Sociological Forum, Vol. 20, No. 1, March 2005 (© 2005)
DOI: 10.1007/s11206-005-1896-1

Seeding Science, Courting Conclusions: Reexamining the Intersection of Science, Corporate Cash, and the Law[1]

William R. Freudenburg[2]

Social scientists have expressed strong views on corporate influences over science, but most attention has been devoted to broad, Black/White arguments, rather than to actual mechanisms of influence. This paper summarizes an experience where involvement in a lawsuit led to the discovery of an unexpected mechanism: A large corporation facing a multibillion-dollar court judgment quietly provided generous funding to well-known scientists (including at least one Nobel prize winner) who would submit articles to "open," peer-reviewed journals, so that their "unbiased science" could be cited in an appeal to the Supreme Court. On balance, the corporation's most effective techniques of influence may have been provided not by overt pressure, but by encouraging scientists to continue thinking of themselves as independent and impartial.

KEY WORDS: punitive damages; science funding; legal precedents; court decisions; bias; peer review.

INTRODUCTION

A funny thing happened to me one day when I picked up the telephone. I learned something new about the mechanisms of corporate influence in science.

Perhaps I should clarify. The topic is scarcely a new one, and even before I picked up the telephone that day, I considered myself to be

[1]This is a revised version of a paper presented at the annual meeting of the American Sociological Association, Atlanta, GA, August 2003.

[2]Environmental Studies Program and Department of Sociology, University of California–Santa Barbara, California 93106-4160; e-mail: freudenburg@es.ucsb.edu.

reasonably familiar with several bodies of relevant literature that I summarize in this article—ranging from critical constructivist literature in the sociology of science to work on law and society and on the political economy of science. That day, however, I was about to start learning about forms of corporate influence that were in some ways broader and in other ways far more specific than anything I had ever read about. They were broader in the sense of having less to do with the profits of a single company or industry than with questions of legal liability in general, but they were also more specific, being intended to "seed" the content of peer-reviewed scientific journals in ways that would shape legal decisions—not at the outset, but in decisions by federal appeals courts and the U.S. Supreme Court.

As the following pages show, I quickly sensed that the unexpected telephone call might provide a rare opportunity for ethnographic insights into the dynamics of influence, but what would not occur to me until years later, and after further reflection, would be the conclusion I now hold: Perhaps the most important sources of potential bias would actually be the opposite of the ones that worried me that day—not that this corporate benefactor would expect me to sell my soul, or more prosaically, to slant my conclusions to serve corporate goals, but precisely the reverse—the very fact that he and his company took such pains to express their appreciation for my independence and professionalism.

I return to this point near the end of the article, but I begin with a summary of the earlier literature. Following that, I return to that unexpected telephone call, and to the learning experience that ensued, before reflecting on some of the implications that emerge.

CORPORATE CASH, SCIENCE, AND THE LAW

For readers not already familiar with existing analyses of the relationships between corporations and science, a useful starting point is to recognize that the topic appears to have been discussed far more often by those who consider it worrisome than by those who see it as reassuring. Although relatively few concerns are raised in some subsets of the existing literature, such as the classic or Mertonian tradition in the sociology of science—where most authors see corporate intrusions into the orderly progress of science as inappropriate but relatively rare and likely to be ironed out over time through normal processes of scientific self-correction (e.g., see Merton, 1973; but see also Lawless, 1993)—the more common patterns involve expressions of concern. Many of the concerns, however, have to do with the potential *extent* of corporate influence, rather than with potential *mechanisms* of influence, or with ways in which that influence is thought to be exercised. Even in the more recent relativist/constructivist literature in the

sociology of science, where authors have devoted a good deal of attention to the mechanisms as well as the extent of the processes through which scientific "facts" are socially constructed, rather than being divinely revealed, much of the emphasis has been on mechanisms of communication and interpersonal influence that function within a given laboratory, rather than on influences that come into the laboratory from the halls of commerce (e.g., see Knorr-Cetina, 1981, 1999; Latour and Woolgar, 1986; Mulkay, 1991; Pickering, 1995; Restivo, 1994; see also Freudenburg *et al.*, 1996, 1998).

Still, among the studies that do consider actual mechanisms of influence, it is possible to find discussions that range from relatively broad overviews to relatively specific assessments, some of which deal with a given industry or even a given company. At the broader end of the continuum are several authors who have discussed corporate influence/control at an aggregate or overall level. In one of the broader assessments, for example, Schnaiberg (1980) emphasizes distinctions between what he calls "production science" as opposed to "impact science." He argues that both corporations and government agencies tend to support the kinds of research that can contribute to increased efficiency or reduced costs of production—"production research"—while tending not to support, or possibly even actively discouraging, any lines of "impact research" that might identify potentially problematic implications of the products and technologies being developed (see also Dietz *et al.*, 1989; Freudenburg, 1996; Gieryn, 1983; Krimsky, 2000; Molotch, 1970).

In several other relatively broad works, most of the attention devoted to actual mechanisms of influence focuses on relatively straightforward considerations, such as the problems of potentially "tainted" sources of funding for scientific laboratories or the ways in which an interest in product commercialization might influence "applied" research, either in terms of overstating benefits or steering away from consideration of potential drawbacks and risks (see for example Kleinman, 1995; Kloppenburg, 1988; Levins and Lewontin, 1985). One of the most commonly noted and readily understood problems, for example, involves cases where corporate interests have sought to keep unfavorable evidence from coming to public attention or to undermine the legitimacy of more critical work (e.g., Dietz *et al.*, 1989; Krimsky, 2000; Rosner and Markowitz, 1985). Other analyses have pointed to problems that are less overt. In an analysis of logging on public lands, notably, Hirt (1994) identified a long-standing pattern that he called a "conspiracy of optimism" about the rate at which new trees would grow up to replace the ones removed by logging. (My colleagues and I—see Freudenburg and Youn, 1999; Freudenburg, 2001; Freudenburg and Gramling, 2002—refer instead to what we call "the asymmetry of scientific challenge," given that the key problem is a structural one: Even if

scientists seek to be as even-handed as possible, it is reasonable to expect an organized industry to encourage and support top-quality scientific work that would challenge any hypotheses or findings that threaten its interests, while seeing little need to question any hypotheses or findings that seem to support its interests.) As Hirt noted, such patterns may be especially likely in cases where the relevant government agencies perceive that their interests as being shared coincide with those of an organized industry, as in the case of the U.S. Forest Service and the logging industry, or of the Atomic Energy Commission and the nuclear power industry (see also Clarke, 1985; Martin, 1999).

Among analyses that have focused on more specific spheres of influence, one of the most-developed bodies of work has concentrated on the content and administration of agricultural research, particularly within the agriculture-friendly institutions of Land-Grant universities in the United States (e.g., see Busch and Lacy, 1983; Kloppenburg and Buttel, 1987). Much if not most of this work has dealt with relatively specific agricultural commodities, noting the influence of large-scale (as opposed to "family farm" scale) growers of crops such as lettuce (Friedland *et al.*, 1981) or tomatoes (Friedland *et al.*, 1975; Hightower, 1972). Another subset of the work notes the influence of industries that sell "inputs" to farmers, whether in the form of fertilizers and pesticides or innovations such as "hybrid" corn (which requires farmers to purchase new seed for each year's crop, rather than saving some of the seeds from the previous year's harvests—e.g., see Kloppenburg, 1988). Still other work emphasizes the ways in which the biotechnology industry has appropriated genetic resources while providing little in return to the less-developed peoples and regions of the world from which those resources are taken (e.g., see Kloppenburg, 1988; Shiva, 2000). The tradition of focusing on the influence of specific agricultural industries has continued even in recent studies that include Sawyer's analysis of efforts to develop alternatives to petrochemical pesticides (1996) and the ethnographic work of Kleinman (2003), who finds that questions of commercialization play important roles even in the laboratory of a scientist who has a strong personal commitment to developing technologies that will benefit the poorest farmers of the world.

