Psychology and Public Policy

Tool or Toolmaker?

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ABSTRACT: People's behavior shapes and is shaped by how environmental issues are managed. As a result, there may be a role for psychologists in various environmental issues. This role offers opportunities to increase the influence and sophistication of our science. However, it also poses risks for both the science and the public. These potentials and pitfalls are discussed here in the context of examples drawn from setting policy for the levels of risk associated with environmentally hazardous technologies.

Psychologists are needed by public policymakers whenever the outcomes of their policies either affect or depend on human behavior (Noll, 1985; Stokols & Altman, 1987). For example, in the context of environmental policy, psychological expertise is needed to (a) determine what people value in outdoors experiences (e.g., as an aid to designing parks or evaluating wilderness areas, Daniel & Vining, 1983); (b) assess the stressfulness of living near hazardous waste facilities, as an input to measuring their environmental impact (Baum & Singer, 1981); and (c) see how noise affects school performance, as a guide to siting freeways or retrofitting sound buffers (Cohen, Evans, Krantz, & Stokols, 1980).

By contrast, psychologists often *seem* needed by policymakers primarily when some of the public's behavior threatens their policies. For example, psychologists were asked (or allowed) to study home energy conservation when a "wasteful" public appeared to be an obstacle to national energy independence (Aronson & O'Leary, 1983; McClelland & Cook, 1980). They were encouraged to study seat belt usage when nonusage increased pressure for mandatory airbags or unpopular seat belt laws (Geller, Paterson, & Talbott, 1982; Robertson, 1983). Economists functioning as psychologists have been paid to ask laypeople what they would pay for environmental improvements in situations in which industries felt they had to pay too much to achieve those changes (Smith & Desvousges, 1986).

On the positive side, any invitation to psychologists reflects a sensitivity to human wants and needs. It offers us, as psychologists, an opportunity to "show our stuff," increasing policymakers' understanding of what psychologists can do. The evidence that we produce ought to be better than the undisciplined speculations that would come in its stead. The funding to create that evidence should enhance the scientific base and public prestige of our profession, attracting better students (and funding) to it.

On the other hand, the terms of these invitations to study the public may be bad for both the public and the scientist. The invitation can harm the public whenever the presenting symptoms (described by policymakers) cast the public in a troublesome (or troublemaker's) role. Indeed, simply by accepting such descriptions, psychologists help undermine the public's political credibility. If they use their expertise to remedy such untoward behavior, then psychologists may shift the political balance against the public's best interest. Even by claiming to explain the public's behavior, psychologists can contribute to a sort of disenfranchisement—by reducing the perceived need to let the public speak for itself.

The terms of these invitations can be bad for science whenever they mislead us regarding the nature of the "problem." We may then be slow to understand what is actually happening, either in the field or in our own data. That means wasting our time and society's resources as well as missing the opportunity to be stretched by the confrontation with a reality outside our labs.

It would, of course, be naive to expect psychologists to be invited to *set* public policy regarding the environment or any other significant issue. Policy-making involves the allocation of resources, a right that is jealously guarded by elected and appointed officials (i.e., politicians and bureaucrats). In one way or another, they justify their actions by claiming to know what the public wants and needs. If they invite us, it is not to share their power, but to fortify it, by fine-tuning programs, anticipating and overcoming resistance, or guiding and legitimizing initiatives.

To some extent, these will be acceptable roles for psychologists. We did, after all, choose a profession rather than the explicit political life. On the other hand, when taking part in public policy issues, we often have greater aspirations than merely being hand servants of good government and efficient markets. We are attracted to issues because we care about their outcomes. We also know that those individuals closest to the locus of decision making have the greatest opportunity to influence its outcome. Scientists who get close can exert influence directly by what they say to policymakers and the press. They may do so indirectly by how they design policy-relevant studies. For example, Executive Order 12291 requires all significant federal actions to be justified in terms of cost-benefit analyses (Bentkover, Covello, & Mumpower, 1985). However, the technical definitions of *cost* and *benefit* used in these analyses do not follow logically from some basic science, but express political values (Campen, 1986;

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Fischhoff & Cox, 1985). By their choice of definition, the scientists who conduct such studies are, in effect, setting policy. Similar political power accompanies defining the terms of other quasi-scientific research, such as evaluation studies, public opinion polls, or risk analyses (Fischhoff, Watson, & Hope, 1984; Turner & Martin, 1985).

