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Introduction

The very name "science court" got us into trouble nearly from the start, suggesting a return to Galileo's time when established authority could dictate that the sun revolved around the earth. Our intent was just the opposite, to make as objective as possible the scientific basis for controversial public policy by resolving technical disputes between biased experts. In the end, the science court itself became so controversial that it had no chance of success. Like a sky rocket, it got a lot of attention as it ascended but just as quickly fell downward to crash and burn.

Looking back, the post-Watergate presidency of Gerald Ford was a contentious time with debates in all arenas flaring one month and fading a short time later. Investigative journalism was still enjoying its greatest success, and many young practitioners in other fields including the natural and social sciences adopted the same combative stance, often as champions of little people at odds with the big interests. A good model for those times is the battle of Love Canal, pitting the working class residents of that community against the chemical industry;[1] or the citizens of Cambridge, Massachusetts, fighting a proposed university laboratory for recombinant DNA research.[2] At that time, perhaps the must visible involvement of scientists in a public controversy was the battle of New York Times petitions, listing in one day's advertisement hundreds of scientists who opposed nuclear power and on another day

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1 ADELINE LEVINE, LOVE CANAL: SCIENCE, POLITICS, AND PEOPLE (1982).

2 SHELDON KRIMSKY, GENETIC ALCHEMY: THE SOCIAL HISTORY OF THE RECOMBINANT DNA CONTROVERSY (1982).

hundreds in favor.[3] To the public (including me), technical expertise seemed no more reliable than psychiatric witnesses in a courtroom whose "scientific objectivity" often has the appearance of a chimera.[4]

Reminiscence

My first knowledge of Arthur Kantrowitz came from Professor Hans Bethe of Cornell University during our correspondence about scientific disputes surrounding nuclear power. I had sent Bethe my recently published analysis comparing the controversies over nuclear power and fluoridation.[5] These technical disputes are confusing, in part because of rhetorical devices used by the contending experts that obscure rather than clarify, and in part because the experts talk past each other. A calm analysis of opposing views — which almost never occurs — could clear much of this verbal thicket, but there would remain points of ambiguity upon which experts may legitimately disagree, and where it cannot be said that one is "right" and the other "wrong."

In order to clarify such disputes for the sake of policy makers, I proposed a procedure:[6]

> to allow the disagreeing experts to confront each other as adversaries before a panel of judges. These judges, who are not personally involved with either side, are presumably able to make a fairly objective, dispassionate decision on the merits of the argument. It is important to emphasize that the judges would not make a decision of policy such as whether or not to accept x cases of cancer for y amount of electricity. Such scientific and technological judges have no special wisdom or moral prerogative to decide how many cancers their society should accept. They might, however, be particularly qualified to make a purely technical scientific decision, such as whether or not x cases of cancer might occur in a population receiving y amount of radiation. The judges must be scientifically and technically competent,

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3 Phillip Boffey, *Nuclear Power Debate: Signing Up the Pros and Cons*, 192 SCIENCE 120 (1976); ALLAN MAZUR, THE DYNAMICS OF TECHNICAL CONTROVERSY (1981).

4 THOMAS SZASZ, THE MYTH OF MENTAL ILLNESS (1961).

5 Allan Mazur, *Disputes Between Experts*, 11 MINERVA 243 (1973).

6 *Id.* at 262.

> though they need not be specialists in the field of the adversaries. As the adversaries confronted each other, attacking and rebutting, points of disagreement would become clear. The natural process of polarization might even be helpful here since the opposing positions, pushed to their extremes, would be clearly contrasted. The judges might then be able to decide if either or both adversaries were wrong, or if the differences between them were legitimate, resting on points of irreducible ambiguity. In the latter case, they might then decide on the feasibility of reducing the ambiguity through further research. The judges' report would constitute part of the counsel given to policy-makers. If the report said that the scientific picture was inconclusive, then the policy-makers would have to proceed on that basis.