Still, only a small fraction of the work on corporate influences within agriculture has to do with the influence of specific corporations (see especially Kleinman and Kloppenburg, 1991). On the other hand, such specificity is considerably more common in the literature on biomedical research, where authors express a good deal of concern about conflicts of interests among scientists whose work is likely to benefit not just specific industries but specific companies. As noted by Brownlee (2004), this concern has grown considerably in recent years, partly because 60% of the clinical trials

are now funded by biomedical companies, rather than by the government, due to a spectacular climb in private funding, which grew from $26 million in 1984 to $2.3 billion in the year 2000.

In a pattern that is uncomfortably similar to Hirt's "conspiracy of optimism," one of the most notable implications of this trend concerns what Brownlee calls "happy talk about medical products." In one case, for example, the manufacturer of the prescription painkiller Celebrex supported a study comparing that drug against over-the-counter medicines, Ibuprofen and Aspirin; the results of a massive 6-month study that favored Celebrex were published in the prestigious *Journal of the American Medical Association* (Silverstein *et al.*, 2000). The final, year-long study, however, which found that the sponsor's painkiller was associated with more gastrointestinal side effects and three times as high a level of serious heart problems than the over-the-counter medicine, Ibuprofen, was never published at all. As evidence that this pattern may not be an isolated one, the work of Krimsky (2003)—which appears to be the most extensive assessment of biomedical research available, involving a survey of more than 60,000 articles from more than 175 journals—found that only 0.5% of the authors acknowledged any potential conflicts of interest in their articles. This low rate is particularly noteworthy in light of the fact that roughly a quarter of biomedical researchers were receiving funding from industry at the time. Another broad assessment—a meta-analysis in the *Journal of the American Medical Association* by Bekelman *et al.* (2003) pulling together the results of eight other articles that collectively assessed a total of 1140 studies—found additional support for the "happy talk" hypothesis, in the form of a clear and statistically significant association between industry sponsorship and proindustry conclusions. Bekelman *et al.* also found that roughly a quarter of investigators had industry affiliations, that roughly two thirds of those investigators' academic institutions held equity in start-up companies that sponsored research being done at the same institutions, and that industry sponsorship was associated with restrictions on publications and data sharing.

As noted at the outset, however, the mechanisms I began to learn about when I picked up the telephone that day involved legal precedents having to do with industry actions in general, not with the profits of a given drug manufacturer, and so before going back to the story of that phone call, it is also worth considering the insights available from sociolegal scholarship. Perhaps in part because so little interaction has occurred between those who study scientists and those who study policy institutions and the law, only a limited amount of research has thus far been devoted to the specific ways in which corporate influence over science might affect political and legal outcomes. Still, we can gain useful insights into the potential value of this second area of work by considering Galanter's classic

assessment (1974), noting the many forms of legal advantages that "repeat players" enjoy over "one-shotters." Key examples include not just familiarity with the "best" and most articulate experts, but also familiarity with procedures and legal precedents—to say nothing of a greater capacity to *shape* those procedures, through techniques such as arranging for out-of-court settlements to avoid the establishment of unfavorable precedents. As Galanter has suggested, such "repeat players" are in many cases "the haves"—as opposed to "the have-nots"—and they have substantial overlap with what Wilson (1980) would later call "concentrated" as opposed to "diffuse" interests (see also Olson, 1965). In more recent years, research in the Galanter tradition has led to more detailed studies, with notable recent examples including ethics complaints against judges who held positions on the board of a foundation supported by industries having cases scheduled to be heard by those very judges—positions in which the judges have been treated to industry-sponsored trips to seminars in attractive locations, where they have attended lectures by and otherwise interacted with "experts" who include some of the same lawyers slated to argue cases in front of them (see Kendall and Rylander, 2004). Clearly, aside from obvious questions about the appearance (and/or presence) of impropriety, such forms of influential access to judges are not generally available to most "one-shotters" in the legal system.

Among the studies identifying more specific legal privileges for corporate "repeat players," two patterns are especially relevant here. First, particularly in cases involving potentially risky technologies, some authors have noted that industry representatives tend to favor an emphasis on scientific/technical arguments, while citizen groups tend to lack access to (or budgets to support) scientific researchers, preferring instead to emphasize broader or values-oriented arguments (e.g., see Dietz *et al.*, 1989; Wynne, 1982). Among the relatively few exceptions to this trend, perhaps the most notable has been the pattern that Brown (1987, 1997) has termed "popular epidemiology," in which local citizens work with cooperating scientists to develop their own findings and analyses. Second, some authors have noted that, contrary to popular impressions, governmental regulation of risks almost always must be carried out under conditions of ambiguous or incomplete scientific knowledge. One implication is that "repeat players" can often enjoy a relatively high level of freedom from regulations if they can successfully insist on delaying or avoiding any regulations except those that can be shown to be unambiguously "justified" (e.g., see Freudenburg and Pastor, 1992; Freudenburg *et al.*, 2003; McEvoy, 1986; Rosner and Markowitz, 1985; see also Wynne, 1982). The most extensive version of this approach appears to have come in connection with tobacco industry efforts, over a period of decades, to convince regulators and the

public that there was not enough scientific evidence to regulate cigarettes and other tobacco products (see especially Glantz *et al.*, 1996; Hilts, 1994, 1996; Rampton and Stauber, 2001; Warner, 1986).

Two additional mechanisms have been identified by authors who have considered broader kinds of law–science–society relationships. First, Martin (1999) has noted that some fields of scientific work may be shaped by what he has termed "the suppression of dissent," through measures such as "censorship, denial of access to research facilities, withdrawal of funds, complaints to superiors, reprimands, punitive transfer, demotion, dismissal, and blacklisting" (Martin, 1999:107). As Martin himself has pointed out, it can be difficult to "prove" whether a given measure represents suppression or an appropriate sanction, but as he has noted, "useful tools" in that effort can include what he calls "the double standard test," as well as the consideration of whether a given measure is typical or unusual (involving communications that go to a scientist's supervisor, for example, rather than to the scientist in question, especially if those communications suggest firing the scientist or cutting off funds rather than doing additional analyses, etc.), along with the question of whether one finds "a pattern of attacks . . . where there are theoretical reasons to expect suppression—namely, the existence of a powerful interest group that has established a routine connection with science, and a challenge to this group from a subordinate or peripheral group" (Martin, 1999:110–111).

