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Elitism vs. Checks and Balances in Communicating Scientific Information to the Public

Arthur Kantrowitz*

Introduction

Science

New technology played a dramatic role in U.S. contributions to allied success in World War II. In many cases, e.g., atomic weapons, penicillin, radar and DDT, these technologies were based on science not applied before the war. Scientists led many of the triumphant efforts. After the war, the prestige of science was great enough that national expenditures for research grew from earlier charity levels to budgets that are now noticeable in the federal deficit. This investment led to a golden age of science-based technology that has enriched the world. It has also permanently altered science.

The money was used to exponentially increase the numbers of educated scientists. It was also used to build an industry that makes scientific apparatus so powerful that, without the latest and most expensive versions, it is difficult to advance. Research careers are critically dependent on success in raising funds to support graduate students and buy equipment.

The power of science to influence policy has not grown as fast as its need for funds. As funding has tightened, institutions of science have become fixated on the federal research budget. Frank Press called raising money for basic research "the most important activity I can undertake as president of the National Academy of Sciences."[1] Thus,

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1 Frank Press, Letter to Members, April 1984, at 9.

institutions of science expend less of their precious political influence on tasks that do not serve the funding imperative and fail to take bold stands against the pseudoscience of our times.

And Society

Statements of preference for a policy express inner values and perceived facts. People can be persuaded to change preferences by altering their values or by changing their perception of pertinent facts. In modern society, pertinent facts have become increasingly inaccessible to the "naked eye." As scientific facts become more important in public policy, democracy has become more dependent on scientists for factual information. This vulnerability has made publicizing tortured versions of facts a favorite tactic for influencing policy.

The situation was eloquently summed up by Margaret Mead in 1976:[2]

> We need a new institution. There isn't any doubt about that. The institutions we have are totally unsatisfactory. In many cases they are not only unsatisfactory, they involve a prostitution of the decision making process.

To oversimplify somewhat, let us characterize two modes of finding facts as the "elitist" approach and the "checks and balances" approach. We have always lived with the elitist approach. There can be no doubt that, if the spokesmen for science were viewed as sufficiently wise and saintly, their findings would be clearly seen as the best obtainable.

However, when anyone can gain by attacking or ignoring the wisdom or the saintliness of the spokesmen for science, we must expect alternate factual statements to become surrogates for value differences in political controversy. Alternate factual statements are calculated to appeal to people who cannot be persuaded to share the values of their proponents. When the public is presented with such statements, the "Which scientist do you believe?" dilemma results in the resolution of scientific controversies *external* to the scientific community.

This paper describes a proposed methodology for communicating what science knows and especially what science doesn't know when scientific facts are needed for public policy decision making, one based

2 PROCEEDINGS OF THE COLLOQUIUM ON THE SCIENCE COURT 25 (1977).

on a norm for communicating with the public intended to be enforced by the scientific community. It also describes efforts to advance the positions that: (1) the scientific community has a duty to minimize the political efficacy of external controversy concerning scientific facts and (2) by extending to public statements. the checks and balances traditionally used to police communication within the community, resolution *internal* to that community can be more persuasive and effective in discouraging external absurdities.

The Elitist Approach

The elitist approach was perhaps best described by Philip Handler, past president of the National Academy of Sciences (NAS), who was quoted as saying,[3] "if what we have to say is credible, the credibility rests on the distinction and prestige of the members."

Handler led a valiant effort by the NAS to implement this approach. In addressing the NAS Bicentennial Symposium, he reported the results as follows:[4]

> But establishing truth with respect to technical controversy relevant to matters of public policy, and to do so in full public view, has proved to be a surprisingly difficult challenge to the scientific community. To our simple code must be added one more canon: when describing technological risks to the non-scientific public, the scientist must be as honest, objective, and dispassionate as he knows he must be in the more conventional, time-honored self-policing scientific endeavor. This additional canon has not always been observed. Witness the chaos that has come with challenges to the use of nuclear power in several countries. Witness, in this country, the cacophony of charge and counter-charge concerning the safety of diverse food additives, pesticides and drugs. We have learned that the scientist-advocate, on either side of such a debate, is likely to be more advocate than scientist and this has unfavorably altered the public view of both the nature of the scientific

3 Phillip Handler, *Interview*, EPRI Journal, April 1980, at 33.

4 Phillip Handler, *Science and Hope* in SCIENCE: A RESOURCE FOR HUMANKIND, PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES BICENTENNIAL SYMPOSIUM, 12 (1976).