In 1974, with my colleague at Syracuse University, W. Henry Lambright, I circulated a modest proposal to test and evaluate this procedure. Following Bethe's suggestion, I contacted Kantrowitz and learned that he had made a similar proposal years earlier, first in testimony to Congress and then in an article published in SCIENCE.[7] Adopting Professor Field's literary device in his introduction to this issue, I could not convince a patent attorney that my "social invention" was importantly different from Kantrowitz's, and his has clear priority. The difference in our views lies more in fervor than substance, with Kantrowitz always seeming to me more certain of the workability and desirability of the science court — whether as a process or an institution — than I ever was. In any case, thereafter Kantrowitz incorporated my work into his own proposal[8] and included me in the rapidly developing activities.

The most effective and best connected entrepreneur whom I have personally known, Kantrowitz got the Ford Administration to set up a task force of the Presidential Advisory Group on Anticipated Advances in Science and Technology with the specific aim of exploring the workability of a science court process. He arranged for my appointment

7 Arthur Kantrowitz, *Proposal for an Institution for Scientific Judgment*, 156 SCIENCE 763 (1967).

8 *See e.g.*, Arthur Kantrowitz, *Controlling Technology Democratically*, 63 AMERICAN SCIENTIST 505 (1975).

to the Task Force; I was the only member whom none of the other members had heard of.

Kantrowitz never cared for the name "science court," usually calling his invention the "Institution for Scientific Judgement," a label so cumbersome that it demanded a nickname. The first person to oblige, as far as I know, was science journalist Daniel Greenberg in a snide article that began:[9]

> With over a score of influential supporters in and out of government, plans are going ahead in Washington to try out the 'science court' proposal that Arthur Kantrowitz...has been plugging for over a decade.

The New York Times quickly picked up the term and fleshed it out with a cartoon of laboratory instruments dressed in judges' robes.[10] Critics soon wrote of the "supreme court" of science, an image that ensured active debate and the attention of the news media.

In the meantime, the Task Force settled on a model for the court that was close to the early ones we had proposed but had a couple of added features. The most important addition, I think, was to initially obtain from the scientific adversaries not only the "facts" upon which they disagreed and intended to debate, but also those relevant facts that both sides accepted as correct. Thus, a useful output from the procedure, even before any debate in front of the judges, was a relevant list of consensually-accepted scientific statements. Most of the other additions were procedures intended to insure the fairness and smooth working of the process.

Kantrowitz got Philip Abelson, then editor of SCIENCE, to publish as an article (no need for peer review) the "interim report" of the Task Force, drafted mostly by him and me, that argued the case for the science court, proposed an experiment to evaluate the idea and announced a colloquium the next month in Leesburg, Virginia, to discuss the proposal further.[11] This prominent display in SCIENCE was

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9 Daniel Greenberg, *Plans Proceeding for "Science Court" Experiment,* Science & Government, Feb. 15, 1976, at 3.

10 John Noble Wilford, *Science Considers Its Own "Court,"* The New York Times, Feb. 29, 1976, at E8.

11 Task Force of the Presidential Advisory Group, *The Science Court Experiment: An Interim Report,* reprinted *infra* at 179.

a climax to the journalistic buildup and promised that the Leesburg meeting would be well attended.

Leesburg was a big event, sponsored by the Commerce Department, the National Science Foundation, and the American Association for the Advancement of Science, and attracting some 250 scientists, engineers, lawyers, administrators, and reporters. For me, lunching at a small table with a Nobel laureate, the President's science advisor, and Margaret Mead was testosterone inducing. Mead had come as the most celebrated critic of the science court. I watched her and Kantrowitz — two senior diplomats of science — charming one another toward their respective points of view. Kantrowitz prevailed, and Mead ended up endorsing the trial.[12]

Just as Kantrowitz had hoped, Leesburg gave the project a big boost, and, by January, 1977, the leaders of the nation's major scientific societies had endorsed the experiment.[13] But within days, Jimmy Carter replaced Gerald Ford as president, and the new White House science agenda, while not hostile to the science court, had other priorities. Plans for a grand experiment quickly faded away.

Retrospective

From the outset I was a strong proponent of an experimental evaluation of the science court process, but agnostic about the value of an institutionalized court. Lacking the grand experiment, I have not changed my views much, but over the years we have run small trials that address some of the questions that have been raised.

