*Scientific Certainty Argumentation Methods (SCAMs): Science and the Politics of Doubt**

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At least since the time of Popper, scientists have understood that science provides falsification, but not "proof." In the world of environmental and technological controversies, however, many observers continue to call precisely for "proof," often under the guise of "scientific certainty." Closer examination of real-world disputes suggests that such calls may reflect not just a fundamental misunderstanding of the nature of science, but a clever and surprisingly effective political-economic tactic—"Scientific Certainty" Argumentation Methods, or SCAMs. Given that most scientific findings are inherently probabilistic and ambiguous, if agencies can be prevented from imposing any regulations until they are unambiguously "justified," most regulations can be defeated or postponed, often for decades, allowing profitable but potentially risky activities to continue unabated. An exploratory examination of previously documented controversies suggests that SCAMs are more widespread than has been recognized in the past, and that they deserve greater attention in the future.

This article will identify a pattern of argument that is sufficiently common in regulatory debates that it appears to deserve its own name—"Scientific Certainty" Argumentation Methods, or SCAMs. In the first section of the article, we draw on science and technology studies to underscore the point that science is often characterized not by certainty, but by uncertainty—meaning that the outcomes of scientific/technological controversies may depend less on which side has the "best science" than on which side enjoys the benefit of the doubt in the face of scientific ambiguity. In the second section, we note that the benefits of doubts may be distributed in ways that are not merely random: Although there is clearly a need for more extensive research, a series of risk-related controversies, over a period of nearly a century, indicate that industrial interests have often managed to delay or prevent legislative and/or regulatory actions even in "tough" cases—those where the preponderance of scientific evidence had indicated significant reasons for concern. The third and final section discusses implications, calling for further research to document the nature and extent of such patterns across a broader range of contexts.

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"Scientific" Decisions in a Context of Uncertainty

On December 21, 2004, after studying proposals to allow U.S. citizens to import prescription drugs from Canada, a task force commissioned by the U.S. Department of Health and Human Services filed its final report. Because the task force "could not be sure" that the imported drugs would be safe, its members recommended that the practice remain illegal. The two Cabinet Secretaries who forwarded the report to Congress said that they would recommend a Presidential veto of any drug import bill "that does not address the serious safety concerns" they saw with current Canadian regulations (for news accounts, see, for example, Pugh 2004).

The next day—December 22, 2004—a different federal agency, the Forest Service, eliminated its policy for preparing Environmental Impact Statements on Forest Plans, as well as the requirement for logging to protect all "viable" species in the National Forests. A vice-president of American Forest Research Council, an organization representing timber companies and landowners, praised the new plan as "moving the Forest Service away from being mired in bureaucracy." Others were less impressed; Princeton University professor David Wilcox, former North American President for the Society of Conservation Biology, argued that the Forest Service might therefore "be allowing large-scale environmental damage" (Borenstein 2004). Soon thereafter, the California Attorney General (Lockyer 2005) sued the federal government, arguing that this and related policy changes would weaken environmental safeguards that were "needed" for California forests (see also Martin 2005).

The two federal announcements, put forth within a day of each other, illustrate the importance of a pattern that has received less attention to date than it deserves. From the perspective of civics textbooks, of course, government agencies are expected to use solid and balanced scientific analysis to limit risks to public health and the environment (for more detailed analyses, see Caldwell 1982; Lowi 1986; Porter 1995), and perhaps the major concern has been that balance might be threatened by "agency capture" (see, for example, Freudenburg and Keating 1985; Kaufman 1960; McConnell 1970). In the classic definition of Bernstein (1955:93), quoting from an earlier U.S. Senate Committee report, "capture" occurs when governmental bodies "become the servants rather than the governors" of the industries they regulate (see also Kaufman 1960; Selznick 1949; Stigler 1975). As pointed out by a number of thoughtful analysts in more recent decades (see, for example, Block 1987; Buttel 1985; Freudenburg and Gramling 1994; Sabatier 1975; Shover, Clelland, and Lynxwiler 1983; Stryker 1991; West 1982), the notion of "capture" can be problematic, as well—but from the perspective of the present article, perhaps the central problem is that "capture" may be the wrong concern. Both "captives" and "servants," after all,

tend to create costs for their "masters"—and at least in regulatory contexts, industries may have no need to take on those costs. If an industry's goal is simply to avoid regulations, in other words, then there may be no real need to capture the watchdog—the agency—or to keep it in captivity or servitude. Instead, the need is merely to keep the watchdog from biting, barking, or enforcing its regulations.

In the decades since Bernstein's classic definition, science/technology studies have suggested a possibility for influencing watchdog behavior that appears to deserve much greater attention than it has received to date—the careful allocating of the benefits of scientific doubts. As the following sections will spell out, although governmental regulation is widely expected to be based on "science," the notion of a clear and unchanging science has been known at least since the time of Kuhn (1962) to be misguided. Actual agency decisions, in fact, often take place in a realm that Weinberg (1972) called "trans-scientific"—one where questions can be *asked* in scientific language, but where the questions simply cannot be *answered* with anything like certainty, at least not in advance.

The reality, in short, is that the scientific evidence available for policy decisions—like scientific evidence in general—is likely to be ambiguous or incomplete (see, for example, Harrison 1991; Hattis and Anderson 1999; Hirt 1994; Jasanoff 1987, 1990; McMullan and Eyles 1999; Tesh 2000). Under such conditions—as illustrated by the decisions of December 21–22, 2004—actual outcomes may have less to do with what is "known" than with how the agency decides to handle what is *not* known. One possibility is to assume that the absence of complete proof is good news—that in the absence of proof of harm, safety should be assumed, as in the forestry decision noted above—while the other is to assume that the absence of proof is bad news, as in the drug-importing decision on the previous day. The key question, then, may well be whether organized industries and interest groups can change the odds that an agency will make one choice or the other, and if so, how.

In dealing with this question, we draw heavily on the growing body of literature that discusses the use of "science" as a rhetorical and/or legitimation device, including the work by Wynne (1982, 1992), Aronson (1984), Brown (1998), Clarke (1999), Davidson (2001, 2003), Dietz, Frey, and Rosa (2002), Draper (1991), Funtowicz and Ravetz (1992), Gusfield (1981), Jasanoff (1987), McMullan and Eyles (1999), Yearley (2002), or Alario and Freudenburg (2003) and Freudenburg (2005). One of the points that emerges from this literature is that, although there are obvious interactions between the findings and the framings of the scientific work (see, for example, discussions by Hannigan 1995; Harrison 1991; Tesh 2000), *the ways in which organized interests frame scientific work* may well be just as important as are the "real" quality of the scientific work and the status of the scientists involved in doing the work in question. As noted by

authors such as Brown (1998), Gusfield (1981) and Wright (1992), the framing of science as uncertain is at least as much a symbolic process as a scientific one.

In the following sections, more specifically, we will argue that the SCAM has the potential to exert as much leverage on actual policy decisions as high levels of scientific quality, scientific consensus, and formal scientific legitimation, *even in combination*. Given that this article offers only an initial statement of our argument, not final proof, we do not wish to claim that SCAMs will counterbalance scientific quality/consensus/legitimation in *all* cases, but we do wish to establish that SCAMs *can* do so even in the "toughest" cases. As a way to illustrate this point, we have sought *not* to be "representative" in our selection of cases, but instead to identify strong or "tough" tests—those where, either at the time or in retrospect, independent observers (including observers with high levels of formal status) have concluded that there was a strong scientific basis for imposing regulations, even if organized industry interests disagreed. Although we stress that we make no claims about a broader "representativeness" from these initial case studies, the preliminary findings do appear to suggest that SCAMs can be remarkably effective even in cases where most scientists see findings as strong or robust—indeed, even in cases where the findings are backed by clear and emphatic statements of scientific consensus from the most prestigious scientific organizations in the world. What our initial investigations suggest, in short, is that in cases where science is necessarily brought together with political and economic realities in the context of regulations, the most important factors, practically speaking, may have less to do with actual levels of scientific certainty or prestige than with the ability of politically skilled actors to construct and maintain the belief that science should mean absolute certainty—and that in the absence of "scientific certainty," no regulations should be put in place.

Yes, No, and Maybe

The key catch is that, as should by now be well known, "proof " and "certainty" are actually in short supply in the world of science. Nearly half a century ago—within a few years of Bernstein's classic definition of "agency capture"—Popper's (1959) work on scientific falsification had led most observers of science to conclude that hypotheses will rarely if ever be "proven"; instead, as textbooks routinely explain, the usual approach involves the effort to disprove, or to "falsify," an opposing or null hypothesis.

Even the process of falsification, however, tends to be probabilistic rather than deterministic. To put the matter as simply as possible, any scientific effort to test a given hypothesis, null or otherwise, can only come up with three answers—yes, no, and maybe. A "yes," to be more specific, involves clear support

for a given hypothesis; a "no" involves a clear rejection; and a "maybe" involves an indeterminate answer, where available data do not permit a clear-cut acceptance or rejection of the hypothesis in question.

