resulted in a negative career impact, it is also true that psi researcher B was involved in other research areas and that B also "never sought any parapsychological positions in the West," where she would more likely have encountered prejudice and informal social control mechanisms, notwithstanding her extremely prestigious curriculum vitae.

However, most of the psi researchers who responded to my letter or whom I queried at the conference had at least one negative anecdote to report. I developed a preliminary set of categories from the responses to my letter, and then expanded and refined the typology after the interviews.

Hindering Advancement in the Academy

Although individual psi researchers may gain tenured positions in the academy, as a community they have not been successful in establishing university departments or faculty lines in their field. J. B. Rhine eventually moved his laboratory outside the Psychology Department at Duke University, thus losing the chance to provide institutionalized graduate training in parapsychology (Mauskopf and McVaugh 1980: 304-305). Likewise, bequests made to Clark, Stanford, and Harvard Universities for psychical research were frequently left unused or diverted to other purposes (49-57). In other cases, such as the University of Virginia and Edinburgh University, psi researchers have been more successful in obtaining an institutionalized position for their work, and some of the smaller, alternative universities--such as Atlantic University and John F. Kennedy University--have shown an interest in having courses or programs in parapsychology, although, as I learned at the conference, the program at John F. Kennedy University was being phased out.

As at the institutional level, there is a similar history of failures by individuals who attempt to find a niche for themselves in the academic world. For example, graduate students who reveal an active interest in psi research are discouraged by their advisors. While this is undoubtedly in the student's best interest, sometimes this discouragement seems unreasonable from the student's perspective, as in the following case:

> The subject matter of my dissertation was such that I could have very easily included a parapsychology component without changing the rest of it at all, and I intended to do [p. 231 begins here] that because there seemed to be some connection between the two subjects. My advisor, who is by no means one of the "hard science" people in the department (in fact he is regarded as, and he publicizes himself, so to speak, as one of the "soft humanistic types"...), looked at me when I said I was going to do this and said to me..."If you really imagine that you are going to get a parapsychology component through a dissertation committee, I think you'd better go back and do some very serious reality testing." So, rather than fight that battle, I just dropped that component, but it would not have changed any of the rest of the experiment whatsoever, because it would have been one additional piece of analysis I would have done, and he would not even let me talk about it (Psi researcher I).

There were a few other stories of problems with dissertation committees, but perhaps the most outstanding was the following:

> I made a contract with the university that if, when I sent out the dissertation to the five scientists on the committee, the recommendation was five to zero in my favor, or four-to-one or three-to-two, then I would get the Ph.D. degree. If it was two-to-three, or one-to-four, or zero-to-five, then I would not get the degree. When I sent the dissertation out, it was four-to-one in my favor. However, the university in its wisdom

decided to accept the recommendation of the one who was against me and ignore the recommendation of the other four who said I should be given the degree.

[And the reason for the hesitation was that the dissertation included a component of psi research?]

Apparently, with this negative one. He wrote in his own handwriting in the margin of the first page, "In a proper dissertation, you must have a statement of the problem," and there on the first page, underlined as the heading of a problem, was "the statement of the problem" (Psi researcher K).

At more advanced levels, there are some cases in which psi researchers thought that they were denied tenure at least in part because of their controversial research interests. One case is described by R. A. McConnell, who writes that he was denied tenure in the Physics Department of Pittsburgh University because "my work was not contributing positively to the department's growing reputation in nuclear physics--a judgment with which I could not disagree" (1987: 8; also see 1983a: 236-7). The McConnell case does not necessarily reflect bias as much as questions of intellectual "fit," and he was able to find a position in the Biophysics Department and to gain outside support from the Mellon Trust. However, one of his students who was interested in parapsychology encountered hostile prejudice when pursuing her graduate work there (1983b: 71-116).

A second case more convincingly shows bias in a promotion decision:

A position opened up in an academic department, which I applied for and which I got. I wanted an academic career at that point....[I was doing] humanistically oriented research, but it wasn't parapsychology, although I had the parapsychology in my background. I got the position because a number of the senior clinicians were high on me and I was very well

thought of clinically, and the research looked pertinent and that it would be a nice addition to the department. Other members in the department in other areas endorsed it and went along with it. [P. 232 begins here.]

I learned that not long after that, some of the senior people in other areas expressed misgivings that this had been run through and they didn't realize that this guy had this other stuff in his background. It was also a current interest of mine. What I was doing in parapsychology at the time was that once a year [a colleague and I] taught a course in parapsychology. It was always taught as a special studies course; it was never advertised and we always had ten times as many applicants as we could fill (it was a seminar-type course); we taught it without getting any teaching credit for it (it was always in addition to our other load); and we did it for fun, basically...Although we applied to the curriculum committee to get it placed in the catalogue, we were always rejected, and we were told that if we pushed it, it would be prevented from being offered even as a special studies course.

I learned that some people were really unhappy about this; they felt kind of tricked by it, and I was told... [one colleague] said as soon as she learned this that she would be dead before I stayed at University X. And this was at the very beginning. I was [also] told...that the barriers against my getting tenure were very, very great, and that I had to do an enormous amount of work, so it was really an uphill battle. Now this was in a department that was already getting proud of not giving tenure, since that's sort of a measure of the stature of a department: how hard it is to get tenure....

I remember a faculty meeting at which some curriculum changes were being talked about, and the head of experimental at that point stood

up. He was upset, red in the face, with a rigid rigid posture, and he said, "It's very important that we get the ghosts out of the curriculum." I thought that was a striking metaphor....

Undergraduates would come to me and say, "Dr. So-and-so had said something snide about your interests. What do you think? Why did he say that?"....Then I'd go talk to the guy, and he'd just dance around...It wouldn't be an apology or an acknowledgement; it would just be obfuscation...

I should say, in fairness, that I had hurt my case in another sense, in that I hadn't published as much as I had intended to or as I should have (in other areas) to really make a strong tenure case for myself. I had some good publications, and I had some more coming down, but to some extent I can't blame all of that decision on this prejudice. But I was told by some friends of mine who were on the committee of full professors who make that decision...One man, for example, said, "I was the sore thumb of the department that stuck out." Another guy said--this was all second hand--said someone said, "It would have been better if I had been a practicing necrophilic"...[and another said,] "The only way to have an international reputation of real stature is not to have anyone who is a sore thumb in any way, and I was the sore thumb that stuck out"....