For example, at Syracuse University I acted as the referee in the initial phases of a science court held to study possible hazards from 60 Hertz electromagnetic fields associated with high-voltage transmission lines.[14] This was a highly contentious issue in New York State at that

12 Phillip Boffey, *Experiment Planned to Test Feasibility of a "Science Court,"* 193 SCIENCE 129 (1976); U.S. DEPARTMENT OF COMMERCE, PROCEEDINGS OF THE COLLOQUIUM ON THE SCIENCE COURT (1977).

13 John Noble Wilford, Wilford, *28 Leaders Endorse Science Court Test,* The New York Times, Jan. 2, 1977, at L28.

14 Mazur, *supra* note 3.

time, and, on contacting the major scientific spokesmen for the opposing sides, I learned that, while they had appeared as witnesses in the same regulatory hearings, they had never communicated directly with one another over their disagreements.

Drs. Robert Becker and Andrew Marino, then at the Veteran's Administration Hospital in Syracuse, were the anti-line scientists warning of a hazard from its fields. They produced for me a list of their factual claims, that I sent to their pro-line adversaries who responded with criticisms that the claims were vague and untestable. Becker and Marino revised their list in response to these criticisms, and I again sent it to the pro-line scientists, who generally found the revision much improved as a statement of facts in dispute. I ended the "experiment" at this point, despite suggestions for further refinement from the pro-line side, because it was already clear that the factual issues could be suitably isolated.

Kantrowitz has administered a few science-court-like hearings, at the University of California, Berkeley, and at Dartmouth, involving health effects from the leaked chemicals at Love Canal, and the feasibility of the "Star Wars" missile defense system.[15] Each of these experiments worked well in a university setting. There was no attempt to follow rigorous formal procedure as in a court of law. The norms of behavior were those of a college seminar, with the few added constraints required by the science court such as disallowing policy considerations to enter the discussion. Audiences always included students, professors and members of the public, most of whom apparently regarded the exercise as a valuable educational experience. Although lacking the rigor of the trials we had initially hoped for, these experiences still provide useful lessons.

The first lesson is that, contrary to the fears of critics, we never had a serious problem in separating questions of scientific fact from questions of policy preference. It puzzles me that this complaint is still raised today, in this journal, since I thought we had complied enough

15 Roger Masters & Arthur Kantrowitz, *Scientific Adversary Procedures: The SDI Experiments at Dartmouth*, in TECHNOLOGY AND POLITICS 278 (Michael Kraft and Norman Vig eds. 1988).

demonstrations to put it to rest. As a practical matter, the isolation of scientific questions is easy, despite philosophical arguments to the contrary. I claim this objection is a red herring, the gut response of those who deny that science is more objective than any other road to knowledge.

To avoid unnecessary argument, let me be very clear on what I mean. Granted that all human discourse involves evaluation and is "socially constructed" to some degree; that is not at issue. The pertinent concern is that people who favor one social policy often come up with different scientific "facts" than those who favor a contrary policy. It is these kinds of blatant policy values, and their effects on knowledge claims, that are to be removed from consideration. Values that are shared by all contending interest groups, or ones too subtle to affect practical decisions, may be intertwined in the statements of fact without causing a problem.[16] As a practical matter, it was always feasible to ask factual questions — e.g.: "Did the chemicals that leaked from Love Canal cause morbidity in the neighboring population?" — apart from one's position about what should be done for the people living there or to the chemical industry.

A second lesson is that, although experts on opposing sides typically misconstrue one another's position, when brought together they have discovered less factual disagreement than they (or anyone else) thought. To appreciate the impact of the science-court-like exchange of statements regarding high-voltage transmission lines, one must be aware that the opposing sides had virtually no direct contact during their three-year involvement in the controversy, and they never before had been called upon to compare their scientific positions on a point-by-point basis. One side found that it has misperceived the position of the other side. Some disagreements were easily resolved by rewording statements to be less extreme or better specified. By the end of our trials, there remained relatively few scientific questions upon which the adversaries disagreed. The most useful output from our procedures has typically been the list of relevant factual statements upon

[16] *See* Allan Mazur, *Science Courts*, 15 MINERVA 1 (1977) or Mazur, *supra* note 3, for detailed discussions of this issue.

which the adversaries agree and the smaller list of factual statements upon which they still disagree.