A further challenge is that, although the common expectation is that agencies will make decisions on the basis of "scientifically proven facts," the reality is that such decisions are almost always empirically underdetermined—based largely on evidence that is in the category of the "maybe," being inherently ambiguous rather than being absolutely clear-cut. Contrary to a popular view of science as being neat, orderly, and definitive, in other words, the reality is that regulatory science—like "pure" science (cf. Latour and Woolgar 1986)—includes large and often important gaps in knowledge. In an assessment of the use of chemicals, for example, the National Academy of Sciences' National Research Council (1984) concluded that no significant level of information on toxicity was available for roughly 80 percent of the 53,000 chemicals in commerce as of the early 1980s, and the number of chemicals in commerce has grown dramatically since then.

In other contexts as well—far from being rare—scientific ambiguity or insufficiency may be *pervasive*. In the words of Jasanoff (1990:250), "agencies and experts alike should renounce the naïve vision of neutral advisory bodies 'speaking truth to power,' for in regulatory science, more even than in research science, there can be no perfect, objectively verifiable truth." In our own experience with environmental and technological controversies as well—an accumulated experience that amounts to more than 60 person-years—almost all of the "scientific" debates have centered on questions where the available findings have fallen into the category of "maybe." As soon as the relevant activists came to see the data as being sufficiently clear-cut to justify a clear "yes or no" answer to a given question, the argument would simply move to the "next" question—meaning one where no such clear-cut yes/no answer was yet possible.

In calling attention to the potential power of SCAMs, we do not mean to imply that either the forces of social legitimation or the rules of scientific method are unimportant; on the contrary, we believe that existing literature has already demonstrated the importance of scientific precision and of formal legitimation. As we will show in the sections that follow, however, we see evidence that a significant fraction of actual policy decisions may have less to do with either the quality or the social status of the scientific search for "proof " than with the question of *which side "wins" in the absence of proof*. In the case of prescription drugs, noted above, federal regulators decided that, in the absence of proof of *safety*, the government would continue to prohibit the re-importation of even U.S.-made prescription drugs from Canada. In the logging case the next day, by contrast, a different set of regulators decided that, in absence of proof of *harm*, the government would abandon safeguards that had been in place since

the Reagan administration, some 20 years earlier. In a world of scientific uncertainty, neither decision *could* have been based on clear scientific "answers." The key point is that they reflected two different ways of dealing with the remaining questions—and that they may have had more than a little company in that regard.

Statistical "Significance" and "Power." Although this point is rarely acknowledged in policy debates, these two possible responses to uncertainty have a strong connection to a distinction that many scientists first learned in their statistics classes, namely, the difference between Type I and Type II errors. For those who have forgotten the details, a pair of reminders may be helpful. First, there are basically just two ways in which a scientist can reach a false conclusion: "Type I" errors are created by accepting hypotheses that are ultimately shown to be wrong, whereas "Type II" errors are created by rejecting hypotheses that are ultimately shown to be true. Second, a "Type I" error is the one that scientists usually worry about—the "false positive" that is created by accepting a hypothesis that is ultimately judged to be wrong. Statistical textbooks on a typical scientist's bookshelves are likely to include dozens of ways of testing for Type I errors, complete with cutoff values and charts for rejecting hypotheses with 95 percent (or 99 percent or 99.9 percent) levels of confidence. Some textbooks even add discussions of Bonferoni corrections or other ways of dealing with the fact that one such test out of every 20 will prove "significant" by chance alone at the standard 95 percent level of confidence.

What will be far less likely to be found on the same bookshelves are any but the most cursory discussion of calculating the probability of "false negative" or Type II errors, *even at 50 to 80 percent levels of confidence*. Instead, the textbooks are likely to contain a brief mention of the fact that Type II errors involve the opposite risk—that is, the risk of rejecting a hypothesis that is ultimately judged true—along with a warning that, as noted above, there is an inherent tradeoff between Type I/II risks: For any given study, the higher the level of assurance that one is *not* committing one error, the higher the probability that one *will* be committing the other. The basic possibilities are noted in Table 1.

For a world of "pure science," it can make a great deal of sense to concentrate largely or even predominantly on Type I errors. The salient risk in such work is that, if we are not sufficiently "careful"—meaning careful about Type I risks—we may wind up granting too much credence to a hypothesis that is ultimately found to be wrong, potentially leading other scientists to waste their time in testing and rejecting a questionable line of thought. In decisions that involve real-world risks, by contrast, *the most important risk may well be just the opposite one*—the risk of assuming that a chemical or a technology is "safe" when in fact it is not. That, however, is rarely the way in which real-world policy debates unfold—a point that is worth examining in closer detail.

Table 1
Two Ways to Be Wrong in Science

"Hypothesis":	Technology is Safe	Technology is Risky
"Reality": Technology is Safe	(correct)	Type I error (usually avoided with 95% confidence)
Technology is Risky	Type II error (rarely avoided with even 50% confidence)	(correct)

Uncertain Futures. In 1972, when her son was first diagnosed as having acute lymphocytic leukemia (ALL), Anne Anderson began knocking on neighbors' doors and meeting with other disease victims, trying to learn more. After several years, she developed the hypothesis that her community of Woburn, Massachusetts, had an unduly high incidence of the disease, probably because of something in the water. She asked state officials to test the water but was told that the agency did not respond to individual citizen requests. In 1979, however, builders found 184 barrels in a vacant lot and called the police, who in turn brought in the state's Environmental Protection Agency—which found that some of the city's wells contained organic compounds that were known to be animal carcinogens, including some at concentrations that were 40-fold higher than allowable limits. In January of 1981—five days after Anderson's son died—the U.S. Centers for Disease Control (CDC) issued a report with two main conclusions. One was that in East Woburn, where drinking water came from the contaminated wells, the CDC could document 12 cases of childhood leukemia, when only 5.3 cases were statistically "expected." The other was that, overall, the CDC did not consider the evidence to be strong enough to reach a conclusion that the contamination had caused the leukemia.

As spelled out by Brown (1987, 1997; see also Brown and Mikkelsen 1990), Anderson's quest did not end with the CDC report. Instead, she and a number of her fellow residents ultimately became known for what Brown (1987) termed "popular epidemiology." They presented the case at a Harvard Public Health seminar, inspiring "a prototypical collaboration between citizens and scientists" (Brown 1997:142). In the end, the collaborative study with the Harvard biostatisticians reached the conclusion that childhood leukemia was in fact significantly associated with exposure to water from the contaminated wells—and they were not alone

in that conclusion. In 1986, after a federal district court jury found that the W. R. Grace company had negligently dumped chemicals, contributing to the problem, Grace agreed to an $8 million settlement. Another 9 years later, the Massachusetts Department of Public Health (DPH) released its own report, agreeing with the citizen–Harvard study finding that there was a dose-response relationship between childhood leukemia and exposure to water from the contaminated wells—although the DPH study challenged the citizen–Harvard conclusions that there were statistical associations with reproductive disorders, as well.

The Woburn experience is similar to what has happened in other communities, but with two exceptions. The first exception is the obvious one, namely, that citizens were able to team up with a set of respected scientists, giving their concerns a degree of legal as well as scientific credibility that few such citizen groups would be likely to enjoy on their own. The second exception, however, is the key one here: After teaming up, the citizen–scientist team was able to gather more data, including over 5000 interviews, greatly increasing the statistical power of their findings. Even after the additional data collection, as just noted, the citizen–Harvard findings remained the focus of dispute—but in hundreds of other communities, the level of indeterminacy has remained far higher. Rather than having 12 instead of the "expected" 5.3 cases of childhood leukemia, a more typical example involves a smaller community, having five or fewer cases instead of an "expected" rate of one case or less (Schulte, Ehrenberg, and Singal 1987). In such situations, it is simply not possible to say that there is "proof," at a 95 percent level of confidence, that a local contamination source is "significantly associated" with the diseases. Neither, unfortunately, is it possible to say that it is not.

That second decision, however, is the one that appears to be reached in a strikingly high fraction of all relevant cases. The "no proof of harm" decision was the one that was reached in Woburn, for example, until citizens had invested some 14 years in obtaining additional data. In other contexts as well, the pattern is sufficiently common that a study from the Institute of Medicine (1991:177) once referred to "Ozonoff's working definition of a 'catastrophe' as an effect so large that even an epidemiological study can detect it" (with a citation to David Ozonoff, Boston University School of Public Health, pers. comm. 1990). To be fair to epidemiologists, however, they may be far from the only scientists to have experienced pressures to emphasize an avoidance of "false positives" in their work—or to have demonstrated in a significant fraction of cases a willingness to do so. Closer examination, in short, is warranted.

