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