A third lesson is that the few scientific claims upon which adversaries continued to disagree have always been unresolvable with the present state of knowledge, usually because suitable data were lacking. This has always become apparent to us during the process, and I assume that judges evaluating such cases would realize it too. But if science court judges would inevitably be confronted by questions that they cannot answer for lack of data, so that their reports would predictably read, "not answerable with the current state of knowledge," then what function is served by the judges? This reasoning leads me to a surprising conclusion: *There is no need for a panel of judges* to decide which adversary is correct because, most likely, neither adversary will be clearly correct. If I were reformulating the science court proposal today, I would leave out the judges, making it in effect a mediation process. This ought to satisfy critics who fear that the court would become authoritarian. It is enough for the adversaries, with the aid of a referee, to work out in clear language the relevant scientific points upon which they do and do not agree. That, I suggest, would be useful information for policy makers, journalists and the interested public.

A fourth lesson: In all cases in which a science-court-like procedure was used, one side always wanted to participate and one side did not; the latter had to be prodded into cooperation. Invariably, the side that was losing the policy fight wanted to participate, apparently because they saw it as politically advantageous to do so. Conversely, the side then winning the policy fight was typically reluctant to enter a science court, apparently figuring that participation could not improve — and might erode — its favorable position.[17] The lesson here, I think, is that once these controversial technical issues reach the public policy arena,

[17] In a related vein, *see* Phillip Sperber, *Overlooked Negotiating Tools*, 20 LES NOUVELLES 81 (1985). In addressing the pros and cons of agreeing to alternatives other than litigation for resolving a patent dispute, Sperber observes, at 81:

> For instance, if patent validity or infringement is questionable, why take a chance with an arbitration *expert* who will know exactly how weak the patent is and how dubious infringement is? It makes sense to take one's chances with a *judge inexperienced in* the *technical and legal aspects* involved. [Emphasis added.]

they are primarily political controversies and only secondarily (if that) disputes over scientific knowledge.

Summary and Conclusions

We still do not know if a science court would work as intended, but some lessons to date are encouraging: The separation of factual and policy questions is practical. Two relevant outputs, a list of agreed upon facts and a list of facts in dispute, are easily obtained and widely perceived as useful. Probably the panel of judges, the focus of so much past controversy, can be eliminated. The subordination of knowledge goals to political goals, that we witness in these trials, emphasizes anew the desirability of counteracting the biases of polarized experts. And whatever else may or may not be accomplished, the educational value of a science court seems substantial.

Should we then press ahead with the old science court proposal? The basic problem addressed by the science court — providing good scientific advice — remains as important as it was in the 1970's, but it then seemed more pressing because challenges to technology were usually raucous and chaotic, coming from the undisciplined grassroots rather than established institutions. Protests often seemed to be voiced by maverick scientists or frantic and arguably uninformed citizens, a mix of media hype and hysteria. That has changed. In the past two decades, several oppositional groups have matured into sophisticated environmental lobby organizations, looking to the grassroots more for fund raising than for marchers. They now form an influential, Washington-based establishment in their own right, working more cooperatively with the federal agencies and Congress than in strict opposition to them. Today, national news media turn to respectable groups, not to the mavericks, for information about technological problems. Thus, the perception of undisciplined, raucous and chaotic technical controversy has dissipated. My personal perception is that technical controversies over social policy reach no better resolution than they did in the 1970's. But today, because they seem more orderly and under control, less need is perceived among policy analysts for a solution as radical as a wholly new social institution, like the science

court. Given that the proposal made little headway in the 1970's, it would make less today.

The present value of a science court lies right where we have located it, in the university, where it fits easily into established tradition, rather than as a grand new social institution. It can be carried out with university resources for the benefit of students and faculty. In that context it offers splendid educational value. It provides an opportunity for research on scientific policy making. It is a convenient device for the rational examination of specific scientific controversies. Let us promote its use in the university but not try to push it beyond that arena at this time.