JL - response to Newell 4-16-69

Memo on "SPACE BIOLOGY"

Since this document seems to scrupulously avoid reference to manned flight, I assume that this is not the place to expand on requirements for biological investigations in support of extended manned voyages. I will, therefore, take the stance of the biologist whose sense of priorities is uninfluenced by the broader issues of the exploration in space and who would view the program solely from the standpoint of basic biology and to some extent its applications for terrestrial problems.

I trust you will realize that this is not representative of my own position which would give much heavier weight to the general issue of the exploration of the solar system.

I do not believe that the emergence of a "coherent and unifying theory of biology" is a realistic goal for the next decade in the context of the present discussion. I am perhaps biased by the view that such a theory is to be found primarily in 1) molecular processes and 2) the general theory of adaptation, natural selection, and evolution. Neither of these is especially germane to studies in space physiology with the particular except~~ance~~ion of the possible bearing of the space environment on evolutionary mechanisms. The implication in the middle paragraph that space biology during the next 15 years is likely to make vital contributions to world problems is going to be attacked by many people and I think you will be hard put to set up a plausible ~~sanerio~~ scenario for that outcome. The thinking in terms of large systems that characterizes work in space may yet prove to be an indispensible contribution to the solution of earth-bound problems; but I believe few biologists would give a very high priority to the specific kinds of information that can be most economically achieved by biological experiments in space.

Space physiology has indeed focussed attention on two areas of investigation mentioned in the document as relating to gravity and periodicity. I have yet to note any really exciting outcome of work on low gravity. On the other hand, I foresee a great deal of interest in studies, stimulated by space efforts, on artificial environments with high gravity and with high intensity vibrational stresses. These are environments which are very relevant to space physiology but which can, of course, be simulated on earth much more cheaply than by space flights.

Almost the same remark can be made about periodicity. I do concede, however, that there is a range of phenomena for which we may not readily produce sufficient isolation from terrestrial rhythms and these would be good candidates for experiments in space biology. That case is still a speculative one but it certainly does demand a place in our forecasts for the biology of the next decade. Once again I would stress that I do not minimize the importance of space-stimulated efforts at studying parameters that had been relatively neglected until now. Periodicity undoubtedly plays a crucial part in behavior and misbehavior in complex organisms especially and may be of very considerable importance in human mental health.

When we turn, however, to the exploration of space we have the potentialities for the discovery of new forms of life, of new habitats to which important adaptations will have to be made, and indeed the whole set of challenges of "unearthly" problems. This is I believe very well stated on page 2 and I may scribble only a few minor adjustments to this section of the write-up.

The program objectives appear, however, to be quite plausibly stated and I find this section of the document quite persuasive. I am, however, puzzled that there is still no reference to lay the groundwork, not necessarily for the support of long range ventures in the manned exploration of space but even to provide the information needed to make wiser judgements on less than a crash basis in the future about the potential feasibility of extended voyages. For many reasons human subjects are not always the most appropriate experimental material for studying the biological impact of space environments. One need not attempt to justify space physiology in terms of a settled commitment, whose costs might be alarming, for early manned space exploration; but we surely should be taking advantage of any lead time we have to uncover special problems and ways of dealing with them. For example, the interaction between space gravity and solar radiation is an important area about which we know very little. Furthermore, since we already know of some potential hazards, for example the depletion of calcium during prolonged space flights, we could use the time for experimental studies to establish the best physiological and pharmacological regimes to counteract these effects. Once again I do not think human subjects should always be used for exploratory work of this kind. Consider the implications of procedures like prolonged hypothermia.

The general studies of adaptation mentioned on page 3, paragraph 3a, would, of course, open the door to the entire area of applied biology. The program can be specified with varying degrees of legitimacy as being space relevant. For example, the approaches needed to make the confined volume of a spacecraft cabin habitable over an extended period of time might have a definite bearing on solving air pollution problems on the larger scale of the earth atmosphere. I have in mind, for example, the development of biological as well as physical-chemical filters for the elimination of noxious products like carbonmonoxide and chlorinated hydrocarbons. The system approach that could make Mars habitable might do some good on earth too. At the very least the spacecabin atmosphere monitoring procedures are very relevant to earthbound conditions.

I am enclosing a few trial efforts at revising some of the specific text for whatever use you can make of it.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

JL/rr