Enjoying Benefits from Doubts

In a classic article, Stone (1980:978) noted that what he called "systemic power" was far from universal, in that "business interests are often divided or

defeated," but that business interests did nevertheless win a far higher proportion of battles than could be explained on the basis of chance alone. In regulatory contexts as well, if every case where a regulation is actually imposed is taken as an indicator of a lack of business success, then it is clear that "business interests are often divided or defeated," even where the available scientific evidence is ambiguous or incomplete. Despite that fact, however, organized business interests appear to have enjoyed considerable success by putting pressure on agencies to avoid any regulation that cannot be shown to be *unambiguously* justified (for further discussion, see Freudenburg and Pastor 1992).

The following cases are intended to illustrate this point, but to repeat, we make no claims for having discovered a "universal pattern" or anything close to one. Instead, our goal is simply to illustrate that similar patterns have emerged in some of the toughest cases we have been able to identify—not just in cases where little scientific work had been done, in other words, but in cases where the body of available scientific evidence ultimately came to be substantial or even overwhelming. Given that there is no known universe of "tough cases," however—let alone a known technique for sampling from such a universe—our main focus in this article is to pull together evidence from enough cases to illustrate the value of doing additional research, and for exploring the conditions under which such other contexts may reflect comparable patterns or different ones.

Given our desire to include cases where relevant scientific findings are available, our cases will be drawn mainly from controversies that have already been investigated and documented by others. We believe that such cases can have special value, given that none of the original researchers appear to have shared our interest in SCAMs. If SCAMs do appear to deserve greater attention, based on these existing studies, this would further reinforce our article's main theme, namely, that the pattern is sufficiently widespread to deserve greater attention. To counterbalance any temptation to sample from a limited range of experiences, however, we have sought to make our list of cases a deliberately diverse one, involving food and toxics, fire and smoke, public health, precaution, and more. Some of the best data, of course, come from historic cases such as Brown's "popular epidemiology" in Woburn, where enough information has become available to provide a relatively clear picture, but to avoid creating the impression that the pattern is simply an old one, we will include newer as well as older cases.

The first pair of cases—one recent, one older—involve agency implementation of specific regulatory statutes that are already "on the books." The first case in this pair is the one where, given the nature of the statutory language, the scientific evidence is perhaps the weakest of any of the six cases to be considered in this section of the article, while the second is one where, based on a truly substantial scientific record, the same agency nevertheless appears to have

shown a comparably limited pattern of enforcement. The next two cases—again, one recent and the other older—involve arguments over whether new laws should be added to the books. The third pair of cases will extend the sampling geographically and historically, first examining the implementation of legislation in another country (Australia) that explicitly required agencies to err on the side of caution, and finally considering a much older case, dating from the early days of statistical analysis. In this instance, perhaps tellingly, industrial interests appear to have used only a partial and less sophisticated version of the SCAMs than would be developed over the decades to follow.

(1) Existing Statutes: Food and Toxics, Pestilence and Pesticides

For years, critics of chemical-intensive American agriculture have worked hard to achieve three major changes in pesticide regulations. First, they have sought to move key responsibility away from the U.S. Department of Agriculture (USDA)—which is seen by many as a classic example of a "captured" agency—to the Environmental Protection Agency, or EPA, which is often seen as being "more neutral." Second, given evidence that rapidly growing children, for example, may be far more vulnerable to chemicals than a "typical" participant in a medical trial, such as a college student or healthy, working-age male (see, for example, McMichael 1976), activists have argued that regulations should protect those who are most vulnerable—particularly pregnant mothers, children, the elderly, and the infirm—rather than treating all humans as more or less equivalent "receptors." Third, given that people can now be exposed to what activists call a "toxic stew" of chemicals, activists have argued that chemicals should be regulated in terms of synergistic or interactive effects, rather than in isolation from one another (for peer-reviewed findings on the importance of chemical "synergies," see, for example, Cavieres, Jaeger, and Porter 2002; for a broader and less technical summary, see Colborn, Dumanoski, and Myers 1997).

It is well known that activists have long been pushing for all three of these reforms. What is less well known is that, more than a decade ago, Congress passed a law—unanimously—that enacts all three. Since President Clinton signed that bill, on August 3, 1996, the "Food Quality Protection Act," or FQPA, has made all three provisions the official law of the land. When he signed the FQPA, President Clinton said that the Act ". . . puts the safety of our children first. . . . It sets clear, consistent standards for all pesticide use on all foods for all health risks. It also sets that standard high." From the perspective of the present, however, it is not altogether clear what the legislation will ultimately be judged as accomplishing, beyond allowing politicians to make such stirring speeches. At least in the view of skeptics, finally, the absence of more concrete accomplishments may not be entirely accidental.

The evidence that supports the skepticism begins with the fact that the original legislation sailed through Congress with no opposition, bringing to mind the old joke that the only time Congress will agree on anything is if its members have found a way to dodge the real issues. At least according to Washington insiders, moreover, something like that appears to have occurred when the FQPA was passed in August 1996: Congressional Republicans wanted to make sure President Clinton and Democrats would not have a popular environmental issue to use against them in the 1996 elections, and industrial interests remained largely quiet while Congress debated the bill. On the other hand, as noted in one of the few mass media reports that continued to focus on the topic after the initial flurry of interest (Overby 2000), lobbying became much more active after the elections. Farm organizations provided the visible "grass-roots" political leverage, but the funding came largely from the pesticide industry and the "American Crop Protection Association," or ACPA, which has since changed its name to "CropLife America."

The ACPA advertising campaign used creative techniques such as giving away fly swatters, to convey the argument that farmers would have no other way to protect their crops if regulations were actually strengthened. Confidential discussions with EPA insiders, however, suggest that the more important aspects of the "implementation" of the FQPA may have been those that were less visible. Even EPA's own advisory group on the bill had a name that rarely comes up at the typical kitchen table—a 44-member committee called the "Committee to Advise on Reassessment and Transition," or CARAT. Still, at least, EPA's committee included consumer and environmental representatives. Other relevant groups did not. In particular, the blandly named "Implementation Working Group," created by farm groups and the chemical industry, was limited to highly skilled consultants and lawyers. Among its other actions, the "Working Group" hired the man who had previously been *running* the EPA pesticide office, asking him to help slow down the implementation process. The resultant game plan focused on forcing EPA to demonstrate scientific certainty before acting.

In support of its efforts, the Implementation Working Group enlisted a range of allies, from farm-state lawmakers to President Clinton's well-known "environmental Vice President," Al Gore—who agreed to send EPA a memo, leading the EPA to establish yet another committee, called the Tolerance Reassessment Advisory Committee (TRAC), established jointly with USDA, and intended to promote what proponents were careful to call "sound science." Although TRAC included representatives of a wide range of interests, all of the public-interest members of TRAC ultimately "resigned *en masse*, noting that TRAC was stalling implementation, not helping to guide it" (Consumers Union of the United States 2001:6). In another development—although of course it

would be impossible to "prove" that politicians would be inspired by the generous campaign contributions that lined up with such actions—more than half of the members of the House of Representatives cosponsored a bill that would have forced EPA to go even further. The proposed legislation would have required EPA to use a closer approximation to the industry's version of "sound science" before moving forward with regulations that would in fact "put the health of the nation's children" first—or for that matter, before implementing any regulations that would put the health of the children even roughly on par with the "health" of the pesticide and agricultural industries.

Although that bill did not become law, other aspects of the industry game plan succeeded in nullifying the act's explicit requirements for prompt implementation. As noted in a relatively early discussion of the FQPA (Reichhardt 1998), the act "came with tight deadlines." One-third of pesticide uses were supposed to have been reassessed by August 1999, with another third reassessed by 2002, and the remainder by 2006—but the actual progress was so much slower that the Natural Resources Defense Council and several other groups sued EPA. In 2001, the EPA agreed to a consent decree requiring faster progress (see http://www.epa.gov/oppfead1/cb/csb_page/updates/nrdcdecree2.pdf). Even so, as the final version of this article was being submitted to *Sociological Inquiry*, during the supposed completion year of 2006, the latest implementation "progress report" on EPA's Web site was from 1999 (http://www.epa.gov/oppfead1/fqpa/fqpareport.pdf). It referred mainly to the "review" of risk assessments—and to the fact that EPA had approved over 1300 new minor uses of pesticides rather than disapproving old uses. The new uses may well have been beneficial at least in relative terms, in that they involved what EPA characterized as "safer pesticides," but what is striking is the general disjuncture between the "tight deadlines" in the law and the slow pace of actual agency regulations. The 1999 "update" that remained on the EPA Web site in 2006 measured progress in terms of 21 Reregistration Eligibility Decisions (REDs) involving cancellations, deletions, or declarations of ineligibility for reregistration, but sources we consulted were able to name only one chemical that had actually been banned for consumer sales—Dursban, a Dow Chemical pesticide, technically known as chlorpyrifos—and it continued to be sold by Dow, under the name of "Lorsban," for farm use.