I talked to the AAUP representative, who thought that there was certainly evidence of discrimination and violation of academic freedom, but it seemed clear to me that I didn't really have an overwhelming case for tenure separate from this, and that a lot people would agree that academic freedom had been violated, but there was no way of proving it, and I didn't have the energy to fight it. I also learned something in general about academic freedom. It really only starts with tenure, and tenure

usually is only given to people who prove they won't use academic freedom (Psi researcher G).

Another case reveals how promotion problems have also occurred even after getting tenure:

I didn't start doing work in parapsychology until I got tenure, so although I've seen various forms of resistance, the only way I think it interfered with my career was the first time I was up for promotion to full professor. In a secret ballot, one person on one of the [p. 233 begins here] committees reviewing my case voted against me on grounds of inadequate scholarship. But at no time during committee discussions did anyone ever advance an argument against me for that reason. Colleagues in my department who served on that committee (and whom I am confident voted for me) suggested which of the others might have cast the negative vote, and they felt that it was nothing more than a prejudice that work in parapsychology was inherently disreputable. So, that negative vote was sufficient to sabotage my promotion, despite very strong letters from famous [members of F's home discipline] and strong support, in writing, from every every member of the committee (at the time, our P & T procedures required a unanimous vote for the case to get the Dean's recommendation). This story has a happy ending. I was promoted the next time my case went up (Psi researcher F).

Psi researcher F's experience was relatively benign compared to that of A, who had come out as a psi researcher at the beginning of her academic career but who had also earned a national reputation in more orthodox research areas by the time of her tenure decision:

When I came up for a decision on tenure, several years after hiring, it was denied me. When I had an interview with a Vice-Chancellor to ask for an

> explanation, he told me that the university-level promotion committee had said I had a "marginal" record, and so they were not recommending tenure, although they did not necessarily recommend that I be gotten rid of. At our university, failure to get a tenure[d] position usually means that one only has one year left to find a job elsewhere, however....
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> As word of my denial of tenure got around, faculty members who were involved in the ad hoc tenure evaluation committee and in the university-level promotion committee heard about it and were shocked. Despite the usual rules of secrecy, a person from the ad hoc committee told me their recommendation was quite favorable, and two people from the university-level committee told me the committee had thought I had the strongest record they had ever seen from an assistant professor. Later I found out that a faculty member in my department, since discharged from employment (in spite of having tenure) for gross lack of productivity in the person's field, had gone directly to the [President]'s level to protest the university giving tenure to a person like me who dabbled in parapsychology. Thus, I was lied to at the [President]'s level (Psi researcher A).

Professor A was eventually successful, partly due to the outspoken protest of colleagues and students, and partly because of her circumvention strategies (discussed below). Eventually, a new president was appointed, and he assured A "that I could call on his help, something which I did have to do later, if anything else occurred." Many other incidents did indeed occur in subsequent years:

> The next major incident after that (I won't bother with the minor ones, even though they have wasted a great deal of my professional time) was some years later when we had a new departmental chairman who was also quite prejudiced against parapsychology. In a routine merit increase

> action (which means a difference in rank salary at my university), the verbal report this person gave me about what my senior colleagues had said about my proposed advancement was so negative and so different from what I felt their attitude to be that I suspected I was being lied to. I did complain at the [President]'s level, and, [p. 234 begins here] again after much waste of time and stress on my part, the administration intervened and had a retired member of the department come back to collect anew letters from senior colleagues regarding this advancement, which, as you can imagine, turned out to [be] much more favorable.
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> My memory is now a little unclear as to whether this was a separate incident or a part of the above incident, but this same biased chairperson in a merit action added critical materials about my research (a published attack by a CSICOP member [Committee for the Scientific Investigation of Claims of the Paranormal]) to my file as it was about to go on to college level after I had checked off the file as being complete. More stress and effort on my part to get this removed was needed (Psi researcher A).

Denial of Job Opportunities

Most scientists who go into psi research recognize that openly advocating their research interest may mean restricting job opportunities almost exclusively to privately funded parapsychology labs. As a result, they avoid conflict by not attempting to find work in orthodox academic settings, or when they do, it is often in adjunct positions where a parapsychology course may even be encouraged because it generally brings in large enrollments. However, even the adjunct positions are not entirely unproblematic. Although some people told me that they had been encouraged to teach parapsychology courses in adjunct positions, one mentioned a case in which a proposal for a course on

consciousness studies was accepted whereas a parapsychology course was turned down (and in its place the school decided to offer an astrology course), and another told of how an apparently recalcitrant administration seemed to create enough registration glitches that course enrollments were too low to go ahead with the course.

Psi researchers know that if they are open about their research interests, doors to employment and career prospects are likely to close, and a few anecdotes provide examples of how this occurs at different stages of a career. In the first case, psi researcher J discusses her attempts to get into graduate school. In the school where J is presently completing her Ph.D., having a background in psi research actually helped her, since one of the department members who interviewed her was very interested in the field, but at another, top-ranked graduate program at an East Coast university, the reception was exactly the opposite. J recounts the interview with a famous scientist at this graduate program:

> He was very open. I proposed to him what I wanted to do in terms of looking at mechanisms of [an orthodox research topic]...and he loved it. "This is brilliant," he said. "This is very exciting! Yes, I'm extremely interesting in this!" And then he said, "Now tell me a little bit about your background." So I told him that I had worked at [an organization/laboratory] and so on. Then he says, "Let's go back to this [organization where you worked]. What's that all about?" By this time I was starting to feel really comfortable with him, because he had been so encouraging and he was telling me I'm brilliant: here's [this famous scientist] telling me I'm brilliant.... [P.235 begins here.]
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> I think it was a good project, and from his perspective it was a really good direction to go in, but I didn't mention the psi part of it. Well, at this point I started feeling so comfortable that I said, "Well, actually that's parapsychology." His demeanor, everything, just shifted, and he

said, "Well, I'll tell you that I have an extreme prejudice against parapsychology and anyone doing work in that field, and frankly it's because of people like you that people like me are having a hard time."