Second, particularly in the context of debates over global warming, researchers have noted that a small number of industry-funded scientists have been able to influence the course of national debates, especially in the United States, by arguing that scientific findings are less solid than the vast majority of climate scientists believe (e.g., see Fisher, 2004). Some of these studies examine relatively direct industry funding and promotion for the views of researchers (e.g., Gelbspan, 1997; Trumbo, 1996), while others have assessed the influence of highly conservative "think tanks" (see especially McCright and Dunlap, 2000, 2003; for more recent assessments of the broader influence of conservative foundations, see Fiore, 1997; Krehely *et al.*, 2004; for formal assessments of scientific consensus on "global warming" issues, see Intergovernmental Panel on Climate Change, 1995, 2001; National Academy of Sciences(National Research Council, 1983, 2001). In the case of research on environmental justice/environmental racism, similarly, some have noted a contrast between the work funded by the Waste Management Institute, which in general has concluded that arguments about environmental racism are ill-informed (e.g., Anderton *et al.*, 1994), versus the independent work that is at least as sophisticated methodologically but has found arguments about environmental racism to be well supported (e.g., Mohai, 1995). The role of conservative think tanks in global

warming research also bears more than a modest resemblance to the role played by the industry-funded Council for Tobacco Research (CTR) during roughly the last half of the twentieth century, except that CTR research may also have helped to shield specific tobacco companies from at least some of the liability that might have been associated with findings of health damage if they had performed or overtly controlled the research themselves (Glantz *et al.*, 1996; Harris, 1994; Warner, 1986; for other studies that provide important insights into the shaping of policy-relevant science, see especially Block, 1987; Buttel, 1985; McEvoy, 1988; Sabatier, 1975; Shover *et al.*, 1983; Stryker, 1991; West, 1982).

The issue of legal liability brings up the relevance of one last body of work that is worth considering before considering methods and findings, namely, the work on the so-called "liability explosion" and what proponents call "tort reform." Although these terms had become familiar (and even taken for granted) by the 1990s, what appears to be less well known is that, by the same time, a small group of sociolegal scholars had been considering the available evidence, often coming to the conclusion that any "explosion" of legal liability awards was largely a myth, and that the true liability crisis included the ordinary citizens who were badly harmed by corporate misdeeds but left with no recourse for redressing wrongs. Researchers at the American Bar Foundation, for example, examined more than 25,000 jury verdicts and found that punitive damages were awarded in only about 5% of the cases, in amounts that were "generally modest" (Daniels and Martin, 1990:28; see also Barton, 1975; Galanter, 1983; Landes and Posner, 1987; U.S. General Accounting Office, 1989). I had a basic awareness of this literature at the time of the first telephone conversation, and I continued to gain insights from this literature and from my participation in a national conference on the topic sponsored by the Law School of my home campus at the time, the University of Wisconsin–Madison (e.g., see Daniels and Martin, 1996; Rustad, 1996), but even this literature gave me only broad preparation for the study opportunity that arose that day when I picked up the telephone.

THOSE TELEPHONE CALLS

By about this point in a "standard" journal article, it is common to provide a summary of hypotheses considered and methods employed. Given that I had no idea I would be starting a research project when I picked up the telephone, the only relevant "hypotheses" would have come from my basic familiarity with the research literature just summarized—that corporations often sought to support and publicize those lines of research that make their products look better, for example, and that "repeat players"

tend to have greater influence over the setting of legal precedents than do "one-shotters," as well as evidently having had more influence over popular beliefs about a "liability crisis." To the best of my ability to reconstruct the telephone calls and my reactions to them, I effectively had no real expectations regarding the phenomenon I was about to witness firsthand, involving the "seeding" of the scientific literature in ways that were intended to shape the course of federal court decision. In terms of methods, however, it would be relevant to say a bit more here.

Methods

Fortuitously, I did come to that first telephone call with relatively extensive experience in doing qualitative fieldwork (e.g., see the results reported in Freudenburg, 1984, 1985, 1986), and I drew heavily on my customary fieldwork methods at the time of that call and thereafter. Perhaps the most important lesson I had learned and relearned from my prior experience was that specific information often needs to be recorded quickly and in people's own words whenever possible. During the first telephone call, accordingly, I made a number of written notes, and immediately thereafter, I pulled out my tape recorder to make much more extensive notes both of what had happened and of how I was responding. I followed my usual habit of making notes both during and immediately after all subsequent conversations, particularly in cases where I felt I was learning something important about the process.

Over the next several days and weeks, I traveled to the corporate headquarters office to meet with the gentleman who had called me; asked for basic guidance from a lawyer who worked for my university and then signed a very simple contract; held a series of additional conversations with the gentleman who had called me—and more—all before eventually writing up a draft or two of material that was intended to be useful both for the corporation in question and for my own professional advancement. Based on the informal advice I received from one of my university's lawyers, I initially clarified with the company in question that, although I would keep any materials confidential for a period of 1 year beyond the end of my contract with them, I would be free to use my notes and other materials after that point. They said they had no objections to that understanding. My intention, during the brief time we were negotiating the details of my involvement, was to share my experience and "lessons learned" with my students, in much the same way as I used other field experiences that seemed useful in adding a "real-world" feel to the more abstract notions and principles that my teaching otherwise tends to emphasize.

Not only did that 1 year of delay go by, however, but so did another and another. With each passing year, my good intentions of dusting off my careful fieldnotes became more deeply buried under the other "good intentions" items on my list of things to do. Save for an informal presentation at my home campus for most of the past two decades, the University of Wisconsin—the student reactions to which helped spur me to write up the experience in the present paper—I never got around to using the information in my classes. I might well have let the matter stay forever embedded in my file cabinets had it not been for another case.

That second case was only loosely related to the first one; in it, I was working for the U.S. Equal Employment Opportunity Commission, but the issue happened to involve the same company. As part of the "discovery" process, one of the company's lawyers not only subpoenaed me, but included in his subpoena the requirement that, in advance of my deposition, I provide copies of any notes I had ever taken that had anything to do with the people or offices in that company. His subpoena, in short, forced me to dig through many boxes and file drawers of long-dormant materials to find the original fieldnotes. Only after I had dusted off those notes—and found them figuratively coming back to life—did I finally return to my original intention of sharing the notes with my students, and now with the broader scientific community.

A Final Word on Identification and Notation

As I have been preparing this article, I have often been tempted to take out references to my own reactions. As can be seen from the pages that follow, however, I have tried to resist that temptation, particularly where the reactions appear to provide potentially useful insights into the dynamics of the process. Just as an accurate record of a firsthand conversation is a better and more accurate form of data about our "subjects" or "key informants" than a subsequent summary, the relevant sections of my fieldnotes from the time generally provide a more accurate representation of my actual thoughts and reactions than anything I might reconstruct at this later date. Therefore, rather than suppressing and/or "summarizing" my reactions, I have cleaned up my notes only by correcting grammatical or typographical errors and making minor clarifications of points that might otherwise be unclear or confusing. My goal, in other words, is to allow the relevant details to emerge from the fieldnotes themselves. The one aspect of my own reactions that may not be fully evident from the following notes, which I accordingly note here in the interest of full disclosure, is that during the entire process, I never felt as if I were being subjected to an evil form of pressure; instead,

to an extent that I found surprising, I greatly enjoyed the process and the interpersonal interactions, and I found the entire experience to be educational.