To be fair, the outright banning of chemicals, or even of certain uses of chemicals, is not the only potential measure of the effectiveness of this legislation. At least in the eyes of the American Farm Bureau Federation and its state-level affiliates, the FQPA had long continued to be seen as one of the top "threats to American agriculture." Missouri's Farm Bureau, for example, continued through 2006 to describe the bill as being "of the utmost concern to Missouri farmers and ranchers" (see http://www.mofb.org/LA_Policy2006.htm).

As the Farm Bureau argued, the law could be important even if chemical companies simply chose not to seek approval of new chemicals. In response to direct inquiry in 2004, however, the Vice President of the Public Relations of Nebraska's Farm Bureau checked with the National office and gave a "bottom line" answer that "no products have been entirely cancelled under FQPA" (Stubbendieck 2004).

Even beyond actual cancellation, only a handful of other chemical uses have been directly affected by the FQPA to date. Perhaps the most important in risk terms were 10 specific food-crop uses for the organophosphate insecticide, methyl parathion—one of the most intensely toxic chemicals ever used on food crops—which was banned just before the 1999 report was issued. Other uses involve a chemical that has effectively been removed from the market by the company that had previously distributed the product—an organophosphate known as isofenphos, sold under the name of Oftanol—which had previously been used on golf turf and ornamentals. Perhaps the best-known chemical to be affected is Diazinon, a neurotoxin class insecticide that was banned for indoor use in 2000—although, as in the case of Dursban/Lorsban, it remained available to farmers, with some stores continuing to sell supplies already on hand (DiFonzo 2003). In most other respects, despite the Farm Bureau's view of the FQPA, most of the Act's "threats" to agriculture have been neutralized by the industry's ability to forestall EPA action in the absence of full scientific "certainty."

If this article's argument is correct, however, the battle over the FQPA should not be an isolated example of SCAMs. Instead, at least roughly comparable patterns of success in resisting regulatory action should also be evident in cases where the agency was able to produce a considerable body of scientific evidence—not just showing that certain substances had *not* been proven safe, but actively showing evidence of *harm*. To examine that possibility, it is helpful to introduce another acronym, and to consider the ways in which EPA got burned—ironically—in regulating asbestos.

The Drama of TSCA. One of the grandest of all grand operas is Puccini's *Tosca*, in which all three main characters wind up double-crossed and dead, with the soprano throwing herself over the wall of the castle in a high-volume finale. At least according to environmental groups (see, for example, Roe et al. 1997), some of the same kinds of chicanery characterize the law that shares the pronunciation of the opera—the Toxic Substances Control Act of 1976, or TSCA. In informal discussions, activists often describe this act with grim humor as the "Toxic Substances *Conversation* Act," because they believe court rulings make it "virtually impossible to get a known high-risk chemical off the market" (Meyerhoff 2001:1). Perhaps in part because this act was passed some two decades before the FQPA, it may illustrate even more clearly how effective

an industry can be in pursuing SCAMs, particularly if the industry happens to enjoy a few extra breaks along the way.

Environmentalists' grim assessments today stand in stark contrast to the views that held sway when TSCA was first passed. Initially, it was seen as one of the "landmark" environmental laws of the 1970s, along with the National Environmental Policy Act, Clean Water Act (1972), Endangered Species Act (1973), and Safe Drinking Water Act (1974). Congress passed TSCA in response to high-profile contamination incidents, including a discovery of polychlorinated biphenyls (PCBs) in the Hudson River and elsewhere, threats of stratospheric ozone depletion by chlorofluorocarbons (CFCs), and contamination of beef and milk by polybrominated biphenyls (PBBs) in Michigan. In brief, TSCA required EPA to identify chemicals that might pose unreasonable risks, and to act if the agency found "a reasonable basis to conclude that the manufacture, processing, distribution in commerce, use, or disposal . . . presents, or will present an unreasonable risk of injury to health or the environment" (see http://www.epa.gov/region5/defs/html/tsca.htm).

As noted in a Congressional Research Service assessment of the bill (2003), the legislative history of the bill "includes a presumption that testing of new products would take place before they were widely used," but thousands of chemicals that were already in use in the 1970s—some 80 percent of the chemicals in commerce today, by common estimates (Schapiro 2004)—were "grandfathered in," remaining in use without going through additional testing. In addition, many new chemicals have entered the marketplace with so little regulatory response from EPA that the European Union moved to a more aggressive approach to regulation, including outright bans on at least a dozen chemicals under the Stockholm Convention on Persistent Organic Pollutants (see http://www.pops.int/).

One reason why the trajectory of excitement has been so different for the regulatory TSCA as opposed to the operatic Tosca—peaking at the outset, rather than in the finale—is that the chemical industry organized early and effectively, focusing largely on the effort to require scientific certainty before EPA would be allowed to impose any real regulations. As noted by Morris (1998), a study from Society of the Plastics Industry argued that tough regulations would be "needlessly" expensive, with a minimum economic impact of $65 billion, eliminating at least 1.6 million jobs—although in reality, "the industry did not disappear," and neither did the 1.6 million jobs. Demanding higher standards of scientific certainty for EPA, on the other hand, proved to be highly successful.

In brief, industrial interests put together a case in what is sometimes seen as the most conservative or probusiness "circuit" for the U.S. Court of Appeals, namely the Fifth Circuit Court (headquartered in New Orleans, and including Louisiana, Mississippi, and Texas). The case, officially known as *Corrosion*

Proof Fittings v. EPA (947 F.2d 1201 (5th Cir. 1991)), had to do with an EPA decision that would have sharply limited future use of asbestos in most products (for a fuller discussion, see Schierow 2003).

At the time, many observers felt EPA's evidence on asbestos dangers came as close to true scientific certainty as in the case of any substance ever studied. Among other steps, the agency had conducted a 10-year study costing $10 million, had completed agency reviews of over 100 scientific studies, and had reached an official scientific conclusion that asbestos was a potential carcinogen at all levels of exposure. Based on 100,000 pages of administrative record, EPA concluded that asbestos exposure posed "an unreasonable risk to human health" under the law, and that roughly 94 percent of all asbestos uses would thus be banned (54 Fed. Reg. 29, 460 (1989); see also EWG Action Fund 2004; Stadler 1993). Industry groups, however, called the ban "death by regulation"—and Judge J. E. Smith of the Fifth Circuit basically agreed, concluding that EPA had "presented insufficient evidence" and that it had "fail[ed] to consider all necessary evidence." Although the decision by the Reagan-appointed judge was widely criticized, the first Bush administration chose not to appeal it, causing the former EPA Assistant Administrator who had written the first draft of TSCA to describe the decision as "the end of any attempt to use Section 6 of TSCA to regulate individual chemicals" (quoted in Schierow, pers. comm. 2004). An article in the *Duke Law Journal* reached a similar conclusion, noting that, in the absence of appeals or clarifying legislation, EPA's first "rulemaking" under the law would "undoubtedly be its last" (McGarity 1992:1385). At least in this case, in other words, a single well-aimed lawsuit, relying heavily on SCAM, proved sufficient to stop the implementation of what was initially seen as a legislative landmark. In the words of one observer who asked not to be quoted by name, "that lawsuit was as effective as David's shot at Goliath—except in this case, it was Goliath who had the slingshot and the right aim."

(2) New Legislation, Fire and Smoke: Global Warming and Cigarettes

If the FQPA and TSCA both illustrate the potential effectiveness of SCAMs where apparently strict legislation has already been passed, the next question has to do with the effectiveness of such techniques in preventing the passage of legislation in the first place. Again in this subsection, we will consider one case that is still unfolding, and another one that is significantly older.

Turning Up the Heat. We turn first to the regulation of pollutants that are sometimes called "greenhouse gases," given their tendency to trap heat in the earth's atmosphere, contributing to global warming. Although the second Bush Administration has been identified as being particularly hostile toward regulations

on oil and coal industries (see, for example, Austin and Phoenix 2005; cf. Kleinenberg 2002), debates over global warming can be helpful in allowing us to ask *how* regulations are opposed—that is, to ask what specific techniques are being used. Despite the fact that the science in this case shows a high level of consensus, the answer has involved an emphasis on uncertainty. An analysis of the Administration's "Climate Change Science Program"—which called for years of additional study and delay—found that "summaries of the report use the word 'uncertainty' 15 times and the phrase 'fossil fuels' only once. But uncertainty is more in the eyes of politicians than of scientists . . . an overwhelming majority of climate scientists say global warming is manmade and is caused primarily by burning fossil fuels" (Borenstein 2003:A4). Two years later, when scientific documents were being doctored by the American Petroleum Institute's leader of the oil industry fight against limits on greenhouse gases—who had since become chief of staff for the White House Council on Environmental Quality—an analysis by Revkin (2005) showed that the major focus of the doctoring was to add an emphasis on uncertainty and remove references to scientific consensus.

