The problem in making decisions about funding basic research is simply that there is no way of knowing in advance whether you are funding a dramatic discovery or another dead-end experiment. It may even be true that after the research is completed and the results reported there is still considerable uncertainty about the importance of the work. Since resources are limited, it is impossible to accept the argument that, in the face of this uncertainty, we should simply fund every project proposed by every scientist who wants to do basic research; basic research is not "invaluable" in the sense that no amount of resources committed to it is too great. On the other hand, we cannot wait for the results and then supply the funds to the new Gibbs and Mendels. We, as a society, must risk some resources in the game that basic science is playing with nature. The correct amount to risk, so long as it comes largely from public funds, must, however, be a public decision; that is, one made by the Congress. It should *not* be one made by scientists. But scientists must help to inform that decision by devising criteria for choosing among basic research proposals. For example, scientists can help by providing judgments about the people involved, the experimental designs, and even the (admittedly uncertain) potential for significant discoveries in the fields in question.

Incidentally, economists have known the difference between price and value for almost 200 years (*1*). The question here is not one of price versus value, but of assigning value to activities with uncertain outcomes, where the variance of the distribution of *possible* values is enormous.

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Reference

1. See the diamond-water paradox in A. Smith, *The Wealth of Nations* (Modern Library, New York, 1937), p. 28.

Russell gives a reasonably precise description of the process whereby federal funds are allocated for the support of research. This process, depending upon congressional and executive priorities and peer judgments by scientists, is entirely familiar. It is necessarily a before-the-fact judgment, and therefore difficult.

This is, however, not the problem that I considered. After the experiment has been done, after the report has been published, how do we decide upon the value of the work? Was it or was it not worth the price that was paid? These after-the-fact judgments should, one might expect, be relatively simple; yet, even these prove to be a subject of controversy. It was to these limited considerations that I addressed myself in the editorial. The question of funding of basic research was well outside the scope of its subject matter and indeed was not mentioned. I surely had no intention of coming to grips with Russell's problem, and I note with interest that he did not come to grips with mine.

I am in complete agreement that there are some economists who appreciate the difference between "value" and "price." I carefully charged only that there were some economists who would have us believe otherwise. These, I must assume, are less familiar with their Adam Smith than is Russell.

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Marihuana and Motivation

In E. L. Abel's report "Marihuana and memory . . ." (10 Sept., p. 1038) the results do not seem to be qualitatively nor quantitatively relevant to the matter under discussion. Abel appears to neglect motivation; he assumes that the attitudes of the subjects to the experiment remained unchanged after intoxication.

It is known that a "high" changes the perceptions of marihuana subjects. Relationships and actions seem to acquire or lose significance independently of pre-"high" processes and desires. Thus, after intoxication, the "high" subjects must have related quite differently to the experiment and to the experimentalists. Such changes would affect their desire to recall word lists, and so forth.

The numerical differences that Abel obtained appear quite small when viewed in this light. They certainly do not have much bearing on a hypothesis about the effect of marihuana on "acquisition processes involved in the storage of information."

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In the report referred to by Marx, the motivational variable was assessed using the β index of signal detection theory. In addition, this variable was also dealt with in a previous publication (*1*). In both cases the data were interpreted with reference to the influence of this variable where it appeared to be relevant. As to what the subjects felt during these experiments, I leave that to Marx. The only relevant experimental findings regarding the effects of marihuana on perception are those of Caldwell and his associates (*2*), who were unable to detect any significant differences in perception between experimental and control subjects except for an auditory discrimination measure. I am thus unable to comment on Marx's statement that "it is known that a 'high' changes the perceptions of marihuana subjects," as I know of no other related experimental studies. Finally, I contend that a 13 percent difference in the ability of experimental and control subjects to learn a list of 120 common words is not quite as meaningless as is suggested by Marx.

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References

1. E. W. Abel, *Nature* **231**, 58 (1971).
2. D. I. Caldwell, S. A. Myers, E. F. Domino, P. E. Merriam, *Percept. Motor Skills* **29**, 745 (1969).

Freedom of Information

Nicholas Wade (News and Comment, 4 Feb., p. 498) refers to the forced release of the Garwin report on the supersonic transport as a prime example of the effective working of the Freedom of Information Act and hails the strong action taken by Appeals Court Judge David L. Bazelon. Would it not be appropriate also to give the name of the attorney whose year-long efforts and able presentation to the lower court, and then to the appeals court, paved the way for the landmark decision? I refer to Peter Koff, an active member of the Sierra Club and other conservation groups, and the assistant director of the Citizens League Against the Sonic Boom.

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