Handling direct political influence responsibly is relatively straightforward. We need to recognize that we have entered the political arena through a back door, realize the limits to our expertise and mandate, and acknowledge when we speak from our hearts rather than represent our evidence. When environmental issues seem too important to be left to environmental policymakers, any path to influence may seem legitimate. However, intellectual hygiene dictates that we recognize where our political agendas abut our research activities—even if we keep that insight to ourselves (Fischhoff, Pidgeon, & Fiske, 1983).

Handling indirect political influence is more difficult. It means examining the political philosophy underlying routine professional work. For example, what concept of *justice* guides the construction of stimuli in studies of perceived equity (Furby, 1986)? What outcomes do we decide to measure when evaluating clinical treatments? How do we describe women who have experienced sexual assaults and, indeed, the assaults themselves (Hindelang, Gottfredson, & Garofalo, 1978)?

In any life, professional or personal, it is hard, but potentially rewarding, to reflect on otherwise unquestioned assumptions. When our assumptions affect other people's fortunes, reflection becomes obligatory. Figuring out how policymakers might use us to further their own agendas can provide particular impetus, and perhaps some cues, to explore who we are. For those who desire the political life, one obvious path is to go where the action is. An alternative is to seek the politics wherever one is already, seeing how one's own profession shapes and is shaped by the world.

To these ends, I will describe several episodes involving psychology and environmental policy, asking how well we have been able to create tools to help the public define and pursue its own interests, rather than becoming tools for manipulating the public to others' ends. I draw primarily from my own experiences. Not only is that material most readily accessible, but I can be most candid about the mistakes that I have made.

I would like to thank Lita Furby for her continuing help in understanding these issues. Much of the research cited here was conducted in collaboration with other investigators, particularly Paul Slovic and Sarah Lichtenstein. Their contributions are gratefully acknowledged. They are not, however, to be held responsible for this personal interpretation of the interface between psychology and public policy.

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Perceived Risk

A critical question in many environmental controversies is "How much does the public know?" If the public understands environmental risks well, then it may be entitled to a more active role in their management. A knowledgeable public should, for example, be taken more seriously when it objects to the siting of an incinerator, the opening of a wetlands to "development," or the denial of information about what has been stored at a waste disposal dump. Not surprisingly, risk management debates are rife with claims and counterclaims about the public's scientific literacy and competence. These claims then are used to buttress proposals for right-to-know laws, consumer protection agencies, referendums, products liability reforms, warning labels, and the like (National Research Council, 1989).

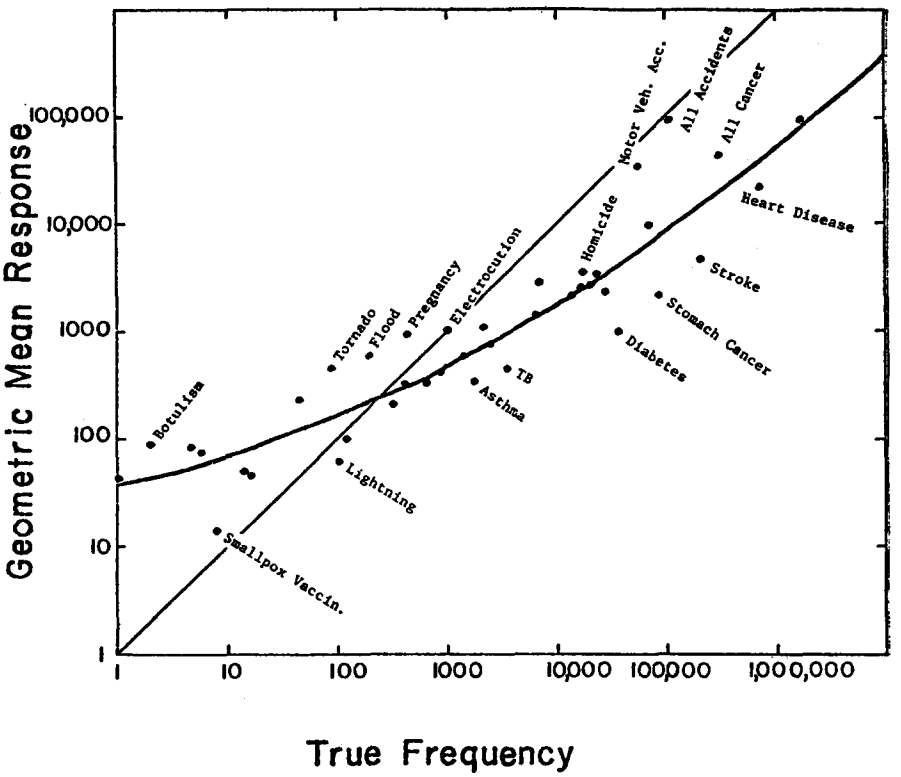
Figure 1 shows one attempt to supplement anecdotal speculation about the public with systematic evidence. It contrasts the estimates of a group of educated laypeople with available public health statistics regarding the annual number of deaths from various causes. It was interpreted as showing two kinds of bias. The first is a flattening of the best-fit curve, relative to the identity line (representing completely accurate judgments). The second is a tendency to over- and underestimate certain death rates, relative to the fitted curve. This *secondary bias* was found to be predicted well by the relative *availability* of deaths from these causes, as measured in several different ways (Combs & Slovic, 1979; Lichtenstein, Slovic, Fischhoff, Layman, & Combs, 1978; Tversky & Kahneman, 1973).