As spelled out perhaps most clearly by McCright and Dunlap (2003), the challenges to scientific consensus on global warming are particularly impressive: The traditional focus of work on "the second face of power" (Bachrach and Baratz 1970) involves keeping an issue off the agenda and out of public consciousness (see also Crenson 1971; Stone 1980). By contrast, global warming provides a case where industry interests have needed to attack some of the most prestigious scientific organizations in the world—doing so *after* the issue had already gained a place on the agenda—and doing so with such effectiveness that "policy-making ground to a halt" (McCright and Dunlap 2003:349).

This case, in short, reveals the effectiveness of SCAMs even in the face of some of the most extensive scientific certification and legitimation ever assembled. An impressive array of scientific bodies have by now formally concluded that global warming is "real" and caused largely by humans; perhaps the most intensive assessments have been provided by the international scientific panel known as the Intergovernmental Panel on Climate Change (IPCC)—an effort involving more than 2000 of the world's best-respected climate scientists, from more than 100 countries. That Panel's overall assessments (see, for example, Intergovernmental Panel on Climate Change 1995, 2001, 2007) conclude that global warming is real and anthropogenic, with global concentrations of carbon dioxide having reached the highest levels to be seen on this planet in at least 420,000 years, and possibly 20,000,000 years. The IPCC assessments are also backed by the most prestigious of national scientific bodies, including the U.S. National Academy of Sciences (2005) and the British Royal Society. When Congressional opponents of regulation tried to undercut the legitimacy of the

most recent summary from the IPCC by seeking additional, independent assessments from the National Academy of Sciences, what they received instead were additional confirmations of the international consensus (see, for example, National Academy of Sciences/National Research Council 2001; National Academy of Sciences/National Research Council 2005; see also the more detailed analysis by Fisher 2004).

On one side of this debate can be found virtually all of the relevant climate scientists in the world, demonstrating a degree of consensus that most social scientists can only dream about. On the other side are a much smaller number of contrarian scientists and their allies—but also the ability to invoke SCAMs. The net result, at least up to the time when this article was completed, was that the small number of climate skeptics, backed by outspoken politicians (see, for example, Inhofe 2005; Pegg 2003), have generally carried the day in U.S. policy debates—doing so largely by constructing the belief that, extensive formal assessments of the available evidence notwithstanding, the science is "uncertain" (Borenstein 2003). As noted by Trumbo (1996; see also Gelbspan 1997; Shanahan and Trumbo 1998), a strikingly high fraction of all media reports about "scientific" disputes over global warming, particularly during the 1990s, actually quoted only a small handful of skeptics—many of whom had been funded by affected industries and/or by politically conservative "Think Tanks" that proved to be especially adroit at publicizing the results of their studies (see McCright and Dunlap 2000, 2003; see also Fiore 1997; Krehely, House, and Kernan 2004). A number of the best-known skeptics were not even climate scientists. Even so, the ability to demand Scientific "Certainty" provided so much leverage that the critics received a disproportionate share of mass media attention, especially while the United States was debating the ratification of the Kyoto Accords for slowing global warming (see Fisher 2004).

By late 2004, even the second Bush Administration appeared at times to be bowing to the ever-strengthening scientific consensus, as when the Assistant Secretary for Commerce, Dr. James Mahoney, reported to Congress that emissions of carbon dioxide and other heat-trapping gases are "the only likely explanation for global warming" in recent decades (Revkin 2004:A18). Other politicians, however, maintained their positions. Referring to the work of the contrarian scientists—and of the well-known fiction writer, Michael Crichton—powerful politicians such as Senator Inhofe (2005:S18), Chair of the Committee on Environment and Public Works, argued in Congress that "man-induced global warming is an article of religious faith to the radical far left alarmists." Even in this vivid speech, however, SCAMs are evident; quoting Crichton, Senator Inhofe argued, "Nobody knows how much of the present warming trend might be a natural phenomenon," and "Nobody knows how much of the present trend might be man-made" (Inhofe 2005:S18).

A few months later, similarly—despite its previous, apparent change of mind—the Bush White House "enlisted an outspoken skeptic of global warming" in a fight regarding overseas energy projects (Kintisch 2005:482). Although other scientists interviewed by *Science* characterized this new spokesman's arguments as "wishful thinking," "a selective use of studies and half-truths," or more concisely, "standard skeptic crap," the skeptic in question kept his focus on scientific certainty, questioning whether the IPCC represented "a true consensus," and insisting that his own skepticism was backed by "a lot of dissenting views" (Kintisch 2005:482). Whatever their number or scientific credentials, the dissenters appear to have been able to exert enough leverage on policy debates to counterbalance an almost complete consensus among the world's climate scientists, even though the latter group was the one that was backed by formal endorsements from what are probably the most prestigious scientific bodies on the face of the earth.

Where There's Smoke, There May Be a Smokescreen? Of all the possible cases in which an industry has stood to benefit from delaying or avoiding legislation, perhaps the most extensive body of evidence is provided by the tobacco industry's efforts to argue that there was not enough scientific evidence to regulate cigarettes. Part of the reason has to do with the extensiveness of biomedical research on smoking, but another part of the reason is that much of the evidence regarding the industry's actual efforts was eventually made public through litigation (see, for example, Bailar 2006; Glantz et al. 1996; Hilts 1994, 1996; Rampton and Stauber 2001; Warner 1986).

What the evidence shows, in part, is that during roughly half of the twentieth century, the industry-funded "Council for Tobacco Research" (CTR) played a role having considerable resemblance to the later actions of conservative think tanks in global warming debates, except that CTR may also have helped to shield specific tobacco companies from liability (Glantz et al. 1996; Harris 1994; Warner 1986). As noted in a Peabody Award-winning series of reports by the science writer, Richard Harris (1994), company documents noted that an "independent" CTR could "avoid the research dilemma presented to a responsible manufacturer of cigarettes which on the one hand needs to know the state of the art and on the other hand cannot effort the risk of having in-house work turn sour." If CTR research were to show high risks from cigarette smoking, in other words, the tobacco companies could maintain plausible deniability about their awareness of findings that would never be finalized and disseminated. On the other hand, any findings that might undermine the growing scientific consensus about the risks of cigarette smoking—or that might prove useful in using SCAMs more broadly—could of course be published and publicized with considerable fanfare.

Documents on file at the Legacy Tobacco Documents Library at the University of California, San Francisco (UCSF), available on-line at http://tobaccowall.ucsf.edu/1950.html#kax60e00, show that U.S. tobacco manufacturers and growers met with lawyers and public relations agents at New York's Plaza Hotel in December 1953, setting up a "Tobacco Industry Research Committee" in 1954 "to meet the challenge raised by widely publicized reports in the press, purporting to link tobacco smoking with the cause of lung cancer" (Bates Number: 93219062/9070). They might well have been worried about reports in the medical "press" (e.g., Wynder and Graham 1950), which were starting to show cigarette smokers to be some 50-fold more likely to get lung cancer than nonsmokers. Even based on evidence that strong, however, an industry with the deep pockets of the cigarette makers—and the technique of the SCAM—would be able to delay or evade a good deal of regulation for nearly half a century. It would be enough delay to allow all of the executives who met at the Plaza Hotel to retire and/or die before any significant regulations began to take effect.

Among other activities, cigarette makers and the industry "Research Committee" published a so-called "Frank Statement" in full-page newspaper ads, stating "We accept an interest in people's health as a basic responsibility, paramount to every other consideration in our business. We believe the projects we make are not injurious to health" (http://tobaccowall.ucsf.edu/1950.html#kax60e00). As another old joke has it, that was their story, and they stuck to it. In Congressional hearings 40 years later, in 1994, seven Chief Executive Officers from tobacco companies would still testify that "nicotine is not addictive" (http://tobaccowall.ucsf.edu/1990p2.html). The Harris series reported that, by that same year (1994), the industry may have invested over $200 million in CTR work—much of it apparently designed to defend companies against lawsuits and "to create doubt about the risk of cigarette smoking" (Harris 1994). Some studies were supported in even more secretive ways. The CTR 1993 annual report, for example, supposedly named all past and present grant recipients, but it did not mention Theodore Sterling, of Simon Frazier University, who questioned the Scientific Certainty of findings on tobacco smoke in office buildings, and on the link between chewing tobacco and oral cancer. In a later, partial listing of CTR's previously secret "special project grants," however, Sterling and his associates were listed as receiving more than $3.7 million between 1973 and 1988 from a special projects fund. As noted, for example, by Chapman (2003), tobacco industry interests in other nations have seen similar advantages in "remaining anonymous."

