Letters

What Price the Lunar Rocks?

The Space Task Group report to the President entitled "The Post-Apollo Space Program: Directions for the Future" is beginning to receive the careful attention it deserves from the several sectors of society most directly affected by its proposals. Unfortunately, a few commentaries such as Abelson's editorial (10 Oct., p. 171) appear to lack adequate objectivity.

While admitting that the Apollo program has provided "boosts to national pride and a sense of dignity to men everywhere . . . [and] stature to the nation . . . more effective[ly] than much more costly military efforts" Abelson observes that "the lunar samples are proving very interesting, but they are scarcely worth the $500 million a pound that some news stories have assigned them." Apparently, all of the first-mentioned benefits are considered to have come free, with no portion of the total program costs chargeable to them. In addition, all that has been learned of a scientific or technological nature from Mercury, Gemini, four preceding Apollo flights, and the potential of nine more lunar landing flights has or will come free, since the full cost of 10 years of manned space flight is included in the figure of $500 million a pound for Apollo 11. This biased and unrealistic accounting procedure should be avoided in our evaluation of the worth of manned space flight activities.

Although the scientific objectives (and their reasonable share of the total cost) of the early Apollo flights have been limited, it seems inaccurate and premature to classify their results as "relatively meager." Even the "Preliminary examination . . ." (19 Sept., p. 1211) of the first lunar samples must stand as one of the most fascinating and significant reports ever to appear in this journal, not only to geologists and mineralogists but also to students of many related areas of planetary evolution.

Abelson's contention that man in space is now of diminished importance (even if our attention were restricted to science alone) does not appear well-founded. It is only because earth-orbital flight is just now becoming more routine that we can begin to utilize man fully in our experiments. The advantage of versatility surely lies with a manned experiment, designed to permit sensor exchange, repair, and modification of the observing programs.

If present plans for a new space transportation system based on a reusable launch vehicle are adopted, a reduction in the cost of earth-orbital payloads by at least an order of magnitude is expected. This would completely alter our present thinking about ways in which men and equipment are employed in space. And this is precisely the area in which the Space Task Group (including the President's Science Adviser) has recommended that NASA should proceed.

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Social Science Research

"Project Cambridge: Another showdown for social science?" (5 Dec., p. 1250) is a good presentation of the early history of that project and of its current state. But the part that attempts to summarize the grounds on which a number of M.I.T. faculty members opposed the project appears abbreviated to the point that it calls for supplementation.

Contrary to the impression created by the article, our objections to the Cambridge Project were not principally that it was funded by the Department of Defense. The issues are very much more subtle and deeper. Part of our concern is over the impact that the introduction of any large project, regardless of how it may be funded or even what its mission is, may have on the Institute. Another is about the propriety of having *any* social science research, however benign or uncontroversial it may appear, funded by a mission-oriented agency, particularly on a contract as opposed to a grant basis. Finally, along with many others, we worry about the impact of social science on society generally, and, more specifically, about the effect that particular sources of support may have on the work of the social scientist.

There is little question in our minds that a number of large projects currently active at M.I.T. have had an effect on the curriculum and the research orientation of the Institute that was not planned at the time these projects were initiated. In some cases these essentially side effects compete in magnitude with the anticipated major effects. They compare to those the Institute might expect were it to start a new academic department. But a new department is first subject to long and searching examination by many components of the Institute's faculty and administration. It seems to us legitimate to ask whether a project as large as the Cambridge Project should not be subjected to the same careful review before it is taken on.

Academic research should be characterized by the open problems it attacks. The fact that some component of the real world may find the fruits of research useful can serve enormously as a stimulant—especially in the search for open problems—but finally the question itself and the ideas proposed to answer it must determine the direction which the researcher takes. From that view of academic research, it follows that a research proposal addressed to a funding source ought to state clearly the questions to be asked, the problems to be attacked, but it ought not to imply that the research will solve the agency's problems. The Cambridge Project proposal begins by outlining the enormity of what are perceived to be social science problems faced by the Department of Defense. It goes on to propose that certain work be done and strongly suggests that even intermediate results will soon prove useful to the sponsor.

We think that this is a fundamentally wrong approach and that it has

already been elevated to very nearly a principle. As such it leads to the erosion of the autonomy of the university. When practiced cynically, that is, on the basis that it is after all what the funding agencies demand, it leads, at the very least, to a kind of double bookkeeping. And that erodes the integrity of the university, especially in the eyes of its students.