Figure 1 has had a remarkable public life, being cited extensively in policymakers' discussions of risk management (e.g., Starr & Whipple, 1980; Upton, 1982). Typically, it has been described as proving the public's ignorance (or even "irrationality") regarding risk issues with the attendant political ramifications. I have heard it described as proving the public's hopeless confusion about risks (e.g., nuclear power) that were not even in the study. Not only were these claims unwarranted by these results, but they went far beyond what could be shown in any single series of studies.

One response to such apparent distortions is to collect the missing data. Thus, one subsequent study found that similar subjects were quite well informed about the annual death rate (to that date) from nuclear power (Slovic, Fischhoff, & Lichtenstein, 1979). A second follow-up study found that making such numerical judgments is sufficiently unusual that whether people seem to overestimate or underestimate these rates depends on methodological details of how the question is asked (Fischhoff & MacGregor, 1983). A third study found that when people think about the "risks" of a technology, they factor in other features, such as its potential for catastrophic accidents as well as its routine death toll (Slovic, Fischhoff, & Lichtenstein, 1980). As a result, Figure 1 shows but a part of the lay public's risk perceptions.

A rather different response is to ask how policymakers reached their misinterpretations. One speculation

Figure 1
Laypersons Direct Estimates of the Frequency of Various Risks



Note. The straight line represents accurate estimation. The curved line fits the subjects mean responses and shows a primary bias of overestimation of infrequent events and underestimation of frequent events. Deviations from the curved line were quite consistent for different groups of subjects and represent secondary biases. These secondary biases are emphasized in the text. Reprinted from "Judged Frequency of Lethal Events" by S. Lichtenstein, P. Slovic, B. Fischhoff, M. Layman, and B. Combs, 1978, *Journal of Experimental Psychology: Human Learning and Memory, 4,* p. 566. Copyright 1978 by the American Psychological Association. Reprinted by permission.

is that they misunderstand the nature of social science data. Social scientists do not seem needed at all until things get out of hand (e.g., public protest over nuclear power). At that point, we are expected to explain (and perhaps alter) the public's views quickly. One study ought to produce confident conclusions. Alternative explanations need not be sought or tested.

A second speculation is that readers simply saw what they wanted to see in our results. The proponents of beleaguered technologies would like to believe that the central issue in the debate is the magnitude of risks, regarding which they are (or employ) the ranking experts. They would like to believe that *risk* refers to average-year fatalities, a subject on which they have the firmest data, rather than including catastrophic potential, which is hard to estimate. They would like to believe that they can deal with the public at arms' length and inexpensively, through a few social science studies, rather than engaging the public directly.

Acceptable Risk I

Some light on these speculations might be seen from the responses to a series of studies attempting to sketch a richer picture of the public's attitude toward risky technologies. Its point of departure was a study by Starr (1969) claiming to show, on the basis of historical data, that technologies with greater risks also have greater benefits. In addition, at any given level of risk, technologies whose risks are borne involuntarily have considerably greater benefit. Starr argued that our society works so well that these historical patterns show the risk–benefit tradeoffs that it wants. If so, then policymakers could determine the acceptability of a proposed technology by computing its expected risks and benefits and seeing whether those fit the patterns.