It was not until roughly a dozen years after the Plaza Hotel meeting—after the release of the widely noted Surgeon General's report on Smoking and Health (1964)—that Congress would pass the Federal Cigarette Labeling and

Advertising Act of 1965, requiring health warnings—albeit with uncertainty—"Caution: Cigarette Smoking May Be Hazardous to Your Health." The labels did not drop the "may" language until Congress passed the Public Health Cigarette Smoking Act in 1970, after which the labels warned that "The Surgeon General Has Determined That Cigarette Smoking Is Dangerous To Your Health." Still, unless it can be argued that yet another old saying is wrong, and that words actually speak louder than actions, these laws were by no means a complete defeat for the tobacco industry. Among its other provisions, the 1965 law preempted more tangible regulatory actions from federal as well as from state and local entities; in addition, for decades thereafter, cigarette manufacturers would shield themselves from many forms of liability by arguing that the warnings, after all, had been present on all cigarettes sold in the United States after 1965, meaning that later courts should presume that cigarette smokers were fully informed about the risks they faced.

A different picture, however, finally began to emerge some three decades later—in May of 1994, a month after tobacco CEOs had testified to Congress that "nicotine is not addictive"—when Stanton Glantz of UCSF, a leading expert on second-hand smoke, received a package from "Mr. Butts" (named after the Doonesbury cartoon character). The box contained what came to be known as "the cigarette papers"—some 4000 pages of secret tobacco industry documents. By February 1995, the Brown & Williamson tobacco company demanded that the UCSF library return the "Mr. Butts" documents, sending private investigators to stake out the library after the UCSF librarian refused, and suing UCSF on Valentine's Day, 1995. A week later, however, Florida became the first of many states to sue the tobacco companies, and the courts soon threw out the lawsuit against UCSF. In July 1995, an issue of the *Journal of the American Medical Association* (vol. 274:3) was devoted to an analysis of the secret documents by Dr. Glantz and his colleagues (see also Glantz et al. 1996). Based in part on information from the previously secret documents, cigarette makers experienced their first important legal loss, being forced to pay damages to a smoker for the first time in August 1996, more than 40 years after the meeting at the Plaza Hotel. The beginning of the losses, however, came close to the end: By July 1997, Mississippi's Attorney General announced the industry had agreed to pay $3.4 billion to settle the state's lawsuit for Medicaid expenses, and other state settlements soon followed. On January 28, 1998, tobacco industry executives finally testified before Congress that nicotine is in fact addictive, and that smoking at least "may" cause cancer.

(3) Getting Out the Precaution, Getting in the Lead

The final two cases have been selected to extend the types of cases being considered in two ways. First, given that "the Precautionary Principle" appears

to be nearly the opposite of the SCAM, we will examine what happened when the Precautionary Principle was given the strongest legal status it appears to have held in any nation to date, namely, in Australia. Second, given our emphasis on Type I/II errors in statistics, we will consider a case that predates the debate over cigarettes and health, having begun before present-day understandings of statistical significance were well established, involving the introduction of lead to gasoline during the 1920s.

The "Precautionary Principle." Over the past decade or more, arguments for protecting public health and the environment have often hailed what Cameron and Abochar (1991) have called a "fundamental" or "revolutionary" idea, mainly the precautionary principle. The basic definition is provided by the Rio Declaration on Environment and Development 1992 (Principle 15): "Where there are threats of serious environmental damage, lack of full scientific certainty should not be used as a reason for postponing measures to prevent environmental degradation" (for more detailed analyses, see especially Bro-Rasmussen 1999; McIntyre and Mosedale 1997).

On the surface, such a declaration would appear to call for an approach to regulation that would be the flip side of the SCAM. In essence, following the Precautionary Principle means *not* waiting for "certainty" before acting to regulate threats to public health and the environment (see, for example, the discussion by O'Riordan and Cameron 1994; O'Riordan and Jordan 1995). As spelled out perhaps most clearly by Fisher (1999), however, its actual "implementation" has proved not to be so different from earlier SCAMs after all.

Although the principle originated in Germany in the 1970s and was included in international treaties by the late 1980s, nowhere does the process of enshrining the principle in legislation appear to have gone as far as in Australia. In the early 1990s, Australia used the precautionary principle as the core of a "national strategy for ecologically sustainable development" (Commonwealth of Australia 1992). The Principle was also integrated into environmental decision making in other contexts, including the Intergovernmental Agreement on the Environment, National Forests Policy Statement, Landcare Plan, and National Water Quality Strategy. Not only was the principle expected to apply to nearly all aspects of environmental decision making, but it was explicitly incorporated into a number of pieces of legislation, both in individual states and at the national level, in laws that included Australia's National Environmental Protection Council Act of 1994.

At least one study, however, found that, although the principle enjoyed relatively widespread support, it was supported for a variety of reasons and tended not to be widely understood (Harding, Young, and Fisher 1996). Interpretations of the Principle have been widely varied within the academic community

as well, ranging from largely statistical interpretations of the sort being stressed in this article, to those that have seen the Principle as challenging the authority of science, as part of the larger critique of the Enlightenment project (e.g., Giddens 1990). The varying academic interpretations of the Precautionary Principle may well have contributed to the ways in which Australian courts chose to interpret the relevant legislation—but so, apparently, did a pattern of SCAMs.

To most scientists, the Precautionary Principle sounds remarkably similar to recommendations that are so often put forward by scientists themselves in the face of uncertainty—specifically including the famous medical dictum, "first of all do no harm." Critics such as Milne (1993) and Stein (1996), however, have argued that the Precautionary Principle is more like an unscientific statement of good intentions—clear enough to be used in international or diplomatic circles, perhaps, but simply too vague to be put into practice in regulation. Although the Principle is widely understood as stating that scientific uncertainty should not be used as a basis for inaction, in other words, such a formulation does not spell out what *should* be taken as an appropriate basis for action. A number of courts held that the principle thus conflicted with the "rule of law," or an at least equally vague legal principle, holding that the law should be clear, coherent, and sufficiently "stable" so that people can comply with it (see, for example, the discussion in Finnis 1980; see also Craig 1997; Fallon 1997). In one influential decision, for example, the Judge expressed the following view:

> [W]hile [the precautionary principle] may be framed appropriately for the purpose of a political aspiration, its implementation as a legal standard could have the potential to create interminable forensic argument. Taken literally, in practice it might prove unworkable. Even the applicant concedes that scientific certainty is essentially impossible. It is only 500 years ago that most scientists were convinced that the world was flat. (*Nicholls v. Director General of National Parks and Wildlife Service* ([1994] 84 LGERA 397:419)

Note that the decision depicts the agency as "conceding" the very point that social studies of science have repeatedly stressed, namely, that "scientific certainty is essentially impossible." On the other hand, even though the Precautionary Principle required that such an absence of scientific certainty should *not* be taken as a reason to avoid regulatory action, the judge then took uncertainty as the reason to do just that. In other cases as well, Australian courts soon began to rule that, in the absence of clearer statements of what agencies *should* do, the Precautionary Principle should be seen not as endorsing typical forms of scientific conservatism, but just the opposite. Industry lawyers (and sympathetic judges) began to characterize even straightforward readings of the Precautionary Principle as being akin to making decisions by flipping a coin or by consulting an astrologer (see Fisher 1999). In short order, in a decision that may have become influential in part because it offered a simple way to relate the new Principle to old practices, the court in *Leatch v. National Park and Wildlife*

Service ([1993] 81 LGERA 270) ultimately declared that the new Precautionary Principle and the older agency practices were more or less the same. This decision held that the new Principle was merely a common-sense reaffirmation of the importance of "taking a hard look" at the available evidence before acting—meaning that the new legislation should be seen merely as endorsing the kinds of decision making that were already being done in Australian agencies before the nation's elected representatives felt the need to enact new laws (see, for example, *Friends of Hinchinblook Society Inc. v. Minister for Environment* [1997] 142 ALR 632 and other cases reviewed by Fisher 1999). Rather than being interpreted as changing the ways in which agencies should strike a balance between Type I and Type II risks, in short, at least Australia's Precautionary Principle quickly came to be seen as yet another way of *requiring* Scientific Certainty before imposing regulations.

Getting the Lead In. An older and thus potentially telling comparison, finally, is provided by the introduction of lead into gasoline in 1923—just months after a laboratory director of the U.S. Public Health Service called leaded gasoline a "serious menace to public health," and some 2000 years after lead had been identified by ancient Greeks as a poison. As noted by the Natural Resources Defense Council (1997), there was no official federal body with the power to investigate such a product in 1923—but in addition, the notion of "statistical significance" was not yet established at the time. The first edition of Fisher's classic statistics text, for example, would not be published until 1925.