...Then he says, "Have you thought about Berkeley? UCLA? I'm sure there's some places out in California [for you]." It's like [he was thinking], "Get as far away as possible!" (Psi researcher J).

A second anecdote depicts a similar situation but at a slightly later career stage. Here, psi researcher A describes how a job offer for position in a research laboratory carried with it some unusual caveats:

[A]fter my first year out from graduate school, during which I'd been a postdoctoral fellow at one of the nation's leading universities, I was interviewed for a position with one of the country's leading researchers in an area dealing with [a more acceptable research area], an area in which I had established a very good track record for independent research. This was a position at a semi-independent research unit, associated with a prominent university. The director, however, stated quite clearly that I could not [do] or publish any research involving parapsychology. I felt this was quite unreasonable of him, but I suggested a compromise: if I published any research on parapsychology I would give only my home address and not use any institutional affiliation that would link my research to his research unit. This was still not acceptable to him. My memory is not perfectly clear on this after so many years, but I also think his policy forbade me to carry out any parapsychological research per se, even if I did it on my own time.

His rationale was that it was already difficult enough working in the area of [research topic] that his lab was involved in and he did not want any further disgrace brought by association with parapsychology. I

could understand this as a technical move but could not go along with it personally since at that time I intended to devote a part of my research time to parapsychological subjects and would not sacrifice my academic freedom. Obviously I did not take the job (Psi researcher A).

Several anecdotes involved bad experiences when job hunting for a teaching and research faculty position:

An old friend of mine had connections at University X, and he had been called, I guess, by the chairman of their search committee because they were looking for someone with a humanistic research interest. This fellow suggested me to them...and then they contacted me, and I did respond with the vita, saying that I would come up [to visit]. They got my vita, which had the parapsychology stuff on it, but I never heard from them. I don't know what the story was on that, but their reaction had changed from really seeking me out in a very warm way to not having any interest at all (Psi researcher G).

In my vita, I have about five or six publications, one of which is with [a leading psi researcher] for a study that he had conducted. I was his assistant and managed to win junior authorship on it. After all, it's one more publication, you know? So I sent my vita all over the place, and at X University there was this one position that I was really eager to have. X University is near me, it's a fairly nice university, and it was one of the best [p. 236 begins here] positions of the ones that had been available at that time with respect to commuting, salaries, and so on. So at one point I called the department and spoke with the chair, who was in charge of reviewing the applications. I said, "Hi, my name is Y, and I'd like to find out what the status of my application is." He says, "Y--oh yeah! You're

that guy who's interested in parapsychology. No, I'm sorry. There were two or three other applicants ahead of you." I'm sorry that I didn't record that telephone conversation, because the way in which he expressed what he said was as if he said, "Oh, you're that asshole who's into partying. What a jerk! Get out of here!" (Psi researcher M).

I believe based on my experience that [psi research] has an adverse effect [on one's career opportunities], certainly in the psychology area job market. I can cite one specific example in my own experience--very exasperating--in which I went to the employment [bureau] at a regional conference. I arranged ahead of time to have an interview there with someone from a certain university. Of course they interviewed many people, and I starting talking, and it didn't take more than a minute and a half for the fellow to say, "Oh, you're the one who's been involved in parapsychology." After that, he never wrote one word in his little notebook, and of course I knew what that meant, and I wasn't offered the job. I can't be sure that was the reason, but nevertheless I rather am inclined to doubt that he would have stopped writing in the case of most of the other candidates...I actually haven't interviewed for a lot of academic jobs, so I wouldn't want to generalize too much on the basis of one experience, but all I can say is that it is really possible that it can be prejudicial.... (Psi researcher L)

Psi researcher L goes on to make an interesting observation regarding an indirect way in which psi research may affect job prospects. While L admits that prejudice or hostility may be a factor, there is also a more general consideration: by doing psi research, scholars narrow their field of expertise and thus their employability in a more conventional sense:

> [A]s I've come to know from working in the academic field, when you hire for a position, you're usually interested in someone who can serve a very specific function for you. That function ordinarily does not center around parapsychological concerns. For this reason, a parapsychologist who's devoted most of his or her time to working in this field may not appear to a search committee to be the best person for a typical academic job. Sometimes parapsychologically-oriented applicants may think that their not being hired is due to prejudice against parapsychology, but an alternative interpretation is that they are simply not perceived as the best qualified for the particular position (Psi researcher L).

Snickering and Ostracism

Anthropologists and sociologists have long recognized gossip as an effective social control mechanism, but I did not encounter many anecdotes of gossip, snickering, or ostracism. I think this is because in most cases psi researchers end up staying in departments or research organizations where their research interests are tolerated (either out of sympathy or out of commitment to academic freedom), so their relations with their colleagues are generally harmonious. There are, however, some circumstances in which [p. 237 begins here] snickering occurs, one of them being when psi researchers leaves their home turf:

> Now sometimes I get invited to lecture at other colleges and universities, but again there's a very selective factor here. The fact that I'm invited to lecture at these places indicates that they know about my interests in parapsychology, expect me to talk about it, and are either favorably disposed toward it or open-minded. Now occasionally there may be a small number of people in the department who do not like what I'm saying, but the responses there are rarely confrontational; they are usually

comments I hear about after my visit is over. At one college where I spoke, a friend of mine was sitting next to a psychology professor, and she whispered to him, "Either this man is a charlatan or he's hopelessly psychotic" (Psi researcher H).

Psi researchers may be more likely to hear snickering and experience ostracism from their colleagues when they adopt the strategy of "coming out" at some later phase in their career, usually after tenure. When this happens, the social situation can be extremely antagonistic. Psi researcher F describes this situation and likens it to divorce:

> I have had to endure lots of snide remarks from other academics about my interests. And although they make me angry, I would have to say that I'm grateful for what they reveal. I definitely have a clearer picture of the intellectual caliber of my peers than I would if I had been working in a less controversial area. I've seen many varieties of intellectual cowardice and dishonesty, sometimes in those from who I had expected better. And I've likewise been pleasantly surprised by those whom I had not held in very high esteem. It's a little like getting a divorce; you find out who your friends really are, and what people are really made of (Psi researcher F).