One obvious question about this upbeat interpretation is whether people's perceptions of these risks and benefits correspond to the values that Starr computed. After all, society works on the basis of what it sees, rather than what some expert computes.[1] We asked some publicly involved citizens to judge the current risks and ben-

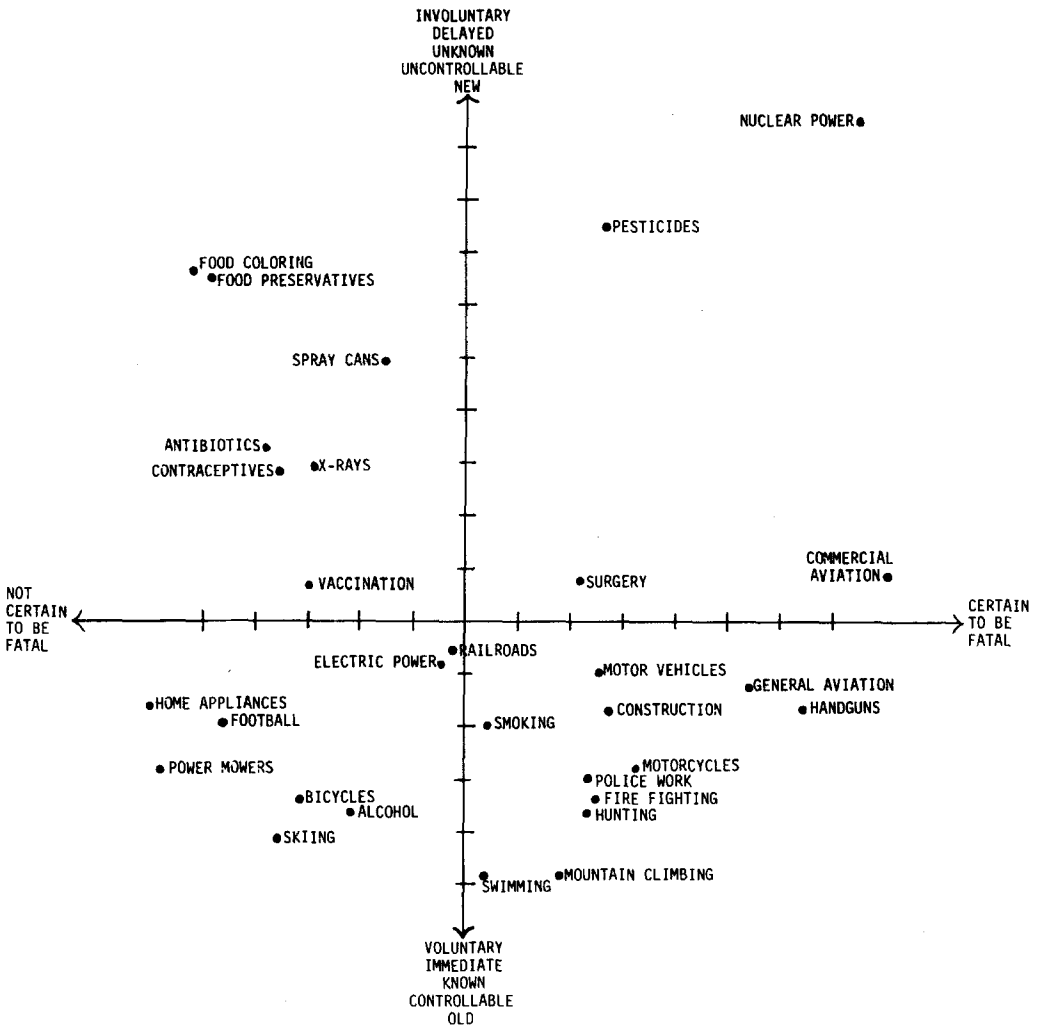
[1] Indeed, the confidence in public risk perceptions implicit in this study seems in striking contrast to the lack of confidence usually expressed by the technical community. Questions have also been raised about the accuracy of Starr's calculations (Otway & Cohen, 1975).

efits of various technologies (Fischhoff, Slovic, Lichtenstein, Read, & Combs, 1978). Their responses showed no correlation between perceived risks and benefits, indicating that they did not see societal institutions as successfully producing acceptable tradeoffs. This result fits the persistent result of opinion polls, showing the desire for greater environmental protection (e.g., Freudenberg & Rosa, 1984).

When risk levels were adjusted to what our subjects thought they *should* be, a willingness to make tradeoffs emerged. In addition, more voluntary risks were allowed higher risk levels. However, voluntariness was not the only qualitative aspect of risk that seemed to justify a double standard. For example, more risk was also tolerable with technologies that were well understood by science and that took their toll one by one, rather than in catastrophic incidents. As a basis for social policy, each of these features expresses quite a different principle. Voluntariness sounds like a civil rights issue; scientific understanding sounds like a question of prudence; how deaths are "packaged" sounds like a macabre question of taste.

