Wildavsky's comment that technology assessment (TA) and other management information systems are being "established without a single successful demonstration, . . . are tried everywhere, and . . . do not work anywhere" triggers a question: How do we know whether or not TA works? I am troubled not so much by the performance of TA to date as by the dim prospects of rationally evaluating and improving performance in the future (*1*).

Such prospects would be enhanced by the performance of *multiple* (for example, three) TA's of given topics. Multiple TA's would enable comparison of usefulness to various parties, post hoc evaluation of the accuracy of forecasts, and estimation of the relative value per dollar invested—each as a function of who the assessors were, methods employed, and topics assessed. Users would be better able to gauge reliability and would be ensured a broader perspective.

While it has been asserted that a TA realistically costs about $200,000 (*2*), the lack of TA evaluations makes it difficult to determine whether a project costing $5,000 is less worthy than a $500,000 venture (*3*). Performance of multiple, coterminous TA's at different funding levels could clarify this issue. One could surmise that the cost would properly be a function of the technological complexity involved and the needs of the users.

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1. This relates to the basic question of social experimentation. See A. M. Rivlin, *Science* **183**, 35 (1974).
2. V. T. Coates, *Technology and Public Policy* (George Washington University, Washington, D.C., 1972), vol. 1, pp. 2–12.
3. This cost range is suggested in U.S. Senate, Committee on Rules and Administration, *Report on the Technology Assessment Act of 1972* (Government Printing Office, Washington, D.C., 1972), p. 21.

Exchanges with China

The informative article by Harrison Brown, "Scholarly exchanges with the People's Republic of China [PRC]" (11 Jan., p. 52), makes it clear that the Committee on Scholarly Communication has a tremendous task in the development of scientific exchanges between the United States and the PRC. As Brown states, the committee obviously cannot expedite exchanges in every field. However, I wonder about a system of priorities that resulted in the selection of a group of Americans to discuss the eradication of schistosomiasis, but not a group to discuss the eradication of venereal diseases.

Epidemic gonorrhea and communicable syphilis currently rank first and fourth, respectively, among reportable diseases in the United States, and the incidences are rising. It has been reported (*1*) that venereal diseases have, for all practical purposes, been eradicated in China. So far, there has been no evidence to refute such reports. Therefore it would seem that, in the order of priorities, one of the "particular areas in which Americans potentially have a great deal to learn from the Chinese" would be venereal disease control.

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1. E. G. Dimond, *J. Am. Med. Assoc.* **218**, 1552 (1971).

I completely agree with Kuhn that the eradication of venereal disease in China is a great accomplishment. However, it may be attributed, not to advances in medical science unknown in the United States, but to China's very effective social mobilization and public education campaigns. The Committee on Scholarly Communication with the PRC has expressed considerable interest in sending scholars to China to study social organization in city neighborhoods and communes, but these programs have not yet been accepted by the PRC.

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Drug Education Conference

Nicholas Wade (News and Comment, 14 Dec. 1973, p. 1114) reports on a travel program which was presented to the participants of the International Congress on Drug Education, held in Montreux, Switzerland, in October 1973. This travel program, which

is described as having been of inferior quality by Wade, offered absolutely everything to accommodate each participant at the lowest possible cost. The people responsible for the program provided more services than would be considered normal. Every individual received exactly what he paid for. The arrangements were clearly indicated and spelled out in the travel brochure received by the participants. Some people received what they did not pay for, since they left their hotels without checking with the cashier. Because prices were cut to the bone, it was not the U.S. taxpayer who was the "innocent victim" but the people in charge of the travel program.

Wade makes little mention of the scientific implications of this congress. Some leading industries apparently thought its value was great enough to underwrite the financial loss that resulted from insufficient participation.

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Pathologic Evaluation and the Blind Technique

At the Conference on Carcinogenesis Testing in the Development of Drugs, held by the National Academy of Sciences–National Research Council (23 to 25 May 1973), Robert Elashoff suggested that, in carcinogenesis tests, the pathology slides should be sent to the pathologist blind (unidentified). Morris A. Weinberger (Letters, 23 July 1973, p. 219) comments on that suggestion.

While several of Weinberger's comments might be relevant to human pathology, Elashoff's suggestion referred to carcinogenesis testing with animals, and it is only in that context that we discuss Weinberger's letter.

A carcinogenesis test is performed in order to determine if a treated group is associated with a higher incidence of cancer than an untreated control group. The control group must be handled in exactly the same manner as the treated group. By sending unlabeled slides to the pathologist we ensure that the integrity of the control group is maintained during the pathologic evaluation.

Weinberger asserts that histopathologic diagnoses are considerably less influenced by subjective factors (for example, knowledge of the treatment given to each specimen) than by other factors. However, a carcinogenesis test is conducted to investigate carcinogenic properties of the treatment in question and not to determine the biases, no matter how small, of a particular pathologist. The prudent investigator should therefore give serious consideration to the blind technique as a means of avoiding errors associated with the influence of subjective factors.

The blind technique can be incorporated into most methods of evaluation. Weinberger points out that the pathologist often wants to review the control slides to establish a basis or orientation for the examination of the slides from the treated group. One method by which this could be done using the blind technique would be to make up extra control slides, select a portion of them by a random process, review these with full knowledge that they are control slides, and then put them aside; finally, the remaining treated and control slides could be examined in a random order using the blind technique. Thus, the pathologist's orientation would be established, and a reduction in bias would be achieved.

Weinberger implies that good pathology is objective. The good pathologist should therefore consider the blind technique as a simple means of control to help prevent systematic errors from being introduced when labeled slides are examined. He should also appreciate that use of the blind technique is an assurance of the lack of bias in the pathologic diagnosis made in an experiment.

There is nothing in the blind technique to preclude a pathologist's exploring "interesting new research clues," as Weinberger fears. Notes can be made on slides that show interesting or unusual findings. After the slides have been identified, the findings can be associated with the treatments received and the pathologist can explore his interests. By using the blind technique he may even avoid wandering down some blind alleys.

The blind technique does not require that the pathologist be kept ignorant of experimental information. Good pathology, like good statistics, requires that the professional partici-