Destruction of Data

Perhaps the most impressive anecdote, and one which is to my knowledge unique, is a case in which a hostile department chair destroyed a junior colleague's data from a parapsychology experiment:

> I did a study of five thousand people. It was precognition, and we used IBM cards. They had ten holes which can be punched in a column, and the person has to choose one and then go on to the next column...and eventually do one-hundred guesses of a number which the computer will generate randomly after the person has made their guess, so it is a

> precognition experiment. My colleague got a transfer to another university and left with his whole family. On the day he left, the office that I had been in for fifteen years was closed to me, and all the IBM cards were sent down to the incinerator, by the chairman of the department, [where they were] burned....He couldn't stand that there was a person in the department doing ESP research, but so long as my colleague was there, he couldn't stop him (Psi researcher K). [P. 238 begins here.]

Summary and Discussion

The university, a highly bureaucratized institution akin to a prison or asylum (especially at the end of the semester), has developed a series of disciplinary technologies that regulate behavior through a routinized system that can be compared to what Foucault has described as "gratification-punishment" (1979: 180). Unlike the gatekeeping functions of funding and journal publication, which operate to control the access of the field of parapsychology as a whole to scientific status, these disciplinary mechanisms operate at the level of individual career trajectories. Thus, many of the same mechanisms described by Foucault as examples of the "normalizing" aspect of institutions can be seen at work in the contemporary academy: the distribution into gradations of rank, the surveillance of promotion committees that may be compared to Foucault's discussion of the panoptical procedures of examinations, and the use of documentary techniques that make individuals into a "case" or, in the parlance of the contemporary academy, a "file" (see 1979: 181-191).

However, the disciplinary perspective applies best to the first two categories: the closely related mechanisms that govern the denial of degrees, promotions, and job opportunities. The other two categories, gossip/snickering and destruction of data, point to another aspect of the academy and the social relations of academic science that could arguably be described as premodern. Although it is reasonable to describe universities

and scientific disciplines as highly rationalized bureaucratic institutions, they are also relatively small communities (particularly when one considers departments and subspecialties as crucial units of social interaction), and consequently they also have a small-town or village quality with great deal of face-to-face interaction. Thus, given the rapid nature in which gossip spreads through and across departments and fields, decisions to deny positions or promotions have a spectacle-like quality. While clearly these spectacles--with the possible exception of the story of destroyed data--cannot be compared to the form of corporeal punishment that Foucault vividly describes in the opening pages of *Discipline and Punish*, the tales of rejection, exclusion, and even ostracism circulate in much the same way that tales of public executions once did. Furthermore, the unwanted scientist may be condemned to wander about, to seek employment as an "academic Gypsy" outside the walls of the academy, in much the same way that the "mad" in Europe during the period preceding the "great confinement" were condemned to wander outside the walls of the city (see Foucault 1973). In short, we might do well to take seriously metaphors that describe the contemporary academy as a "Byzantine" institution or, better, a labyrinth of spectacles and disciplinary technologies. [P. 239 begins here.]

Strategies of Circumvention

Although the Foucauldian framework (and the broader sociology of social control) provides some insight into the "microphysics" of power in the academy, the responses of the psi researchers in my interviews also reveal that they are by no means docile players in the social dramas they enact. Indeed, psi researchers, like all good scientists and social scientists, are also entrepreneurs of a sort who are well aware of how their research interests affect their academic capital, and thus the sociology of strategies developed by Bourdieu (1975) might serve a useful touchstone to an analysis of

disciplinary mechanisms. Perhaps even more to the point is the discussion of subversion by de Certeau (e.g., his discussion of "ways of operating" and *la perruque*, 1984) or DaMatta (e.g., his discussion of the *jeitinho*, 1984), and the discussion of "options for dissidents" by Martin et al. (1986: 243-252). In short, to a typology of disciplinary mechanisms and social control mechanisms, one needs to pose a countertypology of tactics and strategies of circumvention.

Opting Out

The most obvious strategy, and one which several psi researchers have chosen, is to decide to leave their academic or industry positions in order to pursue psi research full-time in one of the privately funded parapsychology institutes or foundations. Opting out may involve combining a willful decision with the realization that it may be better to walk before they have to run. In a sense, the decision beats the system through self-discipline. For example, psi researcher C wrote:

> I simply assumed when I entered parapsychology on a full-time basis that I was "burning my bridges" as far as attractive appointments in conventional psychology were concerned, so I never pursued any. Although I was aware of conflicts of the type you are interested in at two of the places where I worked as a psi researcher [names of universities deleted at the request of the writer], I was not involved in either of those conflicts (Psi researcher C).

Laying Low and Being Collegial

In the North American academy, Marxists and feminists face similar problems to parapsychologists, and I remember once having a conversation with a radical historian who had received tenure in a conservative department not known for his brand of history. I asked him if this had been a problem, and he said "No, I just laid low for six years."

This is almost the same terminology that psi researcher I--the one who had the trouble with her dissertation advisor--used: "I laid low and established my credibility as a good graduate student." [P. 240 begins here.]

In a similar way, psi researcher C gives another example of "laying low" while in the academy:

> Before I got into parapsychology, I did not hide my interest in the field but I did not flaunt it either. During college and graduate school, there were no adverse effects at all. My only academic job in conventional psychology was at X and began immediately after I got my Ph.D. Other than sponsoring a symposium in parapsychology, I did not discuss my interest in parapsychology with most of the faculty or students. I think that some faculty members were uncomfortable with my interest and this may have contributed to the social isolation I felt there, but I left voluntarily before there were any overt manifestations of prejudice (Psi researcher C).

While C goes on to state that "Intolerance of my interest in parapsychology, if indeed there was any, was not a factor in my decision to leave," C also indicates that she felt compelled to hide this interest or at least not to flaunt it.