These features tend to be highly correlated with one another across technologies, so that it is hard to tell which drives a correlation with the acceptability of risks (Hohenemser, Kates, & Slovic, 1983; Slovic et al., 1980). In order to show these interrelationships, we published Figure 2. It shows a factor analysis of ratings for various technologies on nine qualitative features of risk. This figure and variants of it (e.g., Slovic et al., 1979, 1980, 1984; Vlek & Stallen, 1981) have been cited often. On the other hand, the critique of Starr's (1969) *revealed preference* analysis that motivated it has virtually escaped mention.

Figure 2
Location of Risk Items Within the Two-Factor Space



Note. Reprinted from "Facts and Fears: Understanding Perceived Risk" by P. Slovic, B. Fischhoff, & S. Lichtenstein. In *Societal Risk Assessment: How Safe Is Safe Enough?* R. Schwing and W. A. Albers, Jr. (Eds.), 1979, New York: Plenum Press. Copyright 1979 by Plenum Press. Reprinted by permission.

That critique produced a relatively complex conclusion as well as one that was quite unwelcome to advocates of Starr's "whatever is is right" philosophy. By contrast, the figure seems to have functioned as a projective test. In particular, people favoring nuclear power read it as showing the unique (and irrational) "psychological" status afforded their favorite energy technology (Freudenburg & Rosa, 1984). The fact that the judgments of technical experts yield a similar factor structure has done little to dilute Figure 2's contribution to confirming disrespect for the public.

Acceptable Risk II

Still hopeful, my colleagues and I accepted a contract from the U.S. Nuclear Regulatory Commission (NRC) to analyze possible procedures for setting acceptable risk levels. The resulting report (Fischhoff, Lichtenstein, Slovic, Derby, & Keeney, 1981) was not exactly what the NRC had hoped for. It argued, first, that the concept of acceptable risk was ill conceived: People accept options, not risks. One feature of those options is their level of risk. Even if an option is accepted, it might be rejected in favor of another option offering less risk at reasonable additional cost or more risk along with a sizable cost saving. Assuming that people view risk levels in isolation leads to seeing them as responding inconsistently, sometimes accepting and sometimes rejecting the same risks. (Thus, the more reasonable they are, the less reasonable they seem.)

Our report went on to argue that no policy-making procedure meets all the NRC's criteria. None can adequately accommodate all the factors that ought to influence acceptable-risk decisions. None makes wholly realistic assumptions regarding human behavior. None is "objective," in the sense of offering wholly technical solutions, devoid of political values.

Soon afterward, the NRC proposed a policy so incomplete, unrealistic, and subjective that we had not thought to review it explicitly. It set forth a number expressing the acceptable level of risk for nuclear power (U.S. Nuclear Regulatory Commission, 1982). That number (one in one million) seemed to go down well with the industry and represented a first explicit expression of a safety philosophy by the NRC (which had previously embedded its philosophy in decisions about specific technical issues, such as how thick to make pipes). Our (attempted) contribution to this effort was an analysis of when it made sense to rely on such rigid numbers and how they needed to be specified in order to create meaningful, predictable safety standards (Fischhoff, 1983, 1984). It had little discernible effect.

More surprising than having little impact was being asked to try. What are psychologists doing analyzing the logic and ideology of policy-making methods? In part, we were exploiting a relative vacuum. At any time, there are many more people implementing methods than worrying about their fundamentals. We were invited just to explain the public's behavior, but saw the opportunity to fill a larger role. In part, we were exploiting psychology's unique origins. As a discipline, psychology has roots in philosophy, biology, and sociology (or its predecessors). Each of these pieces is needed to fulfill this role. Perhaps it is also easier (or at least more appealing) to probe the foundations of other professions.

Perceived Benefit

Cost–benefit analysis is the name of the game in evaluating many environmental decisions. As a result, any consequence that cannot be evaluated in monetary terms will tend to be ignored. The usual strategy for determining the monetary value of consequences is to look for their evaluation in some marketplace (Bentkover et al., 1985; Smith & Desvousges, 1986). Yet, many significant environmental consequences are not traded anywhere. In order to gain a hearing for consequences such as degradation of landscapes, reduction in visibility, and extinction of noncommercial species, resource economists (e.g., Cummings, Brookshire, & Schulze, 1986; Mitchell & Carson, 1989) have developed a family of survey techniques called *contingent valuation* methods. They ask people to estimate what they would pay, were there a marketplace offering that consequence.

