

UNITED STATES GOVERNMENT

Memorandum

TO : CB/ Scientist-Astronauts

DATE: August 6, 1970.

FROM : CB/ Phil Chapman

SUBJECT: Icebergs Ahead.

It is surely clear by now that the titanic manned space program is not as unsinkable as it may have seemed a year ago. We have very few friends left, in the scientific community, in Congress, or amongst the general public. The budget withers, interest sags, the "middle option" for the '70s program seems more and more an illusory pipe-dream, and there is little sign of any effective action, inside NASA or out, to reverse the trend. The latest blow was von Braun's statement last Monday, at the NAS meeting at Wood's Hole, that OSSA's \$400 million budget proposal for Skylab B experiments has shot that vehicle out of the sky. If you will forgive the metaphor melange, I think it is high time to consider whether there is anything we can do as a group to shore up a bulkhead or two, or whether we should be prepared merely to come to attention on deck as the ship founders beneath us.

I, for one, have a dumb faith that man is in space to stay, and perhaps some dramatic development will save the program -- for example, the Russians will surely overcome their engineering problems with their big booster and the Komarov malfunction, and then may shatter effectively the complacent feeling that we are safely ahead. But we can't depend on this sort of thing, and anyway we are confronting very serious damage in the interim. In any case, our generation has the chance to lay the foundation for getting mankind out of the cis-lunar nursery and I don't think we should allow that historic privilege to be deferred to some later generation without a fight.

Many of NASA's present problems are NASA's own fault. It is of course a difficult time, when science and technology are depicted as destroyers of the environment and/or instruments of war and the nation's attention is engrossed in urgent problems such as VietNam and pollution and the ghettos. All that means is we must perform better and fight harder instead of meekly going under, crying excuses.

I would indict NASA on at least the following counts:

1) Failure to spread understanding of the importance of the program. We have relied on spectaculars and the space race, both of which are ineffective now -- everything is anticlimax after Apollo 11, and the Russians are (seemingly) out of the running. What we are doing will be remembered when VietNam and even the whole communist-capitalist struggle are as dusty as the Peloponnesian Wars, but there is little public awareness of what a remarkable thing it is to be living now.



2) Failure to develop a coherent and imaginative plan for what we are trying to do in space in the '70s. Planning for new generations of vehicles is generally fairly well done, but there is no strong central authority accepting responsibility for what we will do once in orbit. The nation will clearly no longer accept getting into space as an end in itself and there will be no program unless we exhibit a total commitment to truly significant and cost-effective work in space in science and applications. We should demand the most stringent standards of excellence in the conception of experiments, but we are swamped in mediocrity. In view of the investment required, some at least of the P.I.s should have a chance for the Nobel as a result of the experiments we do for them, but most of the work is of a quality which, on an objective basis, without regard for the mystique of having been done in space, would hardly qualify for publication in a high school magazine. If we cannot develop a clear and defensible rationale for the experimental program, we should quit wasting public money on it. The problem is apparent even in mainline, where NASA is very vulnerable because each mission, which now can have no other justification than science, costs over half the annual NSF budget -- but there is virtually no attempt to discuss openly and candidly why the incremental value of each new set of lunar rocks or each new ALSEP on the surface justifies this expenditure. Much worse is Skylab, where, with a few notable exceptions, the experiments are miserable trivia, whether they be concerned with science, technology or applications. I cringe at the thought of trying to defend Skylab before a technical audience, once the paltry inanities of the experimental program are embarrassingly clear. If NASA is in trouble now, just wait until the enemies of the program learn what we are spending on such great breakthroughs as potato circadian rhythm and the gravity substitute electric chair. The only possible way we can get away with Skylab A is by claiming that it is principally a necessary demonstration of human endurance in free fall -- and that rug is likely to be jerked out from under us by a Russian long-term flight before Skylab flies. Unless Skylab B goes with a significant suite of experiments, there will be nothing after about the end of 1973 until the advent of the shuttle -- and that won't be until the '80s unless we do a much better job of demonstrating a value for man in space than we have so far, or will in Skylab. What I have seen of the experiment definition for the space station and space base exhibits the same sorry signs of gestation by committee, which is responsible for most of the congenital defects of Skylab, instead of the strength and creativity which is mandatory to justify those programs and the shuttle.