Another aspect of the "laying low" strategy is playing the role of the good departmental citizen, which may mitigate the controversial nature of one's research. For example, psi researcher D, a tenured professor who is renowned for an easy-going sense of humor and low-key demeanor, writes,

> Much of the bias present in my environment has probably not been very overt, as I tend to be fairly quiet and easygoing, and probably don't provoke people into revealing their position. I tend not to make strong statements about my work. So far things are not bad locally, and I

> probably profit from the positive bias people show (at least publicly) to someone who holds the rank of professor (Psi researcher D).

A related strategy that minimizes differences with one's colleagues is to adopt a rationalist stance toward psi research and subject it to the same language and methodology of orthodox scientific research. In other words, this strategy minimizes parapsychology's differences from other areas of potentially controversial research. For example, E speaks of a "sweetly reasonable" strategy:

> My general strategy when students did research on psi under my direction was to criticize it with the same standards as any other research. If anyone else criticized it on an a priori basis I cited academic freedom to justify conducting any well controlled and legal project that happened to interest the research worker. Minor strategies were rephrasing everything operationally (forget the why it happens, just say what happened), citing meteorites (stones can't fall from the sky) and tides (action at a distance). The general approach was being sweetly reasonable, and it worked--in this setting--to nullify overt opposition to psi. Students reported to me, though, that in private--in [their] own classroom or office--some professor might sneer at psi even though apparently he didn't dare do it in public. That means the strategy was only partly successful (Psi researcher E).

[P.241 begins here.]

A's rationalist strategy also brought only mixed results:

> As to strategies I have adopted to mitigate the stigmatizing effects of doing parapsychology, they are many. The primary one is to make sure that the research I do is of the best possible quality, given my (limited) resources. This is a "rational" strategy that does not, of course, always work (Psi researcher A).

However, H seems to have been more successful:

> I think that part of this [success] of course is my own particular stance in the field, which I don't think is at all divergent. I think it's mainstream in terms of parapsychology, because it's in accordance with our published statement on definitions and terms. This is a position that makes no radical statements about parapsychology or its reported phenomena; it's a position that sees one field as still in the process of changing and evolving; it's a position which is even very unhappy with the word "extrasensory perception" as one of the terms because there is so much theoretical baggage that goes along with that term. So this very moderate, soft-sell approach makes parapsychology a little more palatable to some of the people who are not overtly hostile to the field (Psi researcher H).

Probably a large part of H's success is due not only to a rationalist stance and refusal to make exaggerated claims, but also a demeanor similar to what E described as "sweetly reasonable." As H phrases it:

> You see, I don't ask for trouble. I have enough problems in my life one way or another that I try to put what energy I have into arenas and projects that I think will provide a pay-off without shedding too much blood in the process (Psi researcher H).

Diversifying One's Academic Portfolio

Several psi researchers who have succeeded in gaining a respectable position in the academy have done so because they have first established themselves in some other area of research. For example, psi researcher B writes,

> Parapsychology is not yet a mature field in that it has few openings for full-time involvement. Therefore, for most who are interested in parapsychology, it will remain a part-time endeavor. In such a situation, it

> is essential that one aims at a certain level of eminence in another area as well so that [s/he] has [a] reputation that goes beyond his parapsychological contributions. This will not be easy, but is something that seems necessary until parapsychology comes of age, so to speak (Psi researcher B).

Several psi researchers discussed diversification strategies, for example researcher J:

> I do have some other publications besides parapsychology now, and I do have a book [on an orthodox scientific topic]....I have done a lot of things to structure my life in a way that legitimates me as a person. I wrote this book because I wanted to show that [p. 242 begins here] I could do something else, and the same thing with the dissertation, which I want to be a good dissertation that has absolutely no relevance to parapsychology (Psi researcher J).

Related to the diversification strategy is the decision not to "come out" as someone interested in psi research until after having secured tenure or being securely established in some other area of research. For example, Psi researcher F did not start conducting psi research until after tenure, and F's strong record in her home discipline has helped inoculate her against the pollution of psi research. Regarding this point, F writes:

> Probably one reason I haven't had much trouble in this area is that I had established myself--before turning to parapsychology--as a respectable [another discipline]. So, some folks in the field said to me things like, "Well, I'm at least glad that someone like you is doing the work." I don't doubt that professional recognition or acceptance may be withheld from those who haven't "proven" themselves first, according to criteria which their field takes to be important (Psi researcher F).

Unlike psi researcher F, psi researcher A "came out" as a psi researcher early in her career (before tenure), but A carefully diversified and established a reputation in other areas of "frontier" psychology:

> When I was first hired by the university I'm currently employed at, I had already published several parapsychological research papers and it was known to the faculty in my department that I would probably do some more work in this area, although, I was later told, some of the older faculty who voted to hire me hoped I would "outgrow" this unsuitable interest. At that time approximately one-third of my research efforts were in parapsychology and the other two-thirds with frontier, but somewhat more acceptable research areas, within psychology.
>
> Over the next several years, I published a large amount of work in all of these areas, some of which gained national recognition and which firmly established me in my areas of work (Psi researcher A).

Professor A's diversified scholarly base then stood her in good stead when she had to fight her tenure and promotion battles described above, but A also recognizes the limitations of this strategy:

> ...I have an international reputation for research in these other fields and I'm better known for it among many professionals than for parapsychological research. Unfortunately, these are cutting edge fields, so this does not serve to identify me as mainstream enough to avoid prejudice (Psi researcher A).

This statement involves the interesting suggestion that a diversification strategy may not be altogether successful if the researcher diversifies into related fields that may also be controversial or of low prestige. Instead, in order to mitigate the effects of doing parapsychology research, one would have to be distinguished in a much more mainstream or higher prestige field of research. [P.243 begins here.]

Finding a Tolerant Department or a Powerful Mentor.

Psi researcher E notes that her department was "rather aggressively and self-consciously liberal about blacks, women, and academic freedom," so in general she experienced few problems. E went through promotion and tenure at "the minimum conventional intervals," and she was even "taken into Sigma XI with the other experimentalists in the department." E also had the backing of a mentor who was highly respected in her own discipline, and he helped her to find her first job (although after that E earned her promotions on her own). She writes,

> In 19xx, he [the mentor] persuaded the department at Y that he chaired to hire me for one semester as an instructor...and in a year or so be rehired at Y....Because of [this mentor], psi research actually helped me in the job market. (Psi Researcher E).