Although I do reveal my own reactions, however, I do not intend to reveal the name of the company or of the people with whom I dealt. Neither in my contract nor in private conversations did the company insist on remaining unnamed; instead, my decision reflects three other reasons. The first but probably least salient reason is the long tradition in the social sciences of using pseudonyms and otherwise seeking to protect the confidentiality of research subjects. The second is that my focus is on mechanisms through which a company might shape the course of science rather than on a specific company, and the naming of names would divert attention from a focus on the mechanisms themselves. The third reason involves consistency with my personal policies. As the quoted sections from my fieldnotes make clear, it did occur to me at the time that publicly naming the company might cause some embarrassment for them; under the circumstances, my good-faith efforts to protect confidentiality in this case, to the extent that doing so is feasible, are simply consistent with what I have always tried to do in my other qualitative fieldwork, namely, to do my best to limit any undue embarrassment for the people with whom I have been working.

For these reasons, I have removed all specific references that might serve to identify the person and/or corporation in question and replaced them with a paraphrase enclosed [in square brackets]. All additional and/or clarifying information that has been added to the fieldnotes for this article is also enclosed [in square brackets]. The parenthetical comments or asides that were recorded at the time of the original fieldnotes are reported in parentheses (such as these). In other respects, the block quotations that follow come directly from the fieldnotes that I always recorded as soon as possible after the interactions themselves.

Experiences and Findings

Having considered the multiple bodies of social science research on corporate influences over scientific work and the ways in which an unexpected telephone conversation became the start of a new research project, I turn now to what would be considered the "findings" section of a more traditional journal article. The message is that the experiences taught me about a mechanism for corporate influence that had never before occurred to me—or for that matter, to even the most radical and conspiratorially oriented of my students. I begin the reporting of those experiences

with the notes I recorded immediately after that very first telephone call:

> I just got off the telephone with [a representative of a large corporation]. He was calling me in conjunction with [his company's] appeal of the punitive damage awards in the [*X* vs. *Y*] case. He started by mentioning [one of my articles that might have been of some interest to his company]. I'm not sure I know which article he had in mind, but given that he referred also to my having published in "*Society and Law*, or whatever it's called," later in the conversation, I suspect that some of the finer details of my *vita* may have been of relatively little concern to him.
>
> He started the conversation by asking if I had talked to anyone at [his company] in the process of preparing that article. That immediately raised echoes, in my mind, of the line of questioning from the lawyer in [another case in which I had been involved]. My answer was honest, but probably not terribly helpful to him: I probably did, given that I actually talked to probably a dozen folks at [his company], over the previous 2–3 years, although most of those conversations were in the context of trying to get [the company] to support even a modest level of research on the socioeconomic impacts of [the incident in question]—particularly in light of the tens or even hundreds of millions of dollars that the same company was spending on studies of biological impacts. Unfortunately, I never really did make any significant progress in that effort.

After two or three follow-up probes, he said that his initial line of questioning didn't really matter, and that it wasn't really the focus of his call. Instead, he said, he wanted to see if I might be interested in writing an article that [his company] would be able to use as part of its appeal of the punitive damages in the case. As he put it (the following is as close as I can get to a verbatim recording of his remarks from just a few minutes ago),

> Naturally, we have a range of expert witnesses and so forth, but we find that it's also helpful to have people working on articles that come out in academic publications. We've often worked with economists, for example. A lot of them feel that punitive damage awards are very inefficient, compared to other approaches such as regulation, and naturally, that's a perspective we're quite comfortable in supporting. But we're exploring whether we might want to work with professors in publishing things from a few other perspectives, too.

[Later in the same conversation:]

> Basically, what we're exploring is whether it's feasible to get something published in a respectable academic journal, talking about what punitive damage awards do to society, or how they're not really a very good approach. Then, in our appeal, we can cite the article, and note that professor so-and-so has said in this academic journal, preferably a quite prestigious one, that punitive awards don't make much sense.

[At this point, he and I spent some time exploring various possibilities that might be of interest both to me and to his company. Later, as part of that conversation, he continued:]

> . . . or maybe it could be something along the lines of how difficult it is to prevent these kinds of things under any circumstance. It's a little like the *Challenger*—by

> the way, [a distinguished analyst of that accident] gave us your name. But one of the things she found is that the people involved weren't really all that venal. Sure, there were a lot of mistakes, and she said that when she started, she expected to be pretty outraged, but by the time she was done, what she'd decided was that it wasn't quite so simple, after all. Sure, it was a terrible thing, and it got a lot of people very upset. But maybe it would be better if we handled such things through the regulatory process, which is actually quite a bit more democratic. The legal system really isn't very democratic—its not supposed to be, I guess. But at least with a regulatory process, if you think a regulation is unfair, you still have at least some chance to take your case to the electoral process, to try to get things changed.

By the end of this conversation, I was intrigued in several respects. One of them involved my qualms about doing this form of consulting work: I found that my qualms were calmed enough—both through ongoing interactions with this caller and through learning what it was that I was or was not asked to do—that they did not present a roadblock. In essence, I proposed only those topics that I would feel comfortable in turning into journal submissions, leaving to him and his company the question of what to support and pursue. Another aspect of the intrigue, as suggested above, involved the instincts of someone who had done a good deal of qualitative fieldwork and who thus quickly recorded the notes that are summarized here. In that same spirit, however, the best record of my ongoing reactions and of the ways in which I wrestled with them at the time comes from the similar fieldnotes I recorded immediately after a second conversation, a few days later:

> This morning I had my second long telephone conversation with [the company's representative]. He apologized for having missed my call back to him and commented that, ironically, he'd actually been up in my neck of the woods yesterday—visiting economists at the University of Chicago. One of my main concerns was to get a better idea of just what, exactly, they had in mind. Apparently, he is part of the NRDA team, which stands for Natural Resource Damage Assessment—a topic where my involvement actually goes back all the way to the time when I was on [a federal agency's] Scientific Advisory Committee. While [his company's] group still has the same name as back in the old days, their main focus today is in a different direction—basically, oriented toward reducing the multibillion-dollar punitive damages award through the appeal process.
>
> In some ways, [this gentleman] reminds me of [someone else I knew from the same company], whom I got to know reasonably well during my own days on the Scientific Committee. At least in terms of initial impressions, he doesn't come off at all like an ogre, and at least when he's talking to me, he's been careful to stress [his company's] interest in a rational approach.
>
> The topic came up, in particular, as I was exploring something I still don't fully comprehend, save perhaps at a strictly intellectual level: how it is that a company as big as [his] would actually want to pay a sociologist for doing something that we normally think of as providing a useful example of the word "obscure"—publishing in an academic journal—and what good it could possibly do them. We also discussed, just a bit, how he had managed to identify me; apparently, in addition to [the distinguished colleague's] recommendation, they had seen a couple of my articles, and

also, it just so happens that his son is a student at UW–Madison. But, back to the question of what good it does them.

At the level of the initial or jury trial, he had noted, academic articles really wouldn't do that much good. At least in terms of the analysis he shared with me over the telephone, he saw this as being due to the fact that, as shown by "research by [another of his company's consultants] and some other people who know a lot more about jurors than I ever will," the jurors tend to be swayed by a good number of nonfactual considerations. These included not just the fact that it's easier to sympathize with "little guy" victims than with a big bad company such as his, but also (as he not only told me but reassured me when I raised a mild question about it), "by a kind of lottery mentality . . . they (the jurors) think that 'next time, that could be me'"—the lucky person who might enjoy a windfall of a similarly huge jury verdict.

