OMB Approach to Management

The report on the role of the Office of Management and Budget (OMB) in managing federal science programs by Barbara J. Culliton (News and Comment, 1 Feb., p. 392) reads like a description of the early stages of the Apollo program. NASA hired Bellcomm, Inc., to act as their "OMB" in overseeing Apollo activities. As one might expect, adversary relationships were quickly established. The NASA field centers were never as cut off from top management as, say, the National Institutes of Health appear to be cut off from Nixon, so the associate administrator for Manned Spacecraft often found himself with conflicting advice from his management "experts" and his operating personnel.

My responsibility was the space environment—radiation and meteoroids. Bellcomm arrived on the scene just as we finished a major effort to stop the spacecraft contractor from spending large sums to evaluate radiation damage to spacecraft materials (we felt that if we protected the astronauts, we needn't worry about transisters and rubber hatch seals). We suddenly found ourselves on the other foot, required to show cause for not abolishing the Solar Particle Alert Network, which was the basis for our radiation protection system. The hassle was eventually referred to the Manned Spaceflight Science and Technology Committee, a group of noted outside scientists, who made recommendations in our favor.

Having been out of the Manned Space Program for 5 years, I have tried to look at the experience with Bellcomm from the point of view of the executive who needs help in maintaining cognizance and effective influence over large programs. Bellcomm did some good; I am sure OMB does some good. In any large operation, some waste, misdirection, and general nonsense is swept along with the good parts; there is some probability that an outside reviewer will recognize the nonsense, and successfully bring it to the attention of the executive.

The problem, to borrow a term from *The Peter Principle* (*1*), is that the OMB approach is "input oriented." They continually ask: "What are the alternatives? Should the government be doing this at all?" The problem is not the questions; it is their application to the entire spectrum of federal programs by people who can't tell whether they are given a good answer. The President, and the country, would be better served by an "M" part of OMB that practiced "management by exception," an "output oriented" approach that extended the executive arm in cases where performance showed a need.

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Operations Research

On reading Aaron Wildavsky's admirable review (28 Dec. 1973, p. 1335) of Brewer's book *Politicians, Bureaucrats, and the Consultant* (*1*), I was suddenly brought up short by the phrase, "some low-level operations researcher." I have recently been reminded of my own experiences in that field by the release, by British military security authorities, of a manuscript I wrote in 1946, which they have at last allowed to be published (*2*). In that period, we did not think of operational research (or operations research to Americans) as a low-level activity; 5 of the 30 to 40 people who worked for the Operational Research Section, Coastal Command, Royal Air Force, became Fellows of the Royal Society, and 2 received Nobel prizes. From what Wildavsky writes, I suspect that the poor results of the attempts to provide scientific assistance to the decision-makers in the cities of Pittsburgh and San Francisco arose, partly if not mainly, from a neglect of some of the basic principles developed during the rather successful wartime efforts to help the British Air Staff, who were running an almost equally complex enterprise.

Pat Blackett (later Lord Blackett, Nobel Laureate and president of the Royal Society) wrote two basic memoranda, in 1941 and 1943, about the methods of operational research (*2*, pp. 6–9, 25–30). Among his main points are these: "The first step, that of collecting the actual data, is by itself of enormous importance, for it is not uncommon for operational staffs to be unacquainted with what is actually being achieved." Then, "Closely related to the collection of data is the necessity to obtain a clear definition of the problem which the data are expected to elucidate." It seems that neither of these preliminary steps were adequately looked after in the two city studies. Then Blackett distinguishes two methods of attacking the problem: the "*a priori*" method, by which he means constructing a model system, with variables related by differential equations (for the solution of which we had at that time no computers to call to our aid), and the "variational" method, in which one agrees to "abandon the attempt to construct from first principles a complete imaginary operation something like the real one under investigation, and to replace it by the attempt to find, both by experimental and by analytical methods, how a real operation would be altered if certain of the variables were varied." He points out that "the results of *a priori* investigations can rarely be left to stand on their own feet; in most problems they need supplementing by the 'variational analysis.' " Surely neglect of this lesson contributed heavily to the failure of the San Francisco and Pittsburgh analyses and may also greatly diminish the value of the "limits to growth" model (*3*).

I suggest adding to the very sensible general rules given by Wildavsky two more, right at the beginning: don't be niggardly with time or money in getting the real facts (not opinions, facts); and weave backward and forward from model-making to *testing* the effects of interim decisions. I should also like to add a final rule. It is not enough to see that hypothetical benefits "outweigh estimated costs by at least ten to one." Often the benefits and the costs are in any case incommensurable (for example, aircrew lives weighed against the time scientists spend figuring, or living standards weighed against architects' drawings). More important, it is rarely worth altering existing procedures, which everyone concerned is used to, unless the forecast increase in efficiency is at least 50 percent, preferably 100 percent, to be attained in some reasonably short period, before the next alteration becomes necessary.

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3. D. H. Meadows, D. L. Meadows, J. Randers, W. W. Behrens III, *The Limits to Growth* (Potomac Associates–Universe Books, New York, 1972).

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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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