However, there was one incident where E's parapsychology interests adversely affected her standing in the university:

> When [a graduate school at Y] was formed, the doctoral faculty was constituted and I was excluded. The department chairman told me it was because of parapsychology. He objected; I was refused again. Next year he tried again, with a strong supporting letter from [the mentor/department chair] and some other prestigious supporters. This time I was accepted (Psi researcher E).

The mentor was able to help E out in the time of crisis, but once E was given this opportunity, it appears that her competence as a scientist and collegiality as a department member--as well as her genuine interest in most areas of psychology--were the factors that most contributed to overcoming the prejudice against her research:

> What's odd is that thereafter the doctoral faculty seemed to accept me. I think what happened was that pro forma they gave me committee

> assignments and invited me to colloquia. I had a genuine interest in most of the specific topics as well as the research designs; the rest of the faculty liked the comments I made. They tested me with small nuisance committee jobs and I did them; they kept giving me bigger jobs. By then they were having me teach doctoral courses; these were well received. Eventually to my astonishment they invited me to give a doctoral course in parapsychology at the [graduate level], and of course I did (Psi researcher E).

Several other parapsychologists (D, H, and L) report that their departments and universities have encouraged their research, and H explains that she actively sought out university environments where her interests in parapsychology would be tolerated: "I've only had two major academic appointments in my career, and one of the reasons I chose them the way I did was so that I would not have problems with my parapsychological interests." [P. 244 begins here.]

Appealing and Going Public

When sanctions adversely affect psi researchers' careers, they may appeal to academic freedom, a value which may rest more on the American sense of first amendment freedoms than on the Mertonian "norms" of disinterestedness and universalism (Merton 1973). Although social scientists have questioned how much Mertonian norms in fact operate for science in general, the "norms" still serve as a ideology that scientists use to legitimate their activities (see Barnes and Dolby 1970; Ben-Yehuda 1985: 171-174; Pinch 1979; Pinch and Collins 1984; Mitroff 1974; Merton 1976). Consequently, even if Mertonian norms are not "norms," they exist as part of the ideology of science, and a heterodox scientist may obtain a certain degree of power by threatening to expose--either directly or indirectly--that orthodox scientists are not in fact living up to their ideals. For example, psi researcher E describes the following situation:

> When the APA's [American Psychological Association's] structure changed, I along with other members of SPSSI applied for fellowship status. My application was refused. I happened to know the current president of SPSSI, and he told me it was because of my ESP research. I reapplied, writing a letter that was falsely naive. It listed the way I met formal qualifications, suggested the previous refusal was a clerical error, and then suggested as another remote, inconceivable possibility that the refusal was due to denying me the academic freedom to experiment on a topic of my choice. In the next few months I was accepted as a fellow, with no explanation (Psi researcher E).

While the appeal to academic freedom may not have much power in itself, it carries with it the possibility of going public and creating a scandal. Thus the appeal to academic freedom, even without the explicit threat of going public, may be a very effective strategy of circumventing social control mechanisms. Of course, in order to use this strategy, heterodox scientists must be sure that their research will withstand public scrutiny, since the scandal will bring the scrutinizing eye of the scientific community or public on both the orthodox scientists and the heterodox scientist.

Psi researcher A, who had an extremely good scholarship record, was denied tenure, adopted a similar strategy:

> I had a couple of days of considerable depression, for I knew that even if my parapsychological work was totally ignored, the rest of my more conventional work was still a very good record. Then I became extremely angry and resolved in my mind that the University would give me tenure within the next two weeks or I would resign in a high profile way by calling a press conference to denounce the University's prejudice....
>
> My rational arguments had no effect on subsequent developments. What I did do was tell the administration every few days that reporters

were calling me because they had heard rumors that something was going on, but I was trying not to talk to them. Also, all my colleagues but the one mentioned in my department sent in a strong letter of protest, all [p.245 begins here] of the graduate students in the department did so and also went en masse to the [President]'s office at least once, possibly (my memory is vague here) several times to protest, and numerous students from my large (400+ students) undergraduate classes also signed petitions and went in person to protest this.

A major external factor was the outbreak of student activism, which frightened many universities. This was the year of the [major student demonstration].

As pressure on the administration built up, they gave me a story (which I think was a complete lie) about how, in their liberality, they were getting an outside consultant to read my publications, but it might take several months for him to read them as they were so voluminous.

The final result was that the day before the two-week deadline I had set in my mind, the Vice-[President] who had lied to me before about the report on my record gave me a cock-and-bull story about how the ostensible outside consultant had suddenly been able to read all of my papers and had pronounced favorably on them, so they were now giving me tenure.

I learned a great deal about how little academic freedom is actually cherished and how much the secrecy involved in these kind of university processes can protect corruption (Psi researcher A).

Another strategy which psi researcher A has used has been to appeal through existing institutional mechanisms. However, if the various bureaucrats in the institution

are prejudiced against parapsychology, this can be a frustrating process, as A goes on to narrate:

> The final incident I will mention is currently in progress and has dragged on for three years. Again I was up for a routine merit increase, and all the senior faculty entitled to vote on me did vote positively, although a couple of them included some negative comments showing some prejudice against parapsychological matters. The Dean of the college denied my merit increase in spite of the unanimous vote. He then denied an appeal of this initial denial.
>
> In the appeal I indicated, among other things, that since there were no faculty on my campus competent to judge my work in parapsychology (and a second area of work) , it would only be fair, if he had questions about the competence of my senior colleagues who had favorably assessed my work, to bring in outside experts from parapsychology and the other field. He never replied to this issue.
>
> I applied a second time for the same merit increase, with even more publications. This was denied, and the appeal of this denial was denied. I then appealed both of these denials to the [President]'s level and this appeal was also denied. In any [case], no one paid any explicit attention to the issue of having competent people evaluate my record.
>
> I am currently applying for this same merit increase a third time. I am also appealing to our academic senate committee on privilege and tenure to overturn the initial denial, and their first reaction has been in my favor. I don't know what will happen, however, as this committee can only recommend to the administration, not force any change.
>
> As you can imagine, all of these incidents have created a great deal of stress in my life and have wasted enormous amounts of time. I imagine

> I could have written a couple of new books in the time that I have spent trying to have the university play by its own ideals and rules (Psi researcher A). [P. 246 begins here.]