While he didn't see much chance that the initial set of jurors would be all that impressed with "an erudite article," as he called it a couple of times, "once it gets to the judges, you start to have a better shot; . . . with the judges, there's at least a reasonably good chance that they'll be able to see things as they ought to be."

I asked how that could be the case, particularly based on something so obscure as a journal article. That, he responded, was one of the reasons why liming is important, although apparently there is at least some flexibility in this case.

I indicated to him during the long conversation that I'd be out of the country and/or at sociological meetings from roughly the middle of July through the middle of August (leaving not much time before the September target date he initially mentioned to me.) "There's actually a bit of flexibility in that," he assured me, in part simply because of the mechanical details of the appeals process. [His company needs to] submit something in writing in the court case this fall, "But there, one of the things we can do is to mention that Professor so-and-so has written this important article, which has been submitted to what of course we hope would be a highly respected academic journal. We can also provide a copy of the article. The judges themselves don't usually read them, but often their clerks will read them, . . . and quite a few of the clerks, nowadays, are pretty open to these kinds of arguments; . . . quite a few of them now come out of a law and economics program or something like that."

Then, apparently, many months pass. (I must admit I can't recall all of the details of the [timing], but if this actually moves forward, I'm sure there will be plenty of opportunities to get the details straight). His basic point was that when [his company's] lawyers respond to the judges' questions at a later time, that provides them an opportunity to add, "Oh, and by the way, the learned article by Professor so-and-so (the one that makes a point that bolsters the company's case) has now been accepted for publication in this prestigious journal."

Even if the process stretches on beyond that, moreover, it may or may not be that worrisome to them; as he assured me several times, they see a very high likelihood that this case will go directly to the Supreme Court. In this case, moreover, there's the additional advantage that "the Supreme Court justices won't be encumbered" with the problem of "trampling on the states." He referred at some length to the [then-] recent Supreme Court decision overturning the multimillion-dollar punitive damage award from Alabama for the man who had a repaint job on his BMW that the company hadn't told him about. At least if I understood [him] correctly, one of the main reasons why it was such a close decision (5-4) was that a number of the Supreme Court justices saw this as an inappropriate intrusion into what ought properly to have been state-level decisions. For cases from the federal courts, on the other hand, there won't be any such problem. He mentioned in particular Justice Breyer, whose views on regulations and the marketplace I've actually read (and frankly find to be somewhat ill-informed), but he also appeared to be referring to other considerations when he said of the Supremes that "We know they're thinking hard about general guidelines for punitive damage cases, and that apparently the

folks at [his division] have good reason to feel their case, in particular, will give the justices the very opportunity they've actually been looking for, to spell out those guidelines."

At that point in my fieldnotes—as if to make the point that my fieldworker instincts had already been engaged, including my usual instincts toward the protection of confidentiality—I added the following observation:

During much of the conversation, I couldn't help but think of how fascinating these details would be to many of my students, many of whom are perfectly comfortable in making vacuous assertions about overt and blatant ways in which "capital" supposedly tells "the state" the ways in which policies will be changed, but few (if any) of whom have much real idea how a more subtle form of influence might be exercised. I wouldn't guess that this would be the sort of thing that [his company] would care to see widely advertised, particularly before the case goes to court ("Massive Corporation Attempts to Buy Scientists, Brainwash Judges"), but [he] was amazingly willing to discuss the strategic questions openly, so I simply kept asking.

I suspect that by the time this is all over (although that may of course happen at almost any time), I will have learned quite a few more of the details, but at least in terms of the specifics he mentioned today, there are also some other options.

One is to try to get things placed into major, policy-related newspapers—a category, he said, that basically comes down to the *New York Times*, the *Washington Post*, and the *Wall Street Journal*. He specifically (and relatively enthusiastically) mentioned a *Wall Street Journal* Op-ed piece by Alex Kozinski, a judge on the 9th Circuit Court of Appeals, which he promised he'd photocopy and send to me, on how juries are somehow fundamentally antidemocratic as compared with regulations. "With regulations, of course, things can get pretty unreasonable, too—but at least with regulations, you have the possibility of recourse to the democratic process. You can try to take your case to the people, and get somebody else elected, and try to get a more reasonable set of regulations put into place," while with a jury award, there really isn't such an option. (My assumption, although I didn't check this with him explicitly, is that [his company] will have more than enough contacts, "stringer" journalists who are looking for news stories, and so forth, to be able to get some reasonably sympathetic articles published—if only in response to complaints about "liberal media bias"—but the company clearly wouldn't have much difficulty in buying full-page ads in the very newspapers he mentioned if they wanted to express their own views more forthrightly.)

He also mentioned that it's possible to offer small amounts of support to academics who already show some tendency to express views that [his company] finds congenial; he mentioned in particular a forthcoming piece by [another author], which he promised to send me. "Beyond that, you can sponsor workshops and so forth, but that gets kind of tricky. For one thing, once you get to that point, you pretty much have to invite both sides." Also at that point, it appears, something that would be sponsored by [his company], in particular, might come to be seen as "tainted." I told him I could understand that reaction, at which point he continued, "but there are ways of getting around that . . .. There are a lot of other organizations out there that are basically in the same kind of position as we are, thinking (something like—I think I have one or two words of this quote wrong) 'there, but for the grace of god, go I.'" Partly because of the existence of such groups (evidently including an array of well-supported, business-friendly "think tanks"), "often it's possible to set something up that's sponsored jointly" by a whole group of companies and/or organizations.

He ended those illustrations by saying, in a summarizing sort of way, "There are other ways of dealing with it, too," but by this time, I was starting to feel that I

> might be pushing things just a bit too far, so I didn't ask any more follow-up questions along those lines. Instead, he agreed to send me several of the articles we had talked about during our conversation, and I agreed to send him my *vita*, right away, following up with some ideas about the kinds of article(s) I might be willing to do for them within a few days.

The next several paragraphs of fieldnotes have far more to say about my own reactions than about the ways in which a corporation can influence the course of science, but I have included them, if only in the interest of full disclosure:

> I must admit, I have quite a complex set of reactions at the moment. Part of me is deeply bothered by the fact that this sort of thing is going on—at all—let alone by the fact that I might become part of it. Another part of me—the middle-of-the-road part—is tapping me on the shoulder, reminding me that I've always said I try to be a straight shooter, I call them as I see them, and whether it's [his company, or his company's] sworn enemies, if they can use my stuff, fine, and if not, that's their choice.
>
> Yet perhaps the biggest part of me is the ethnographic researcher—roughly the same part of me, I suppose, as is dictating these fieldnotes at the moment. I really do want to learn more about how this works. I see a clear potential for ethical quagmires and quicksand, of the bottomless-pit variety, but I guess at least for the moment, so long as I continue to be worried about those questions, there's at least some reasonable hope that I'll continue to learn more, while not completely selling my soul to Satan.
>
> Much to my amazement, moreover, he hasn't even mentioned anything about keeping any of our conversations confidential. Maybe that's because he's readjust enough of my stuff, or has just enough of a sense of who I am on the basis of reading that stuff, to know that I'd probably be just as embarrassed, at this point, if our conversations became public, as he would. After all, at one point during our conversations, I remember thinking to myself that, whether this goes forward or not, whenever I am finally able to use this in a class, it's going to take quite a bit of discussion, because the students will want to do a fair amount of processing of just what the ethical implications are. (Hell, so do I—right now—why should they be any different? Besides, that's virtually guaranteed to be a splendid learning opportunity.)
>
> The problem, really, is with my friends, not my students. My right-wing friends, I was thinking to myself, will mainly be jealous that they didn't get the job, or the bucks, when they'd actually be quite happy to write the kinds of things that would make hearts at [his company] beat proud, so I won't really win any points with them. On the other hand, my left-wing friends will likely conclude that this proves, beyond any shadow of a doubt, that I'm the kind of low-life scum who represents a blight on the face of the earth. Now that the conversation's over, and I've actually spent a fair amount of time recording these comments, I don't have any clearer insights than I did [while he and I were talking]; I guess I simply need to remain true to my ethnographic principles, but also my researcher principles—keeping careful records, which may well provide me with valuable insights into the nature of power in society, while nevertheless performing a remarkable balancing act at the same time—giving [his company] a quality product for the money, if it ever gets that far, while not sending anything off to a peer-reviewed journal that I'm not comfortable signing my name to.