The current mood of the nation should have made us all sensitive to the social and political consequences of what we do. Murray Gell-Mann said (7 Nov., p. 723) that "it used to be true that most things that were technologically possible were done. . . . Certainly, in the future, this cannot and must not be so." The "element of choice" which he argues must be an essential element of engineering from now on must, in our view, include not only the decision to do or not to do, but also the decision regarding for whom the research is to be done, and what controls are to be imposed. The consequences of military sponsorship must be searchingly examined. Project Camelot has taught us that the very credibility, hence the effectiveness, of the social scientist can be eroded by military funding. The question raised with respect to the Cambridge Project is not so much whether the researcher funded by it will similarly lose his credentials in the eyes of the peoples and communities he may wish to study, although that is a serious danger, it is rather whether such discreditation does not represent a sort of folk wisdom. Perhaps the people generally understand much better than the social scientists do that militarily sponsored research ultimately serves the military and strengthens an already overpowerful military establishment. This is independent of the good intentions of the scientist. The Cambridge Project proposed to barrel ahead with its kind of work, however, without even once asking for a cent to study the social consequences either of carrying on that kind of work, or of its success if that should be achieved. It is too late to be that confident in or that blind to the future.

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. . . In the spring of 1967 I participated in the exploratory discussions that have since matured into Project Cambridge. We believed then, and I still believe now, that the time is ripe for a major effort to apply modern computer science to the study of individual and social behavior. This effort would involve both the reformulation of social science theories in terms compatible with computer analysis, simulation, retrieval, and experimentation, and the development of programming systems and languages appropriate to the special interests of social scientists, both to be supported ultimately by a large, powerful computer installation dedicated to this sole purpose.

The computer promises to be as important for the quantitative sciences of behavior as the microscope was for scientific biology, so one would hope that some of the nation's best talent would develop this new tool. The Cambridge community, which includes outstanding contributors both to the social and behavioral sciences and to computer science, seemed an ideal place to undertake such a major effort. Other suitable locations could probably be identified; I have argued elsewhere that the government should establish several such projects around the country.

It is my impression that no one has questioned the importance of the purposes of Project Cambridge. Responsible opposition has not even claimed that the healthy development of social science poses any insurmountable threat to our individual freedom or our democratic institutions, although this kind of neo-Ludditism has been heard in other quarters. The central argument against the project seems to be that it is funded by the Department of Defense.

The temptation to try to jockey Harvard and M.I.T. into the position of rapping the Department of Defense on the knuckles by explicitly withholding "institutional endorsement" is understandable, perhaps, but hardly rational. Such an audacious gesture would gratify those who see in the Pentagon all that they dislike most about our society, but it could cost far more than it is worth: It could reduce scholarly contributions to socially relevant decisions about defense and leave them even more firmly in military, industrial, and political hands. It could delay the development of scientific knowledge and techniques that our whole country sorely needs. It could establish the precedent that one segment of a faculty may outlaw the research of another segment. It could imply that a university must endorse all aspects of an agency's or foundation's program before accepting their support. And it could confirm the all too common fear that social scientists cannot be trusted to be socially and morally responsible for their own work.

The basic issue, of course, is not Project Cambridge per se, but who should support what research. It is a sad commentary on our current situation that the Department of Defense should be the only branch of our government with the vision and resources to support such a major project. Certainly, more appropriate channels could be imagined if Congress were interested in creating and funding them, and many alternative proposals have been heard in recent months. The issue deserves wide public attention and wise congressional resolution. While we are discussing the broader issue, however, this particular baby is likely to be thrown out with the bath water. A project of enormous potential value to the country is in danger of being lost.

Congress, already suspicious that civilians in the Pentagon have been redirecting defense dollars into research that was not sufficiently "mission oriented," are likely to listen with special interest while the social scientists of Cambridge try to reassure their local critics that the Project will not *really* serve the defense agency's operations. By the time Harvard and M.I.T. finish what Coburn calls their "useful soul searching," they may find that Congress has resolved their dilemma unilaterally. In that event, everyone will be a loser—the public included.

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A Name to Fit the Crime

I'm sure I speak not only for myself but for other concerned citizens of the same name when I say that I resent your use of BOB to refer to the Bureau of the Budget (Editorial, 26 Dec., p. 1579) and suggest that, at least as long as it maintains its opposition to medical research, you refer to it as BuBu.

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