The strategy of appealing and going public can ultimately slide into legal options, which psi researcher K considered when her university refused to honor the dissertation committee's four-to-one vote in her favor:

> I didn't know how to deal with this situation. I consulted a very tough lawyer and we planned the strategy that we would use when it came to a court case. At that point, my health broke down, and I was in bed for ten weeks with a temperature of 103º...What happened at about the tenth week is that a very important person in the field heard about my story and decided to take action and write a very, very stiff letter about this and send it around to very important people in the field. The university was put under heat with this, and they couldn't take the heat, so what happened after a little while was that they decided to abandon this one no-sayer and go by the recommendations of the other four and let me continue on to the orals and let me get the degree (Psi researcher K).

Summary and Discussion

Although the majority of psi researchers reported at least some instances of prejudice, and some experienced dramatic episodes of suppression (as revealed by the anecdotes), there were also many cases where they reported few if any problems. A pattern seemed to emerge in these cases: those who experienced few problems and little prejudice and who were relatively successful in their academic or research institutions were people who had the qualities one would expect in successful academicians and scientists. In other words, in some cases colleagues may be willing to overlook a psi

researcher's rather unorthodox research interests if the psi researcher is valued as a teacher and colleague, or as a fellow researcher in a more orthodox field.

There seem to be three criteria for avoiding some of the disciplinary mechanisms discussed above: (1) psi researchers should be methodologically sophisticated, established in, and knowledgeable about one or more areas of orthodox scientific research; (2) they should be intelligent, engaging, confident, and unabrasive colleagues--in short, the kind of colleague the faculty and student would want to have in one's department; and (3) they should find a department where their research interests are either tolerated or welcomed (as might occur more easily in some alternative or religious colleges and universities). As psi researcher L states,

> I honestly believe that in almost any situation if a person can get hired for a job despite the fact that they are involved in parapsychology, my guess is that--and this is just a guess--in most situations that if one is not standoffish in one's dealings with other faculty members, and is a team player and has an interest in the things that they are concerned with (which has really nothing to do with parapsychology), if one has genuine interest in the things that the department is concerned with and what they're trying to educate about, [then] I really don't think there are going to be many problems (Psi researcher L). [P. 247 begins here.]

Indeed, successful psi researchers occasionally mentioned to me that some of their less successful colleagues may blame their own failures on prejudice, when in some cases they would probably not have been successful even in a more orthodox field. Certainly, however, this would not apply to all cases, as is revealed in some of the extreme examples of prejudice documented here.

Conclusions

Parapsychologists regard themselves less as deviants than as reformers who abide by the rules of science and reveal anomalies in established ways of seeing the world. Some academic psi researchers may deviate from the values of materialism and mechanism (since some follow Rhine and support a version of mind/body dualism as an appropriate framework for understanding extrasensory perception and psychokinesis), but they also believe that orthodox scientists and skeptics deviate from ideals of open-mindedness and impartiality. Indeed, the phrase "corruption" even turned up in parapsychologists' discussion of how scientists' deviate from their professed ideals. Parapsychologists may find ways of mitigating their pariah status, but the solutions sometimes achieved on an individual level should not blind us to the broader social phenomena that these interviews reveal in detail: parapsychology is a taboo science. Why is that the case?

A starting point for the solution appears in Gieryn's discussion of boundary-work (1983), which revealed some of the dynamics at stake in the contest between science and religion for authority in society. Given the long and cozy relationship between parapsychology and religion (see Hess 1992), the historical basis of the taboo nature of parapsychological inquiry can be framed in terms of the struggle for authority between religion and science. Parapsychology is, in the semiotic term, a "marked" category; it is a science with a difference, a science "set apart" from the mainstream of orthodox science, not because it is "sacred" in the Durkheimian sense, but instead because it violates a taboo. To practice parapsychology is to violate a taboo that calls into question the authority of scientific knowledge with respect to other rivals for authority in the ideological arena. Religious groups (and some parapsychologists) can argue that the anomalous nature of parapsychological phenomena validates or at least lends credence to the claims of religion to some kind of extrascientific (or "parascientific") knowledge or experience. Although not all parapsychologists today would accept the close linkage between parapsychology and religion as it was formulated by the early psychical

researchers and by J. B. Rhine (see Rhine 1953; Hess 1992), they still labor under the taboo status engendered by this construction of their field.

It matters little that many parapsychologists eschew any connections between their field and religion, and they see themselves as merely asking [p.248 begins here] research questions about a controversial area of claimed phenomena. What matters is how they are perceived by their more orthodox colleagues, and the perception is likely to continue until parapsychologists are able to postulate a "physical" explanation for the controversial phenomena they claim to document. Once scientists perceive parapsychology to support nonscientific rivals (such as religion) to the authority of science, then parapsychologists become "marked" as practitioners of a taboo science. In the process, parapsychologists become "marked" scientists in a second, more mundane sense of the term. Parapsychology legitimates religion, and because, even today, science and religion still contest their relative position as a source of authority in society, parapsychology is "marked science" and parapsychologists become "marked scientists."

Thus, conflicts over the legitimacy of psi research in the academy can be seen as a further extension of boundary-work between science and religion. In other words, attempts to demarcate boundaries among scientists reproduce those made between scientists and nonscientists. We find, then, that the boundary between the scientific and the nonscientific is multiple and nested. This becomes particularly clear as one extends the analysis toward popular religion and other nonscientific discourses, as Gieryn has implied in his research and as I have done in more detail in *Science in the New Age* (1992). Here, if one adopts the anthropological perspective of attempting to understand how science is constructed from the point of view of different communities (not all of which are orthodox scientists), almost everyone is revealed to be drawing the boundary between the scientific and nonscientific in different ways.