Soon after that point, his company paid to fly me down to their headquarters so that he and one of his colleagues could meet with me face to face. At that meeting, I remember saying to them that, although I had

written some articles that would warm their hearts and others that would be more likely to bring them heartburn, the ones they had in front of them tended to be of the "heartburn" variety. Under the circumstances, I wondered, why had they invited me down anyway? Their answer, offered without hesitation: "How do you suppose we could find somebody credible who *hasn't* said some critical things about us?" That response, and the good-natured way in which it was presented, did a good deal to put me at ease, as did the congenial tone of our conversations more broadly. By the end of that visit, we agreed that we would examine some five different possibilities we discussed that day, with my working to develop one or more of those ideas for an article.

Next Steps

After that visit, much of my ongoing involvement with the company became a matter of writing up the agreed-upon work, only the relevant details of which will be summarized here. I did in fact develop several outlines for potential article submissions and discussed them with the company. The option they found most interesting had to do with some thoughts I had already "been thinking about writing up some day," arguing that the adversarial approaches of U.S. jurisprudence ran precisely counter to the prescriptions for sensible management that were beginning to emerge from the literature on risk and risk management at the time. In essence, while adversarial procedures encourage secrecy, the findings from the literature on organizations and risk were beginning to suggest the importance of "organizational permeability" and visibility, as well as other forms of openness, for improving organizational performance in general and risk management in particular (e.g., see Clarke, 1993; Shrader-Frechette, 1993; see also the later work of LaPorte, 1996 or LaPorte and Keller, 1996). Next, I did a quick write-up of a draft paper that spelled out the basic argument and included a sampling of the relevant citations, and sent it to my contact at the company. After that, however, the reactions from the company slowed considerably.

Neither my contact nor anyone else at his company expressed any strongly negative reactions to me, although at least one of the lawyers in his division apparently argued that even an academic article advocating "openness" could be more than a little bothersome from the company's perspective, given that "openness" could lead to an increase in the number of people who would know enough about the company to sue it. The larger problem, as my contact explained to me—in an explanation that I believe to have been a genuine one—simply came down to the value of my argument to his company. He thought the article would be a "nice" one, he said, but it

wouldn't be sufficiently helpful to his company's case to be worth spending the additional dollars that would be required, at my consulting rate, to turn it from a draft discussion paper into a published article.

At the time, I remember thinking that his reaction was similar to a rating of "good" on a grant proposal for an agency such as the National Science Foundation—even if a reviewer might check the box that says, "fund this proposal if resources are plentiful," no one I knew had ever encountered a case where resources were that plentiful. As evidence of the fact that my own evaluation was not that different from the reactions in his company—with the strongest evidence being provided by actions, rather than words—I need to acknowledge that, even after all these years, I still have not put enough of my own time into doing the revisions that would be needed to rewrite that draft paper and send it out for review. It remains buried in a file cabinet, and unless some new opportunity arises, that paper may be less likely to go in to a journal that to go into my university's recycling bins.

REFLECTIONS AND IMPLICATIONS

Many years after the telephone conversations at the start of this article, I received another telephone call, and it started a new learning process. First, that later call began to teach me, again, something I already knew, namely, that the academic temptation to dismiss "mere journalists" may be too hasty, significantly underestimating the amounts of care, rigor, and hard work that can go into investigative news stories in particular. Second, I learned something new about the interplay across the boundaries of academic research, the courts, and the world of mass media. Third, and most centrally for the purposes of this paper, I began the process of learning a bit more about the paper's actual topic from the newspaper reporter who made the call.

Again in the case of this call, my familiarity with the existing literature did little to prepare me for what I began to learn, and some of the details of my own experiences and reactions are sufficiently relevant to deserve more discussion than I would normally include in an article in a mainstream journal. The first of the relevant details is that, after I finally wrote up the experience summarized thus far—in a paper that had been accepted for presentationas at the annual meeting of the American Sociological Association—I also sent copies to a few colleagues whose judgment I valued, whose sensitivity to the potential delicacies of the issue I trusted, and whose experience gave them what I thought would be particularly useful perspectives on the issues with which I was attempting to grapple. Within a few weeks, I did hear back from one of these colleagues—a higher "return

rate" than I normally expect from a group as busy as this—and I took his comments into account before submitting an earlier version of this paper to *Sociological Forum*. Only a week or two after that submission, I was surprised by the new telephone call.

This call came from a reporter for the *Los Angeles Times* who was working on a story and had just learned about my paper from the one colleague who had actually read it and provided me with feedback—no small job of finding a needle in a haystack, by my way of thinking, given that the colleague in question lived and worked at the opposite end of the continent. The reporter said he was interested in doing an article on a phenomenon he found to be interesting—in essence, that after years of corporate "investments" in biological and physical science research, at least one company was starting to make small but significant efforts to court *social* scientists.

My initial reactions—and indeed, my predominant reactions even later on—were highly conflicted. On the one hand, the reporter's interest was clearly genuine, and his curiosity was understandable. On the other hand, I have always sought to avoid having "my results" reported in the mass media before they are published in peer-reviewed journals. Ultimately, after several telephone conversations—including some in which he made it clear that he was already well aware of many relevant details that he could not have learned from my paper and that he truly was interested in much more than just my own experiences—I agreed to meet with him. I had by this time moved from Madison to my new institution (the University of California, Santa Barbara), which was close enough to his base in Los Angeles that he drove up, met me on campus, and spent several hours with me, in a wide-ranging conversation that took place not just in my university office, but also at my home office and—with my automobile headlights providing the only illumination—also in a "U-Store" locker that still held a number of boxes from my then-recent move from Madison to Santa Barbara. Then he went back to Los Angeles, and many weeks passed.

With each passing week, I spent less time wondering what, if anything, would result from the long interview. Finally, to my great pleasure, I received word from *Sociological Forum*. Not surprisingly, the reviewers had expressed a wide range of views toward the original submission—not greatly different in substance from the mixed feelings I had long held—but the editor saw enough potential to invite me to submit a revision, if I felt I could respond in good faith to the reviewers' suggestions and concerns. The only immediate difficulty I remember was the fact that the reviewers' comments and suggestions—some merely offering constructive assistance, others involving more serious concerns—were clearly legitimate ones, of the sort that could not be handled overnight.

