Technical Issues and the Adversary Process

The proposal for a science court seems congruent with other initiatives taken in the past several years to throw a variety of controversies into the mode of quasi-judicial resolution.

These initiatives generally come from those whose experience with the law is limited. Trends within the law itself are moving in other directions, particularly with regard to the involvement of psychiatrists and other expert witnesses in legal proceedings. The bruises and misunderstandings of the adversary process have led many experts either to shun the courts entirely, or else to seek to have expert witnesses made the auxiliaries of, paid by, and even interrogated by, the court itself—not the parties to the controversy or their representatives.

The proposal for a science court is an attempt to inject institutional adversary argument into the resolution of conflict on technical issues while avoiding the hazards of regular legal procedures. To borrow a phrase from earlier battles, we might say that the objectives are admirable but the methods misguided.

First, the proposal overestimates the power and efficacy of the adversary process as a means of finding a truth—even current, provisional, working, technical truth—that the "loser" will concede to be truth.

Second, it underestimates the difficulty of framing questions in such a way that they will be appropriate for resolution by adversary proceedings. The main contest, one may predict, will shift from the stage of evidence to the stage of the pleadings.

Third, it underestimates the difficulty of wording decisions in such a way that they will both respond to the questions finally framed and help in furthering the discussion of the issues of policy. Practice might develop ways of deferring, modifying, narrowing, or broadening; but acquiring those skills takes time, and exercising them uses time that the political process may not tolerate.

Fourth, it rests upon a distinction—rendered no more persuasive by frequent repetition—between facts and values. The sponsors hope to couch the important issues in such a form that they can be resolved by findings of fact, with no contamination by decisions on value. Even if it were appropriate theoretically, the distinction would not be attained in practice. Findings in the purely technical sense would have and deserve no greater authority than the latest qualified professional contributions would; their special position as answers to questions posed in the context of a current controversy would make them implicitly value-laden—no matter how dryly numerical their language—if they were to be relevant to the further stages in the larger processes of decision.

The proposal, in sum, rests upon the wistful hope that conflict conducted in the public view on difficult technical and scientific aspects of controversial issues can be leached of its political juices. Do we have to spend years, and millions, to relearn the folly of such a hope? Over the last few years, experts and laymen have found other and better ways of communicating with one another, in a variety of official and unofficial forums. The proposal for a science court appears to be not so much a useful addition to the spectrum of working devices as a quaint fantasy of technical closure in circumstances of disagreement over policies.

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The Attraction of Minicomputers

Tad Pinkerton and Larry Travis raise some valid points in their response (Letters, 15 Oct., p. 257) to Arthur L. Robinson's article (Research News, 6 Aug., p. 470) on the increasing use of minicomputers for large-scale scientific computation. However, their conclusion that the apparent economies of the minis are due primarily to inadequate cost accounting is not entirely justified. It is true that a certain proportion of the true costs of running a minicomputer never appear explicitly in the budget, and that use is made (at universities) of cheap or unpaid student labor to run these machines; but it is also true that a significant fraction of the costs of using a central computer facility do not reflect the needs of the researcher who carries out large-scale, mostly numerical calculations.

A major part of this extra cost is simply the cost of complexity. As large computers evolved to provide simultaneous service to a varied community of users with differing and often conflicting needs, both the hardware and the software, and particularly the operating system, became extremely complex. The high price of modern maxis is largely attributable to this complexity, and so is the high cost of running a computer center, maintaining the complex operating

system, and providing the required services to the numerous, often unsophisticated users. As noted by Pinkerton and Travis, this complexity is also the reason for the long lead time in the development of new maxis, resulting in high development costs as well as a serious lag in the use of up-to-date technology (and thus a further increase in costs). The extent of these costs was largely unforeseen when the trend to large time-shared and interactive systems began. If research scientists can avoid this extremely expensive mode of operation, there should be no reason to discourage them.