> endeavor and the personal attributes of scientists. In turn, that has given yet a greater sense of urgency to the public demand for assurance that the risks attendant upon the uses of technology be appraised and minimized. And what a huge task that is!

Three examples of the current retreat toward a "risk free" society illustrate Handler's point.

1. Abandoning expansion of nuclear energy in 1978 (before Three Mile Island), the U.S. has now fallen behind most of the industrial world in its utilization.

2. The social beneficence of innovation has been so discredited that our courts have, since the sixties, adopted the doctrine of "strict liability."[5] It puts the whole burden of potential harm on innovators even when they are innocent of any negligence. Reflecting today's search for a risk free society, strict liability implements the assessment that innovation benefits only innovators, so they must bear the whole burden of risks, putting U.S. innovators at a competitive disadvantage.

3. A recent comparison of U.S. practices with those of other industrial countries in the approval of new drugs for marketing illustrates the damage consequent on risk avoidance.[6] The Food and Drug Administration approved 55 new chemical entities for marketing in 1987, 1988 and 1989. Eighty percent of these were available in foreign markets, a mean time of 6.5 years before U.S. approval.

The weaknesses of an elitist approach have led to checks and balances *external* to the scientific community. Thus, Congress has felt a need for "independent" fact finding. It set up the Office of Technological Assessment independent of the executive branch but without safeguards to protect it from political control. Similarly, any political group (and its opposition) in need of "scientific" support to influence public opinion has little difficulty in recruiting lists of names that seem authoritative. In the resulting media contest between competing authorities, it is not possible to tell whether science or politics is speaking. We then lose both the power of science and the credibility of democratic process.

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5 PETER W. HUBER, LIABILITY, ch. 3 (1988).

6 Kaitin, DiCerbo & Lasagna, *The New Drug Approvals of 1987, 1988, and 1989*, 31 J. CLIN. PHARMACOL 116 (1991).

The noise introduced by the activities of "Madison Avenue" and of "Hollywood" has degraded communication enough to seriously threaten our ability to undertake new adventures in science-based technology. When your auto windshield is splattered with mud, obscuring the road ahead, you stop.

Distinguished economists such as Paul Krugman have great difficulty accounting for the decline in U.S. productivity growth in the last two decades. He reports that "Even among the experts,... stagnant American productivity is not a fashionable topic. ... Why did it happen? And what can we do about it? The answer to both is the same: We don't know."[7]

These are the same decades in which the U.S. set out to minimize risks due to new science-based technology. This period has seen a decline in the traditional expectation that Americans have always had that their children would lead better lives.

I will not pretend to understand all causes of this decline. However, I believe that it is incumbent on the scientific community to reexamine its procedures for communicating what it knows, and especially what it does not know, to the public when controversial scientific facts have a bearing on the making of public policy. This is precisely the area that Handler characterized as "a huge task." It is necessary that we face this task before America can restore its confidence in the adventure of science-based technology. It is of course not sufficient because no signal the community can send can overcome the noise that can be introduced by "Madison Avenue" and "Hollywood." After we have faced up to our duty in reducing what Handler called "cacophony," we can more confidently call on the media to do their part.

The Internal Checks and Balances Approach

The scientific community has its traditions that provide a kind of due process in what Handler called "the more conventional, time-honored, self-policing, scientific endeavor." What is needed is to extend these procedures to provide information basic to policy making in a way that the policy maker, the scientist and the average citizen will find more

7 PAUL R. KRUGMAN, THE AGE OF DIMINISHED EXPECTATIONS, 13 (1990).

credible than alternate sources. To achieve this end, I propose that the "one more canon" should be:

> *Any scientist who addresses the public or lay officials on scientific facts bearing on public policy matters should stand ready to publicly answer questions not only from the public, but from expert adversaries in the scientific community.*

This norm is to be enforced by the scientific community.

The proposal to create an Institution for Scientific Judgment was an attempt to institutionalize this norm.[8] It contrasts with elitist approaches in the same way that a "government of laws" contrasts with a "government of men."

Today, scientists can sign petitions or take positions in committee votes consistent with their politics without having to concern themselves with being challenged on their knowledge of the science or technology basic to their positions. They can advance policies they favor that may involve science far from their expertise. This is a "government of men."