After receiving word from *Sociological Forum*, I decided that I should look for the reporter's phone number, call him up, and ask if he could hold the story for just a few more weeks, if indeed he still planned to finish it. Before finding that phone number, however, I needed to finish up a few more lectures for an important new course preparation—and only about a week later, before I ever managed to make that telephone call to the reporter, his long and detailed story (Zarembo, 2003) came out.

My first reaction involved a combination of grief and denial—grief because I worried that the *Los Angeles Times* would "scoop" what I had thought of as "my" story, and denial that involved repeated variations on a theme of, "I can't *believe* I didn't get around to calling him!" Several other people saw the newspaper story as well, including two colleagues in Santa Barbara who asked to see copies of the original paper that I had prepared under contract but never submitted, as well as the earlier version of this paper, as submitted to *Sociological Forum*. One colleague actually read both, and reported that he agreed with my belief that the *Sociological Forum* submission was significantly more interesting. I was also contacted by the author of a sociological textbook who was interested in adding a few words about my experiences to his book's discussion of ethical questions faced by sociologists, and who, after exchanging e-mails with me, prepared a discussion that did a careful job of summarizing the key aspects of the experience while respecting my own sensitivities.

Most important for my own purposes, however, the passing of some time also provided me with the perspective I needed to reread the newspaper story on its own terms, rather than with a sense of impending doom about what it might have had to say about me in particular. It was only at that point when I began to realize that this careful reporter had managed to uncover a number of "findings" and even to do some legal analyses that went beyond the possibilities that had previously occurred to me.

In all, the reporter had managed to identify and contact all of the six other people who were still alive and whom I knew or suspected to have had financial arrangements at least reasonably comparable to my own, and he had identified three other beneficiaries as well. Some of them, the article reported, had used the company's cash strictly to fund the costs of studies, including one in which 8000 persons were paid for participating in a "mock jury" study—roughly two to three times as many as in a "large" study funded by the National Science Foundation. One accepted payments only to cover his travel expenses. Others, like me, accepted the cash personally. Neither the reporter nor I ever did identify anyone else who shared my

experience of parting ways with the company, bringing an end to what the article called "research funding that most social scientists can only dream about" (Zarembo, 2003:1A).

Three other aspects of his story, however, struck me as being more important. The first had to do with the question of potential biases, seen and unseen. In general, other authors who agreed to speak to the reporter offered assessments like my own—namely, that, rather than "buying" people who wouldn't have agreed with the company's perspectives otherwise, the company identified authors whose views it had reasons to like, offering those authors whatever forms of support appeared most likely to help in getting publications finished and submitted. Still, even in denying that they were influenced by corporate funding—and in the case of one, in suggesting that any criticism of his new-found prosperity must have "stemmed from professional jealousy" (Zarembo, 2003:A20)—the other authors showed little evidence of grappling with the concern suggested by Brownlee (2004) and by feminist critiques of science. As critics point out, the absence of *awareness* of bias may or may not correspond to an *actual* absence of bias; from Zarembo's article, however, none of the other researchers appeared to have shown much greater insight than I had shown in the fieldnotes summarized above in promising myself that I would "remain true to my principles"—an admirable-sounding promise, to be sure, but one that could become conveniently vague in practice. Instead, as Brownlee has noted in the context of biomedical research, corporate sponsors may even understand the importance of encouraging researchers to feel "convinced of their own impartiality." Brownlee quotes an observation offered by moral philosopher Carl Elliott (e.g., see Elliott, 2003) about pharmaceutical researchers: "If they understood that they were being used as industry mouthpieces, they would probably pull the plug on the whole enterprise" (Brownlee, 2004:40).

The second important aspect of the article in the *Los Angeles Times*, in my view, was that the reporter's careful work had effectively provided me with a way of responding to a reviewer request that, while reasonable, had stymied me up to that time. One reviewer, reasonably enough, wanted to get some idea about what kind of overall impact this new strategy might have had, but until I read the Zarembo article, my only honest answer would have been, "I don't know." Among the sources that Zarembo had located, however, was an article in the *Wake Forest Law Review*—a journal that is certainly respectable, although the UCSB library does not carry it—which was previously unknown to me but is directly relevant to the experience reported here.

The major thrust of that article (Eisenberg, 2001) involves further debunking of the common belief that punitive damage awards have become

"unmanageable." As Eisenberg (2001:1131) summarizes the matter, "All studies based on the mass of cases confirm that punitive awards in products liability and medical malpractice cases are rare, and not out of control." In addition and more specifically, however, the Eisenberg article also includes a two-page discussion of specific studies funded by the same corporation that had contacted me—studies that were then used in legal briefs by this corporation and by others. The Eisenberg article is quite critical of nine articles that had by then been published, most of them in reasonably respectable journals, based on the company's funding, concluding that the corporation in question had decided to "attack and discredit" the legitimacy of punitive damages by paying for a body of scholarship that actually devoted "hardly a nod to actual cases it seeks to undermine" (Eisenberg, 2001:1148). Zarembo had located all nine articles, plus four other articles and a book, and found that all had acknowledged receiving funding from the corporation in question; he also found, however, that the corporation's appeals made "no mention that it had funded the work," instead arguing simply that "these articles present recent social science research demonstrating that jurors are generally incapable of performing the tasks the law assigns to them in punitive damage cases" (Zarembo, 2003: A20).

Finally, the fallout from that second telephone call also taught me something about the potential interplay among academic studies, journalistic reports, and the law. Almost as soon as the *Los Angeles Times* article came out, I found out why a request for delay might well have been futile even if I had managed to find the reporter's telephone number more promptly. As I had learned long ago from a friend and college roommate who briefly became a professional journalist, one of the elements in deciding on an article's "newsworthiness" has to do with timeliness and topicality. As my friend had explained, one reason why so few academic studies receive much media coverage—independent of specialized language, technical analyses, and more—is that most of our studies focus on longer term "issues," rather than reasonably current "events." The exceptions, he continued, usually had to do with cases where a reporter could find a "news peg"—an appropriately current event that would make a story about that issue more timely in week *X* than in weeks *W* or *Z*.

In the case of the events that had led to my own involvement, the corporation in question had by then spent over 15 years in appealing the original, multibillion-dollar verdict. Although it had encountered many losses and very few successes, the incredibly drawn-out appeals process had included two accomplishments that presumably would have found favor in corporate boardrooms. One was that, during the entire process,

not a penny of punitive damages had yet been paid—allowing more than one chief executive officer to retire without such a financial blemish on his record and presumably encouraging other potential litigants to "think twice" before taking on this company. The other was that on two occasions, higher courts had sent the case back to the judge who first ruled on the case, requiring him to reconsider the punitive damage award in light of guidance from the decisions that had been rendered by higher courts, including the Supreme Court.