3) Failure to adapt managerial policies which were effective during the massive, time-critical phase of Apollo, when cost-effectiveness was subordinate to getting the job done, to the realities of the operational and utilitarian program of this decade. Over-design of systems (for a trivial example, have a look at the milled aluminum doors of the Skylab lockers in Dick Truly's office), failure to streamline documentation, maintenance of bureaucratic structures which are no longer relevant, acceptance of incredible costs for fixing minor RIDs and a lack of a hard-nosed, cost-conscious central direction are just inexcusable in the context of the program with which we are now dealing. In system after system for Skylab, the engineering and even the fundamental concepts are simply

incompetent, and failure to discard the obsolete management practices which have allowed this is nothing short of dereliction of responsibility to the space program and the nation.

It is, of course, easy to be critical in this crisis for NASA. The only meaningful question, however, is what we can do, as individuals or as a group, to move towards correcting the situation. I would like to suggest the following:

- i) We should speak out boldly and loudly and often in in-house circles about the fundamental problems listed above, especially #2, where we should have a special competence. If this treads on some toes, or offends our hierarchical superiors, or steps outside our assigned responsibilities, that is preferable to sitting back passively while the program collapses.
- ii) We should make a vigorous effort to formulate a clear understanding amongst ourselves as to why man has a useful role in space, especially for scientific purposes, and then try harder to influence our scientific peers, by attending meetings, giving papers, and writing for publication. When we have access to the Press, we should abandon the platitudes of the party line and speak out strongly on the fundamental purposes of the program and its relationship to other social goals. We should not be afraid of constructively criticizing NASA where that seems appropriate.
- iii) If Skylab B fails to fly, and no alternative project is defined and approved for the mid-Seventies, the manned spaceflight program is very likely to lose all momentum and the nation will be faced with a serious and costly problem of re-building the organization when external conditions eventually demand that we get back into space. If lack of an economical and effective experimental program is killing Skylab B, I suggest that we tackle this problem by ourselves, as an independent project. I would like to ask each of you to come up with a list of at least five significant experiments in your discipline, either from your own ingenuity or as a result of talking to your colleagues in NASA or elsewhere. If we truly believe that there is anything meaningful for us to do as scientists in space, we should be able to back up the conviction with some concrete examples. After a candid and critical bull session to cull out the trivial and insignificant or overly ambitious from the resulting list of (hopefully) 75 to 100 experiments, we can enlist the aid of experts as needed to form preliminary estimates of weight and power and space and cost requirements, and finally come up with a document setting forth an example of a complete suite of important experiments for a mission in the Skylab class. I believe we can do this because we are almost certainly the most highly-selected group of scientists which has ever been assembled and probably represent the strongest concentration of diverse scientific talent which exists in NASA, if not in the country. I am quite impressed with the potential capabilities of the scientist-astronauts, compared to other scientific groups I have worked with in the past, even if we have had little opportunity as yet to demonstrate the contribution we are capable of making to the program. I am proud to be a member of this team. Furthermore, for the present purpose, we have the great advantages of interdisciplinary contacts among ourselves and a better understanding of what is and is not feasible in space than is available to scientists outside the astronaut office. The resulting document, if well done, could have a considerable impact not only in NASA

HQ but in the National Academy of Sciences and other scientific circles, demonstrating that a little dedication and imagination can in fact generate a significant scientific program for man in space.

If I get a favorable response from most of you to this suggestion, I shall ask Tom and Deke to allow us some time from our other responsibilities to work on this, but I think we should be prepared to do it in our own time if necessary. I shall offer my house and a copious supply of beer to this project and undertake to act as file clerk and administrative factotum. To be effective, we need to finish the project as soon as possible, certainly within a couple of months.

I have tried to word this memo strongly, but I think I have understated the seriousness of the crisis facing manned spaceflight. I do not think we would be usurping the function of anybody in undertaking to generate a meaningful scientific program for Skylab B, because there does not seem to be any unified group in NASA charged with this responsibility, which is now left largely to the chance response from the scientific community to Flight Opportunities announcements. OSSA and the MSFEB serve primarily a critical rather than a creative function -- and of course I would expect to submit the output of the study to these organizations if it got as far as being a serious proposal. Right now, the important thing seems to be to give a powerful example of a creative program, unified and scientifically sound and offering a good return on the investment. I would therefore envisage the document as falling somewhere between the typical Blue Book of candidate experiments and an actual proposed program.

It is possible that, in the course of this study, we might come up with some experiments which were sufficiently cheap and easy to build to have a chance still of flying on Skylab A, especially if, as seems likely, that project slips further, thereby offering an opportunity to alleviate the disaster for the credibility of man in space NASA will confront when called upon to defend its experimental program for those missions.

The basic message I am trying to put across here is a call to action in defense of the program, and I would certainly welcome any suggestions for other things we might do.

Phil