Perhaps in part for these reasons, the spokespersons for leaded gasoline appear to have been less skilled in framing their arguments than their later counterparts would be. In particular, rather than remaining focused on requiring opponents to prove that the product was risky, the proponents of leaded gasoline tended instead to invoke a less sophisticated form of a SCAM—arguing that potential risk concerns were not proven, to be sure, but placing most of their emphasis on distracting attention away from questions about their product's risks (and their own profit motives) by emphasizing instead the potential benefits to nearly everyone in society. As noted in particular by Rosner and Markowitz (1985) and Markowitz and Rosner (2002), they would argue that this poison was a "Gift of God," and that keeping it out of gasoline would be a tragic mistake.

The industry spokespersons were aided by the fact that only the industry was supporting relevant research at the time (Needleman 2000). When lead was first added to gasoline, the Surgeon General did write to "inquire whether there might not be a decided health hazard," but the Public Health Service had few options except to rely on the industry for the answers. Ironically, the primary developer behind the use of leaded gasoline at General Motors, Thomas Midgley, was forced to decline speaking engagements in January 1923 because

of lead poisoning, but he did respond to the Surgeon General's letter. With little evident concern about balancing the burdens of proof, or for that matter about the fact that no experimental data had been collected at the time, he assured the Surgeon General that the use of leaded gasoline would be safe.

Beginning in the next year of 1924, when GM did hire a toxicologist and set up a medical committee—chaired by Standard Oil's own consulting physician—the committee produced a highly cautionary report (Kitman 2000). That report, however, was not made public. Instead, GM tried to generate other, "independent" studies to demonstrate the safety of leaded gasoline. After several reputable academics turned them down, the companies went to the U.S. Bureau of Mines, which agreed to do the studies. As Rosner and Markowitz (1985:345) spell out, the Bureau's initial agreement with GM in September 1923 was fairly even-handed, but by June 1924, GM added a stipulation that the Bureau should refrain from "giving out the usual press and progress reports," because "the newspapers are apt to give scare headlines and false impressions." It also required that, before publication, all manuscripts be submitted to GM for "comment and criticism." Two months later, the provisions were amended again, requiring all papers or articles to be submitted "for comment, criticism, *and approval*" (emphasis added).

In fact, the Bureau of Mines even refrained from referring to "lead," using primarily instead a term that is still familiar to those who remember the era of leaded gasoline, namely "ethyl." General Motors, itself, by contrast, seems to have been less concerned about refraining from making early pronouncements. In July 1924, for example, 5 months before even a preliminary report was issued, GM's director of research, Gram Edgar, predicted to an American Medical Association representative that study results would show "no danger of acquiring lead poisoning through even prolonged exposure to exhaust gases of cars using ethyl gas" (as reported in Rosner and Markowitz 1985:345). Emery Hayhurst, a noted industrial hygienist with Ohio's Department of Health—but also, quietly, a consultant to the Ethyl Corporation—told the Bureau of Mines that "Ethyl Gasoline . . . retains none of the poisonous characteristics" of its ingredients (as quoted in Rosner and Markowitz 1985:347).

Unfortunately, between October 26 and 30, 1924, over 80 percent of the workers producing the new "Gift of God" at Standard Oil's laboratories in Elizabeth, New Jersey, suffered either serious poisoning or death (Kitman 2000). Of 49 workers, 5 died, and 35 others "experienced severe palsies, tremors, hallucinations, and other serious neurological symptoms of organic lead poisoning" (Rosner and Markowitz 1985:345). Ironically, the Bureau of Mines released the findings from the GM-funded study on the day after the fifth and last Bayway victim died. *New York Times* headlines provided a summary: "No Peril to Public Seen in Ethyl Gas—Bureau of Mines Reports After Long

Experiences with Motor Exhausts—More Deaths Unlikely." The headline appears to have been correct in calling for no further deaths, at least at that plant, but the Workers' Health Bureau discovered five deaths at other plants, and the *Times* uncovered over 300 other cases of worker lead poisoning during the previous 2 years. Perhaps not surprisingly, the Bureau of Mines report did not have the desired effect. New York City's Board of Health banned leaded gas, and other bans followed.

In response, the industry sought other ways to calm public fears, requesting that the Surgeon General hold public hearings. Several public health experts spoke clearly about the hazards of lead, with Harvard's Dr. Edsal closing the hearings by noting "a hazard of considerable moment," and judging that the only way leaded gasoline could be said to be safe "would be after very careful and prolonged and devoted study as to how great the hazard is" (U.S. Public Health Service 1925:77, as cited by Environmental Research Foundation 1997a). For the most part, however, the hearings were dominated by industry experts, and again, those experts largely challenged risk concerns and instead emphasized the "risks" of *failing* to add lead to gasoline. Standard Oil's Frank Howard, for example, said "it would be an unheard-of blunder" to abandon leaded gasoline "merely because of our fears. Possibilities cannot be allowed to influence us to such an extent as that" (Needleman 2000). Other experts claimed (falsely) that lead was the only anti-knock agent available; or more grandly, that the continued march of progress demanded the adoption of new technologies, but they also invoked SCAMs, arguing that impacts on humans could not be predicted through use of animal studies, and that the dangers had not been "proven" (U.S. Public Health Service 1925; as cited in Environmental Research Foundation 1997b).

The hearings ended with the appointment of an expert committee, which reported several months later that more research was needed, particularly in regard to long-term exposure (Kitman 2000). Still, in accordance with later argumentation patterns, the report's authors summarized the findings in terms of "proof "—reporting that the committee had found "no good grounds" for prohibiting sales of leaded gasoline, based on then-available evidence—allowing the report to be characterized as a clean bill of health, and impressively, later to be used as the justification to *avoid* doing further studies.

In the absence of definitive scientific proof to the contrary, generations of American consumers would grow up learning to think of "Ethyl" gasoline as being "the good stuff." The Surgeon General would ultimately promote leaded gas, persuading local governments to lift their bans and convincing foreign governments that no further testing should be required. By 1936, 90 percent of the gasoline sold in the United States was leaded, and the Federal Trade Commission issued a restraining order, preventing Ethyl's competitors from

criticizing the fuel, describing it as being "entirely safe to the health of motorists and the public." The use of lead in gasoline would not be banned until 50 years later, in 1986, at which time the Lead Industries Association would continue to argue that there was not enough research to justify the ban (Needleman 2000). According to Kitman (2000), however, the mean blood–lead level of the American population has declined more than 75 percent since leaded gasoline was banned.

Discussion: Toward a More Even-Handed Approach?

Proponents of so-called "sound science"—those who argue for a single-minded focus on Type I errors while ignoring Type II errors—might well claim, with at least a germ of legitimacy, that even though 80 percent of the workers at the Bayway facility suffered severe symptoms of lead poisoning, that fact alone could not be taken as "proving," with a 95 percent level of confidence, that the substances being handled at that facility were hazardous. Clearly, however, the experience at the Bayway facility also could not be taken with a straight face as "proving" with any reasonable confidence that the chemical was *safe*. To the extent to which a SCAM is successful, however, the question of proving the *safety* of the chemical or technology is one that simply never comes up.

Three quarters of a century after the Bayway poisonings, Trumbo (2000a:199) noted that a study by the Iowa Department of Public Health produced "no conclusive resolution," in either direction, regarding citizen concerns over a nuclear reactor in Ames, Iowa—but that the study actually reported only that cancer increases in the area were "not statistically significant." Contrary to fears of overblown media reporting in the popular press, the Agency finding was dutifully reported in the local newspaper—not as an absence of findings, but under the headline of "Study Clears Neighborhood of Cancer Risk."

At least for those who remember their statistics classes, it may seem questionable to use any statistical significance tests in such cases. Significance tests indicate the ability to extrapolate from a sample to a population, after all, and a study of the actual prevalence of a disease in a community provides evidence from the entire affected population, not from a sample. To be fair, epidemiologists have developed complex arguments for expecting statistical significance, and more, before concluding that an observed effect is "real"; the problem is that applying *only* those arguments can lead to a notable lack of balance. In a review of 61 investigations of apparent cancer clusters by the National Institute for Occupational Safety and Health (NIOSH) (1983) from 1978–1984, for example, Schulte, Ehrenberg, and Singal (1987:53) carefully noted that, of the 16 cases having "a numerical excess of cases compared with expected numbers," eight "were not statistically significant at the 0.05 level." Even for the remaining eight cases where citizen concerns presumably *were* supported by statistically significant evidence, however, Schulte and his colleagues were careful not to

conclude that the suspected sources of contamination should be considered "guilty." Instead, as they noted for example in the case of the cancers among firefighters who responded to a blaze at a chemical dump (NIOSH 1983), the evidence was still not conclusive, because "there were no measurements of exposure or correlations with duration at the fire" (Schulte et al. 1987:53–4).