The first such reconsideration had taken place in 2001, at which time the original judge—who had been nominated by President Ronald Reagan—had reduced the amount of damages by roughly 20%, to $4 billion. Rather than paying the new, lower amount, however, the company continued to pursue an aggressive array of appeals. As those additional appeals were argued, however, the courts were also hearing other cases. By making skillful use of legal databases, the reporter (Zarembo) managed to learn of an especially interesting "citation cluster," involving an otherwise-unrelated case in which a large insurance company was appealing a (relatively) smaller settlement. In that second case, still other companies had filed a "brief," or legal argument, that included extensive citations to the research supported by the company that had contacted me. In an interesting concatenation of circumstances, the U.S. Supreme Court overturned a $145-million judgment against that insurance company in April 2003, sending the case back to the Utah Supreme Court for further deliberation. Shortly thereafter—making reference to that insurance company case—the U.S. Court of Appeals for the second time instructed the original judge to reconsider the remaining $4-billion judgment against "my" company. As Zarembo put it, this effectively meant that the studies "had been used in a separate case that was now coming back" to help the original company (Zarembo, 2003:A20). The legal arguments in that original case were to be heard on December 3, 2003—and that was the day when Zarembo's article came out in the *Los Angeles Times*.

If the Supreme Court justices found the research to be compelling, however, the original judge apparently did not. In late January of 2004, in a long and carefully reasoned judgment that reflected many of the same arguments and conclusions put forth in the Eisenberg (2001) article, the judge decreed that the proper amount was actually $4.5 billion, not $4 billion—but also that, by now, the corporation also owed roughly half that amount in interest, bringing the total judgment to nearly $7 billion. Although the corporation announced at that time that it would appeal the judgment once more, a number of articles on the case in the legal and popular presses indicated that the litigation might by this point be entering "its final stages."

Reflections on the Future

Overall, just as the experience allowed me to learn about a specific mechanism of corporate influence that I had not previously imagined, it also allowed me to learn that the details of the experience were not nearly so dramatic or mythic as the virtual Faustian bargain that I (or many other sociologists) might well have expected. Rather than asking for a detailed, five-volume contract, full of fine print, the company's lawyers simply exchanged brief letters of agreement with me. Rather than living up to their prior reputation as some of the toughest sharks of all, the company's lawyers treated me with kindness, respect, and a good deal of friendly informality. Rather than demanding the deed to my soul, the company's representatives made a good-faith effort to identify topics of mutual interest, and when there proved to be too little overlap to justify further funding, we simply parted—doing so on quite friendly terms.

The gentle and basically cordial nature of the experience—in conjunction with a helpful reminder from a reviewer, namely, that I needed to offer an appropriate term for the phenomenon I encountered—is why this paper's title refers to "seeding" the science, and to "courting" conclusions. As with literal "cloud-seeding"—efforts to extract more rain from stingy cloud formations in water-poor regions, generally by adding silver iodide crystals to provide the kernel around which rain drops can form—the company in question worked with "clouds," or professors, that were already in roughly the "right" positions, rather than trying to recruit professors who would not otherwise have been found anywhere in the vicinity. Like cloud-seeding efforts, moreover, adding corporate silver probably did change the overall distribution of publications, if not the overall number, in that the time being spent on this company's work was no longer available for other topics—although much the same would have been true, of course, if the same professors had instead spent time in responding to initiatives from a dean, or in providing helpful comments to a colleague or to the author of a paper that had been submitted to a peer-reviewed journal. As in the more traditional version of "courting," finally, the effort required an appropriate degree of mutual or shared interest in continuing to interact with one another in the future.

Yet this is not exclusively good news. In retrospect, when I ask whether the cordiality serves as evidence that sociologists have been overly concerned about the influence of corporate cash on the conduct and content of science, I find it more difficult than I once would have imagined to answer in the affirmative. Instead, I worry today that the absence of drama and trauma may indicate the need for even closer attention—particularly

regarding what I still consider to be the closest approximation to a gold standard in the world of science, namely, peer-reviewed journal articles in the open literature.

The problem is not that I believe the other authors who worked with the company would have been any more comfortable than I in writing down lines of argument or conclusions that they found questionable; in fact, I doubt if there would have been a clear difference on that point. At the same time, however, I sense at least three generally unseen ways in which this experience may have affected the content of peer-reviewed journals.

The first two have already been suggested. The first is the most obvious, involving what lawyers call "but for" causation—the fact that, "but for" the interest of a single company in reducing court judgments in a single case, certain articles probably never would have been written or published. Other scientists who were contacted by this company finished and published the papers they were developing under contracts similar to my own, leading to at least some small change in the "open" scientific literature—in terms both of what is present and of what is absent, namely, the articles they might have worked on otherwise.

The second has to do with "later" articles or lines of research, possibly pursued well after the end of a given contract, influenced by the ways in which an apparently minor alteration of trajectory can lead to a cumulatively greater change of direction over the long term. As suggested by work on discipline formation, or for that matter by daily experience, even distinguished colleagues are often influenced by the enthusiasm (or the lack of enthusiasm) that their ideas inspire—especially if they have not yet invested the time needed to put those ideas down on paper—and scholars who become "known" for a given line of work often find themselves putting more of their energy into continuing just that line of work. Even for the most prolific scientists and scholars, in other words, the number of papers on the "to do" list tends to be significantly greater than the number of papers actually finished. Except perhaps when strong or even visceral preferences arise from within ourselves, the ideas that many of us seem most likely to pursue are ones that ignite the interest of others—whether students, colleagues, or the nice people who are providing funding—and in the longer run, many of us seem to devote a greater fraction of our efforts to the lines of work that have had the best reception in the past. In much the same way as "one thing can lead to another" in the case of a city, in other words (see Molotch *et al.*, 2000), it may be important not to overlook how the experience of doing one or two articles that bring both personal and financial rewards might influence the course of a career.

The third and perhaps most important form of influence, however, has to do with the possibility of subtle shifts in our own sense of what it means to do balanced, professional work. The pressures toward bias that concern me here are not the ones we recognize, but the ones we fail to sense. I have little doubt that most professionals take a justifiable pride in resisting overt pressures to reach biased conclusions; I have greater concerns about those more subtle forms of pressure that we might never even recognize as such.

I want to be especially clear that I offer this observation as much in confession as in accusation. My fieldnotes, as quoted above, emphasize my concerns over resisting any obvious pressures toward bias, noting that I made a pledge to myself about "not sending anything off to a peer-reviewed journal that I'm not comfortable signing my name to." I fully expect that some of the other scientists in question would have made much the same commitment to themselves, and that, if asked today, they would report the (honest) belief that they had kept those commitments.

At least in retrospect, however, what I find striking is the limited extent to which my fieldnotes reflect on any possibility that I might be tempted to shade my interpretations, not because the company's representatives were so sinister, but precisely the opposite—because they were so likeable. This is not merely a question of what I would have done if those likeable gentlemen had asked me to make a few minor wording changes here, or to add a few paragraphs of amplification there. Instead and more subtly, it involves the question of whether I might have started to make changes *in the absence of any such suggestions from them*—simply because I might want to anticipate the preferences of people I was coming to see as friends, who would by then have demonstrated repeatedly that they respected my professionalism and balance. I have no way to know, but this last issue, in particular, is the one that creates greater concern for me today than it would have before I picked up the telephone on that fateful day.

ACKNOWLEDGMENTS

The author thank Sheldon Krimsky, Eric Zimmerman, the students and faculty who participated in the seminar on Science, Technology, Agriculture, Resources, and the Environment at the University of Wisconsin–Madison, and the editor and reviewers of *Sociological Forum* for helpful comments on earlier drafts. All errors and interpretations, however, are mine alone, and should not be taken as representing the views of any other individuals or institutions.

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