The prospect of embarrassment, by being under obligation to publicly answer questions from expert adversaries, would reduce the noise. Perhaps then we would be able to hear the voices of those who know most about the science. Perhaps then we would know "which scientist to believe."

It need hardly be stated that the success of any such institution would depend on the wisdom and the saintliness of its members. However, it will gain credibility because the public will recognize that it embodies some of the elements that our society has developed for dealing with controversy. It would have some of the attributes of a "government of laws."

Efforts to Institutionalize the Norm

During early efforts, the name "Science Court" (SC) was attached to this concept as the media's recognition of the fact that there are some

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[8] Arthur Kantrowitz, *Proposal for an Institution for Scientific Judgment*, 156 SCIENCE 763 (1967). *See also*, Task Force of the Presidential Advisory Group on Anticipated Advances in Science and Technology, *The Science Court Experiment: An Interim Report*, reprinted *infra*, at 179.

parallels between the norms of scientific controversy and legal due process. The name has stuck, and we will continue to use it. This makes it important to point out that there are many salient differences, e.g.:

1. Courts concern themselves with both finding facts and pronouncing verdicts, i.e., expressing society's values. The SC would deal with facts only and would not recommend actions or policies.

2. All science is tentative. Therefore any output from the SC would be offered as the state of knowledge at a time when that was needed for action. In controversial situations, we would expect early obsolescence as new knowledge was generated.

3. Important differences between scientific and legal mores have been noted whenever these cultures interact. In the SC, it is proposed that the mores of the scientific community be enforced. Ad hominem attacks are unacceptable in scientific debate.

4. Following well known philosopher, Sir Karl Popper, nonfalsifiable statements are not part of science and do not share the credibility that scientific statements earn by surviving varied refutation attempts. Nonfalsifiable statements (e.g. recommendations for action) will not be part of SC reports.

The Science Court Task Force

In the mid 1970's the SC attracted considerable support. President Ford had appointed a Scientific Advisory Group restoring the advisory function that President Nixon had abolished. This group appointed a task force (that I chaired) to examine the SC and to propose action. In an Interim Report the task force proposed "a series of experiments to develop adversary proceedings and test their value in resolving questions of scientific fact." It outlined initial procedures and anticipated the need for a number of experiments before a persuasive procedure could be developed.

The Task Force proposed conducting a colloquium to give proponents and opponents an opportunity to state and debate their positions. A second objective was to give people who had been active in scientific controversy an opportunity to "criticize and develop the rules of procedure...." The proponents were members of the Task Force.

The Scientists Institute for Public Information had vigorously opposed the SC. Margaret Mead and Alan McGowan, chair and president of the Institute, accepted invitations to present their views to the colloquium. However, Mead gave the matter more thought. By the time of her talk, she expressed the need for a new institution, as quoted above. She objected to the notion that one side might "win" in a SC procedure and also feared that, in time, this procedure like all social inventions would be corrupted. McGowan expressed fears that the SC might result in cutting off further research. That fear was inspired by the use of the word, "Court." Task force acceptance of the media term, "Science Court," was a mistake and has sadly led to much confusion.

Science Court Experiments

Efforts to begin developing a more credible procedure started with an attempt to persuade the federal government to sponsor experimental procedures. It was not hard to persuade presidential *campaigns*[9] to promise such developments. However it was not found possible to get *elected officials* interested in developing an institution intended to limit their flexibility to state the scientific facts as they wanted them stated.

At a public meeting of President Ford's advisory group in 1976, Philip Handler expressed a willingness to have the NAS host the experimental adversary procedures. Meetings were held, but in the end it was decided that these experiments would not fit the academy structure.

It gradually became clear that although both sides of the Washington politics-science complex would give lip service to the need for new procedures, they were unwilling to aid in creating an institution that might not be easy to control. I gradually came to understand that, when undertaking to change a "government of men" to a "government of laws," you must expect "men" to resist.

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9 Ford and Carter statements are reproduced in C&E News, Oct. 18, 1976, at 28; Reagan's endorsement appears in Physics Today, October 1980, at 50.