Boundary-work between the scientific and nonscientific also becomes part of the discourse within the interdisciplinary field of science and technology studies. Here we

can turn the analysis back reflexively on ourselves to inquire into the meaning of a term sometimes used to label heterodox sciences such as parapsychology: "deviant science." (Examples of uses of this term include Dolby 1979, McClenon 1984, Ben-Yehuda 1985, 1990.) Ben-Yehuda defines "deviant science" as "science that by virtue of its hypotheses or methodology is regarded by the relevant scientific establishment as deviant," and parapsychology is one example (1985: 106, 120). Deviance, in turn, involves some notion of a "violation of norms," and definitions over what constitutes deviance therefore involve clashing "symbolic-moral universes" that have political as well as moral dimensions (1990: 4).

Social scientists who characterize some sciences as "deviant" may intend to be relativistic; that is, they may define deviance not according to (claimed) asocial philosophical criteria but only relative to scientific orthodoxy in a given historical and cultural context. However, I find the term problematic for several reasons: (1) experimental parapsychologists with whom I have spoken bristle at the appellation "deviant science" and reject that there is anything deviant about their scientific practice; instead, their view of their research is, at least [p.249 begins here] methodologically, that "nothing unscientific is happening," to borrow the phrase of Collins and Pinch (1979); (2) if deviance may be defined as a "violation of norms" (Ben-Yehuda 1990: 4), it has long been recognized that it is unclear to what extent science is normative (e.g., Barnes and Dolby 1970, Mitroff 1974), and therefore it is unclear how meaningful it is to speak of "deviant science"; (3) the term "deviance" suggests comparisons with criminology and the sociology of "deviance" rather than with political ideology, religion, and the suppression of minority or heterodox viewpoints; and (4) as is certainly evident by this point, there is some evidence that the critics of experimental parapsychology have engaged in rhetorical strategies and intellectual practices that might be considered to violate the so-called norms (or, as I would prefer, the "ideals" or "ideology") of scientific conduct (see also Pinch and Collins 1984; Rockwell, Rockwell, and Rockwell 1978). On

this last point, Ben-Yehuda flags a similar issue when he makes a distinction between "deviance in science" (fraud, fabrications, falsifications) and "deviant science" (1990: 182), but more generally one confronts the paradox that a so-called "deviant science" may be nondeviant at least at the level of methodology, whereas so-called "nondeviant science" may in some sense be deviant in terms of methodological issues (such as the case discussed here where one scientist burned a parapsychologist's data).

For these reasons, I have preferred to use the term "heterodox science" (1987, 1991) in place of "deviant science" or even, as Wallis has suggested, "marginal science" (1979). The term "heterodoxy" has the advantage of signaling sciences that are relatively unacceptable to those scientists who control the means of scientific production and reproduction. Furthermore, the term suggests parallels in the social studies of ideology and religion rather than of criminology. It is possible that all three terms might be used for different analytical purposes: deviant, for violations of accepted codes (that is ideals, not norms) of scientific practice (such as fraud, corruption, or unprofessional experimental design and analysis); marginal, for a rather unimportant position in the status and funding hierarchies of science; and heterodox, for anomalous claims that violate existing scientific perceptions about the world. Thus, a science such as parapsychology is heterodox, and many parapsychologists are marginalized, but in general its experimental designs are not deviant with respect to the standards articulated by other experimental psychologists. Likewise, defenders of scientific orthodoxies may engage in so-called "deviant" practices in order to debunk heterodox sciences or maginalized colleagues. Furthermore, a science may be heterodox even if many of its members do not occupy a marginalized position in the social structures and power hierarchies of contemporary science. The triad of terms--heterodox, deviant, and marginal--might be used to give more analytical power and precision to social scientific discussions.

However, more is at stake here than a question of terminology or even methodology and theory in the study of heterodox sciences. The very use of [p. 250 begins here] the term "deviance" may tell us something about the social studies of science and technology. In the field of STS, social scientists who study heterodox sciences may themselves become "marked." Contact with heterodox sciences is so polluting that a member of the STS field who is engaged in a sociology or anthropology of a heterodox science--that is, someone who is not making claims about the validity or lack thereof of a heterodox science--may encounter various kinds of sanctions that make doing the social studies of heterodox science not only uncomfortable but also dangerous. I have heard more than one claim from social scientists that their study of a heterodox science such as parapsychology was not the best career move. At the minimum, with the "turn to technology" in STS (see Woolgar 1991) the subject may have acquired a backwater status.

Thus, another instance of boundary-work emerges, now within the science of science and technology. Parapsychology provides a fine litmus test for social scientists' commitment to principles (ideals?) such as symmetry and relativism. Many seem perfectly happy with the strong program and constructivism until someone comes along and treats a taboo science like any other science; then, suddenly, the social scientist's discourse frequently shifts to ad hominen questions (What's your agenda? Do you believe it?) or nonconstructivist arguments (Well, they're suffering from disciplinary mechanisms because, after all, they don't do real science) to simple value judgements (Well, they *should* be kicked our of the academy! Or: Why study that crock of ----?).

Even when the discourse does not shift to such unsophisticated arguments and comments, more subtle kinds of sanctions are possible. For example, in this essay I have left unopened the "black box" of the content of parapsychological facts, theories, and methods. As a result, those whose primary interest is the social construction of knowledge and technology might find my essay uninteresting or, worse, "neoMertonian." But I

choose to open boxes of my own, ones that neither Merton nor many constructivists seem very interested in opening: boxes of marginalization and of culture, meaning, and power. In STS feminist critics tend to open such boxes most consistently, and I have chosen to flag an elective affinity with them by gendering as female the psi researchers I interviewed. My critical, cultural perspective would thus apply not only to the processes of marginalization and circumvention within science, but also reflexively to the social studies of science and technology. I ask: to what extent does STS tend to replicate social and ideological hierarchies that would exclude visions of knowledge rooted in radical social movements, feminist critiques, consumer and popular perspectives, non-Western cultures, underrepresented ethnic groups, and even religious experience and oral tradition? Let us open not only black boxes, but also red boxes, brown boxes, purple boxes, and a rainbow of other boxes. [P.251 begins here.]

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