Roughly a dozen years later, another analysis of reported cancer clusters in the same journal found that, in the vast majority of the 1000 citizen requests each year to state health departments for investigations, it was simply not possible to provide statistically significant "proof," at a 95 percent level of confidence, that the citizen concerns were correct. Neither, unfortunately, was it possible to say that they were not (Trumbo 2000b). Instead, in the vast majority of cases, scientists are simply unable to reach unambiguous conclusions.

That, apparently, is a job for politicians and interest groups.

As seen in the cases considered in this article, even when virtually all relevant observers have ultimately concluded that the accumulated evidence *could* be taken as sufficient to issue a solid scientific conclusion—that cigarette smoking, for example, truly is dangerous, or that adding lead to gasoline also increased threats to human health—the battles continued to be dominated by SCAMs. Indeed, an examination of the historical record shows that, until the most dogged of industry proponents finally abandoned their positions, many of the key "scientific" representatives in the ongoing debates were still offering spirited arguments that the findings were not definitive, and that anyone who might seek to impose regulations in the absence of Scientific Certainty was being unscientific and irresponsible.

What is needed, in our view, is a more balanced approach to science. The observations in this article—unlike the earlier arguments by industry representatives—should be treated with appropriate levels of scientific conservatism and balance. Rather than being universal, SCAMs appear only to be sufficiently clear, consistent, and widespread enough that "further research is required." To repeat our earlier warnings, the cases in this article have been selected to represent a wide variety of policy issues and contexts, including some of the toughest cases we have been able to identify, but we are definitely not making the claim that SCAMs are universal. Instead, we offer three more specific conclusions: First, *scientific uncertainty* may be nearly universal in debates over public health and the environment. Second, requiring Scientific Certainty in a world of probabilistic uncertainty would be a shrewd Argumentation Method, even if it is one that flies in the face of truly "sound" approaches to science. Third, SCAMs have appeared in enough contexts in the past to deserve far more research attention in the future.

Based on findings to date, future research should consider a number of tentative but reasonably testable predictions. To return to one case in this article,

for example, many of the most outspoken participants in global warming debates were vigorously insisting, in mid-2005, when this article was originally accepted for publication, that "Nature," rather than any human activity, should be blamed as "the overwhelming factor" behind any actual warming (see, for example, Pegg 2003). In a prediction that appeared significantly more daring in 2005 than it will by the time the article is published in 2008, we predicted that, in light of growing scientific consensus to the contrary, even committed opponents of regulation would soon need to concede that global warming is real and anthropogenic—but that this concession would have few practical consequences. Instead, we predicted that the "scientific" debates would then move to the "next questions"—those where scientific findings are still in the "maybe" category, most likely involving uncertainties over how those climate changes will affect people in the future.

Similar patterns appear likely to emerge in other disputes that can be characterized as lacking "true" Scientific Certainty. As another example of a testable prediction that may still appear relatively daring in 2008, the perspective of this article would predict that future findings on global warming will show a nonrandom trend. Given that the use of SCAMs in debates over global warming have led to what Freudenburg and Youn (1999) first called the "asymmetry of scientific challenge"—with evidence and conclusions pointing toward the presence of global warming being subjected to withering and well-funded criticisms, while evidence and conclusions pointing toward the absence of global warming were more likely to be the focus of triumphant press releases—our perspective would predict that independent scientific findings in the years ahead will be more likely to conclude that actual levels of global warming are proving to be "higher than expected," rather than being "lower than expected."

It is also worth considering potential policy implications that would be most prudent for the near future, as the new research is being carried out. Roughly since 1990, scientific journals have begun to carry articles noting inherent conflicts between Type I and Type II errors (see, for example, Costanza and Cornwell 1992; Peterman and M'Gonigle 1992; Underwood 1993). Some articles have begun to argue that the appropriate response to the dilemma is simply to "reverse the burden of proof" (Dayton 1998), particularly in areas of resource management the authors identify as problematic, such as fisheries management (see also Levidow 2000). Still, except in cases where the human health and/or environmental implications of a Type II error are especially grave, such an approach could well replace a bias toward under-regulation with a bias in favor of over-regulation. More balanced approaches, however, are also possible.

In certain contexts, for example, EPA has moved toward a more even-handed approach toward balancing Type I/II errors, with its "Data Quality Objectives Process" (U.S. Environmental Protection Agency 2000). In that

process, however, the agency has carefully avoided the kinds of political and/or distributional implications that are entangled in most regulatory decisions; instead, it has focused on indoor lead dust, in buildings where the residents are the ones who might be exposed *both* to any health risks and to any clean-up expenses (US EPA 2000:C1–C4). In addition, EPA had already selected the precise level of contamination it saw as presenting a requirement to act. In that case, in other words, the Agency was not trying to decide whether or not a material was risky, but simply using sampling to decide if contamination levels were above or below the cutoff line.

In the more typical case of a community exposed to a complex and to some extent unknowable mix of chemicals, where the only "reliable" information comes from epidemiological data of limited power, the decision-making considerations tend to be more complex. Even for those cases, however, at least three options would be more even-handed.

First and most obviously, in cases where it is feasible to gather additional data without undue delay, there are good reasons to gather more data. Such cases, however, may be less common than might be expected: Even in the "popular epidemiology" case considered by Brown (1987, 1997), the gathering of additional data involved well over 10 years of effort, and in cases involving health risks to children or the potential extinction of a species, waiting another 10 years for definitive data might produce only the definitive knowledge that the children had died or that the species had vanished from the earth.

Second, Peterman and M'Gonigle (1992) have proposed a form of triage: On one hand, in cases where evidence of harm is statistically significant, the agency should act to reduce the risk, and on the other hand, in cases where any trends are nonsignificant statistically *and* the relevant studies are appropriately high in statistical power, the agency should avoid imposing any additional regulations. Peterman and M'Gonigle (1992:232) propose that the Precautionary Principle be followed for those remaining cases in which the "no harm" or null hypothesis cannot be rejected but where the relevant studies have low statistical power: "In these latter cases, especially where the confidence interval on the effect size is large, regulatory action to reduce emissions should be recommended as a precautionary measure in case an important effect exists."

Third and finally, rather than arbitrarily focusing exclusively on either Type I or Type II error, the simplest approach might be the most even-handed one: In cases where available data might provide less than a 95 percent level of confidence that a chemical is "risky"—as for example only a 78 percent level of confidence—the next question would be what level of confidence could be provided on Type II errors. The ultimate regulatory decision would be determined by *the balance of Type I and Type II errors*. If available data were only sufficient for the possibility of a Type I error to be rejected with a 78 percent

level of confidence, for example, but the possibility of a Type II error could only be rejected with an even lower level of confidence, such as 43 percent, then the most balanced approach would be to *protect against the error having the lower level of confidence*, or in this case to regulate the risk.

Some colleagues who have considered earlier versions of this article have argued that this even-handed approach to scientific uncertainty would not be sufficiently protective of health, safety, and the environment, as the two kinds of risk are not equally important. If we worry too much about Type II risk and regulate too vigorously, for example, we may deprive ourselves of valuable new technologies or profit opportunities—but if we worry too much about Type I risk and regulate too weakly, we may deprive innocent bystanders of health or even life. We acknowledge this point, as well as acknowledging that other approaches are possible, such as attempting to calculate the "costs" of making a Type II versus a Type I error (Kendall 2005).

What is vital in our view, however, is not to reach a definitive conclusion at this time, but instead to reach agreement that differing approaches to the weighing of risks need to become the subject of open debate—in contrast to a process in which one side is made to bear nearly all the costs, physical as well as financial, of demonstrating Scientific Certainty in a world of inherent uncertainties. There may be no one "best" enforcement level—one that can simultaneously meet all of the objectives an agency is expected to satisfy. Under the circumstances, it would be understandable for industries to put pressure on regulators to "demonstrate," despite the irreducible or unavoidable ambiguity, that a specific "regulatory burden" is *unambiguously* justified (Freudenburg and Pastor 1992). What is not so understandable, however, is for that same approach to be seen as reasonable for protecting public health and safety, or to be seen as acceptable for government agencies that have the statutory responsibility to protect public health and the environment.

Our position, in short, is that a single-minded obsession with *either* Type I or Type II errors reflects a lack of balance that is altogether out of place in any decision-making process that actually does value "sound science." Sound science requires balance, as well as requiring careful thinking about just what is at risk. In environmental and technological controversies, a Type II error is not merely an abstract possibility, but a risk that innocent people will get sick or die. In light of this reality, it is difficult to believe that anyone who believes in truly balanced or "sound science"—or for that matter, any well-informed person of good will—could seriously contend that the "proper" balance involves a decision to focus exclusively on Type I errors while deciding to ignore Type II errors completely. That, however, is nevertheless the net effect of successful efforts to argue for full "scientific certainty" before a regulation can be said to be "justified"—and that, in short, is a SCAM.

ENDNOTE

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