These are very ambitious questions, compared with the rating scales typically used by psychologists. If these questions can be answered meaningfully, then laypeople's enfranchisement has been extended. Their responses would be plugged directly into cost–benefit analyses. On the other hand, if people cannot answer these complex, novel questions, then their own responses will misrepresent their values. Insofar as mean responses in a study will be multiplied by the number of people in a county or country, small errors in responses can produce large errors in evaluating an environmental intervention.

Several agencies sponsoring such research have invited psychologists to look over the shoulders of the economists performing it. When asked, we expected that many aspects of the procedure would not ring true, in terms of psychologists' norms (and tastes) regarding how studies should be conducted and analyzed. We were not disappointed (Fischhoff & Furby, 1986; Gregory & Furby, 1987; Kahneman, 1986).

We were surprised, however, by the incompleteness of the economic analysis. Taking the *contingent market* metaphor seriously means presenting subjects with enough detail to clarify what market is intended. However, the investigators neither agreed about the nature of those details nor realized their own lack of agreement. Features that were stressed in one study were unspecified in another. Features essential to making the task meaningful were sometimes noted, other times ignored. Might economists lack a clear idea of what a market is? Might psychologists be the ones to help them?

In order to review and design contingent market studies systematically, we developed a framework for specifying markets—or, what we more generally called *transactions,* situations in which people might trade a "payment" for a "good" (Fischhoff & Furby, 1988). That payment might be money, but also time or effort, so as

to avoid reifying monetary sacrifices. Our framework showed that far more detail is needed to describe a novel market than is presented in any contingent market studies. It may also be far more detail than could even be absorbed by most subjects under the conditions of most interviews. If such a task could be understood, subjects would still face the task of determining their own response to it.[2]

The theoretical challenge at the moment is to understand how people decide what they want when faced by explicit, but unfamiliar alternatives. The practical challenge is to make these tasks meaningful so that people do not misunderstand the question and their answer to it. The policy challenge is to know which market to specify as best representing the issues actually being considered. These challenges are all a part of a developing science of subjective evaluation requiring the skills of psychology and other disciplines (e.g., Fischhoff, Slovic, & Lichtenstein, 1980; Hechter, Cooper, & Nadel, in press; Hogarth, 1982; Mitchell & Carson, 1989; Thaler, 1980; Turner & Martin, 1985; Tversky & Kahneman, 1981).

Conclusion

The details of these experiences reflect the particular niche occupied by the people working in this area. The work might not ever have been funded had the environmental movement not been pressuring industry in the public name. The work might have emerged quite differently had it not been performed largely by investigators living on soft money. That dependence makes one more interested in people with problems severe enough that they might want psychologists' help. It forces one to look for ways to address basic research questions in the context of applied problems. Nonetheless, there seem to be some general lessons for settings in which psychologists confront environmental policy: (a) Expect to make slow progress in understanding the underpinnings of one's own field; (b) expect to fulfill nonpsychological roles; (c) expect one's empirical results to be distorted, both deliberately and inadvertently; (d) expect "amateurs" to try to usurp the need for psychological expertise, replacing our research with their self-serving speculations; (e) expect to stick with a problem long after any financial support has been exhausted; (f) expect conflicts of conscience (and charges of bias) when balancing science and politics; (g) expect to be misdirected by the presenting symptoms described by clients; and (h) expect the temptation to overshoot one's competence.

The final analysis for such involvements might include three questions: Is it good for society? Is it good for psychology? Is it good for oneself? Answering each of these questions requires an appraisal of what has happened and what matters. Personally, I am not unhappy with the overall results of these gambles or of others taken in the same vein (e.g., Fischhoff & Furby, 1983; Furby & Fischhoff, in press; Lanir, Fischhoff, & Johnson, 1988). However, they were quite different gambles than trying to publish in more conventionally respectable journals. And they might have turned out quite differently.

[2] These details include substantive features, such as just what is the consequence (e.g., is visibility measured by haze intensity, visual range, plume, or light extinction?), why is it valued (e.g., what is being seen?), and what is the source of the change. They also include formal features, such as how big a change is, when it will begin, how long it will last, and how likely it is actually to be received if the transaction is accepted. Other details include a precise description of the payment and the social context within which the transaction would be conducted (e.g., who else is doing it, what precedents are being set, and who guarantees the transaction). Our ability to comprehend everyday transactions must owe much to the number of details that can go without saying, allowing us to focus on what well-known things are worth to us in terms of well-known payments.

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