Pinkerton and Travis question the use of research scientists' time in procuring, operating, and repairing computer equipment; but are such situations all that different from experimental researchers having to take care of their equipment? The time normally spent on this type of activity should be offset by the time (and frustration) involved in coping with the operating system and with the sometimes irrational operating policies of centrally administered computer facilities. Such policies are often determined on the basis of considerations which have little to do with getting the maximum use out of the expensive facility and often result in a lot of computer power remaining unutilized, while many large-scale computing needs cannot be satisfied because of high costs. Apportioning service and other costs in proportion to the time it takes the computer to run the program penalizes the experienced large-scale user who rarely utilizes such services extensively. Many such users would probably be quite willing to accept super-low priority service, at suitably reduced rates, to take advantage of unused capacity (mainly at night and on weekends) in order to carry out projects that otherwise could not be afforded.

Having control of their own facility and thus being able to choose optimal configurations and set operating policies to satisfy their own needs is another important attraction of owning a mini. The software limitations discussed by Pinkerton and Travis are usually not very severe, at least for the intended applications. In many cases a good Fortran compiler, an assembler, a text editor, and a file handling system are all that is really necessary.

This is not to say that the use of the big central computers is not often appropriate, or even essential. In many cases only the biggest available machines will enable a satisfactory solution of a scientific problem, and this is the principal reason for the National Resource for Computation in Chemistry. (Another important reason is the accumulation of

specialized software and applications programs and the expertise needed to develop, use, and improve them.) The principal issue, however, is money. With today's tight budgets it is often impossible to carry out much large-scale scientific computation at a central facility, even though the computer center can offer software facilities such as RPG, Cobol, PL/I, and centralized billing.

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A Neglected Literature

Konrad B. Krauskopf's appeal for information on radioactive waste disposal (Letters, 8 Oct., p. 134) indirectly draws attention to a problem of long-standing and increasing magnitude. Although Krauskopf seeks knowledge of unpublished reports, it is my observation that some *published* reports are not readily available. One of the reasons for this problem is that national and international concern about energy, pollution, and other important scientific issues is generating a voluminous amount of literature in the form of monographs, journal articles, reports, and so forth. Reports tend to be poorly covered by the traditional abstracting and indexing services, which still emphasize coverage of journals. In turn, these services are the bases for the so-called "on-line" computer services. Unless measures are taken to increase the visibility of this literature, its potential value to society may not be realized.

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Dating the Deluge

Science has published comments by five geologists (Technical Comments, 24 Sept., p. 1268) criticizing Emiliani's correlation (Reports, 26 Sept. 1975, p. 1083) of a sudden rise in sea level 11,600 years ago with the deluge dated by Plato as having occurred 9000 years before Plato's own work on the subject. The accuracy with which Plato's geochronological studies were carried out is clearly a question of substantial scientific interest.

I assume that Plato's deluge is the same event as that described in the Old Testament. I am therefore in the process of forming an organization to finance expeditions in search of Noah's Ark, in order that we may get to the heart of this matter with radiocarbon dating. The response to Emiliani's correlation gives me great hope that generous financial backing for the organization can be found within my own profession. I invite the writers of these comments and all others who take their work so seriously to join with me as founding fathers, mothers, and stockholders in a venture guaranteed to return profits and glory as well as a scientific solution to the problem of Plato's flood.

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Peer Review: Preventing "Nepotism"

In my previous position, I was a scientist at a large midwestern university in a section composed of seven staff members charged with the study of cancer biology. The section is supported by about 50 percent of the funds allocated to a cancer center grant from the National Cancer Institute. The yearly budget of the center is more than $1 million. During my last year, the center was "site-visited" in order that a request for a 5-year renewal of the grant could be considered. On the site-visit team were four or five members responsible for evaluating the section. Of these, one had been the doctoral thesis adviser of two staff members in the section, and a third staff member had done postdoctoral work with a second member of the site-visit team. I question whether these site-visitors could possibly judge the scientific merit of the work in the section without bias. Some of the projects carried on by the section had actually been spawned in the laboratories of the two site-visitors. One may ask why these site-visitors did not disqualify themselves. Serving on a site-visit team carries considerable prestige; furthermore, academic salaries are frequently barely adequate and consultants' fees are attractive. Obviously, we are all human, and so, too, are potential site-visitors. Criteria should, therefore, be established by the granting agencies to disqualify scientific "fathers, mothers, and other close relatives" from site-visit teams, study sections, and other peer review groups.

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