University-Based Scientific Adversary Procedures

Such efforts have been much more welcome on university campuses where they have a number of advantages. Scientific Adversary Experiments (SAE), i.e., experiments intended to begin the development of effective procedures have been conducted at U.C. Berkeley (on Love Canal) and at Dartmouth (on SDI) and are more fully described elsewhere.[10] Also, a record of the Dartmouth Proceedings is available.[11]

The development of Scientific Adversary Procedures (growing out of SAE) to a point of general utility is a substantial undertaking. It will be necessary to develop procedures that are not only acceptable to all parties but also exhaustive enough to lead to a statement of current knowledge adequate for the needs of policy makers. Time and resources required for such a comprehensive statement would be comparable with or larger than those needed for current committee procedures. In the SAE reported below, time and resource limitations prevented any attempt at full statements of current knowledge.

Mediation Results

The most striking output from these procedures has been the degree of agreement that could be achieved between the scientist-advocates on the scientific facts. Preceding the scheduled public cross examination, the advocates were invited to make factual statements basic to their positions. It was almost always found that adversaries were in substantial agreement so that an agreed upon statement could easily be negotiated. In the Love Canal case, these agreements were not recorded. However, it was apparent that areas of disagreement selected for cross examination were far less important than advocates' previous positions had led us to expect.

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[10] Roger Masters & Arthur Kantrowitz, *Scientific Adversary Procedures: The SDI Experiments at Dartmouth* in TECHNOLOGY AND POLITICS (Michael Kraft and Norman Vig eds. 1988).

[11] The record of the first and second Dartmouth Scientific Adversary Experiments (2 vols. 1985), available ($5.00) from Thayer School, Dartmouth College, Hanover, NH 03755.

Therefore in the Dartmouth proceedings, care was taken to carefully negotiate agreed propositions. These were so far reaching that very little serious disagreement remained for the scheduled cross examinations.[12] It is worth noting that in areas where secrecy precluded cross examination, the advocates reported that disagreements on the scientific facts still persisted. The contrast between open matters (under the threat of public cross examination) in which apparent disagreements were quickly dispelled and secret matters (where no such threat existed), confirms our faith in this method.

These procedures served to emphasize an important limitation — secrets cannot be dealt with in open procedures. When authorities support opposing interpretations of secret information, we are constrained to depend on faith in the saintliness and the wisdom of the authorities. The history of the damage done to the U.S. by governmental secrecy has not inspired that faith. Governmental secrecy and democracy are polar concepts.

Scientist-Advocates

It will also be necessary, as Mead pointed out at the colloquium, to develop the profession of scientist-advocate with a cadre of people skilled in the use of procedure to exhibit facts that support their point of view. We need skilled scientists who know the weaknesses in an opponent's position and are prepared to ask illuminating questions.

I began to appreciate the lack of this cadre in the early 1970's when Congress asked the NAS to examine auto industry claims concerning the availability of technology that would enable them to meet the emission standards set in 1970 amendments to the Clean Air Act. Handler had proposed that the academy answer this thorny question by a "Science Court" procedure, and I was appointed to the NAS committee that was to supervise this bold undertaking. We quickly discovered that all the leading academic scientists involved in emissions technology had their research supported by the auto companies. They told the committee that asking their sponsors the probing questions we

12 Masters & Kantrowitz, *supra* note 10, Appendix.

needed answered would destroy their research support. (The committee was rescued by Honda which, by producing the Civic, demonstrated that the technology was indeed available.)

Another example of the problems created by the lack of such a trained cadre is to be found in the current debate about "Global Warming." Federal and international funding for climate modeling is largely dependent on media attention. Professional climate modelers asking penetrating questions that would attempt to bring unnoticed weaknesses in current models to public attention are needed to test the claims of the modelers. Yet, before we can realistically expect to satisfy this need, we need a reward system (such as we have for whistle blowers) to compensate scientist-advocates for the damage done to their careers in uncovering information that might reduce funding for the whole field.

Self interest of scientists or their sponsors frequently favors one side of a public policy controversy. It is important to recognize that, today, the careers of all scientists are affected by public perceptions of their fields of activity. If the existence of this self interest is not publicized and balanced, it can greatly contribute to the noise level in the communication of scientific information.

Conclusions

Scientists attempting to influence public perception of what science knows or doesn't know should honor the self-policing norm that enables effective communication between scientists — namely they should stand ready to publicly answer the questions of expert adversaries. This paper reviews experiments with procedures based on the interaction between scientific adversaries in public policy disputes.

It was found that apparent factual disagreements between scientists prominent in public debates on policy issues could be dramatically reduced when they faced the prospect of public cross examination.