STANFORD UNIVERSITY MEDICAL CENTER

DEPARTMENT OF GENETICS

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Dr. Edward C. Ezell
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Johnson Space Center
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Houston, TX 77058

Dear Dr. Ezell:

Since I spoke to you last week I have now had a chance to look over the draft of the first two chapters of your manuscript on Mars exploration. You have the advantage of perspectives of high levels of decision making that give you such an advantage with respect to breadth of view, that I really do not have a great deal to add to the detail of your account. Certainly it gave me a great deal of information that I did not have before, particularly about the policy background of the choices between lunar versus planetary and manned versus unmanned missions. So I only have a few comments of a rather general nature.

Your discussion is rather thin about the maneuvering that preceded the establishment of NASA and the allocation of tasks to the civilian versus military competitors for a role in space. Perhaps this is not the place for such discussion but I thought the issue deserved more explicit mention. My own efforts to seek a policy about planetary quarantine were begun against that background of uncertainty as to who would have responsibility for U.S. efforts in space.

Similarly I might have suggested that you highlight a bit more strongly the shift, from pre-1958 images of the exploration of the planets by manned flights, as against the subsequent emphasis on the development of instrumentation that could telemeter significant data on issues like the presence of life on Mars. This shift was very much connected with concerns about quarantine - which would have been impossible for a manned mission. So, paradoxically, the development of this caution about the means of planetary exploration undoubtedly accelerated the pace at which it was eventually initiated!

As to the scientific background of exobiology: Chapter 1, page 37, I would also draw attention to the very important development of "comparative biochemistry" between about 1936 and 1956. The book by Kluyver and VanNiel The Microbe's Contribution to Biology, 1956 is a seminal overview of this doctrine, which reached its culmination with the work by Beadle and Tatum (1941) on the biochemical genetics of neurospora. Norm Horowitz was one of Beadle's proteges. Of particular relevance is Horowitz's paper, 1945,

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8/30-77

"On the Evolution of Biochemical Syntheses" which appeared in PNAS 31:153; it was reprinted in "Extraterrestrial Life: An Anthology and Bibliography" as an appendix to the 1965 NAS MARS summer study. This global perspective on the underlying unity of life on earth was an essential precondition for an informed inquiry about patterns of life elsewhere.

As we discussed in our conversation, a good deal of effort - and this was the major responsibility of the space science board - had to be exerted in response to substantial criticism of investment in planetary exploration on the part of a wide variety of other scientists. This is one of those points that everybody knows but it did not come through very explicitly in your draft.

At page 73-74, you discuss the microscope and multivator experiments. The decision making about the role of these instruments was more complicated than your account gives out. The microscope was by no means a "complex heavy instrument", especially if viewed as an optional attachment to an existing camera system. It did have the problem of requiring a costly data channel for getting the information back to earth. In addition, as I mentioned in our interview, there were difficulties in interpreting objects selected at random that made this a less preferred choice in the absence of other methods of getting selective information or selective concentration of the sample. We did some quite promising work on the latter approach using flotation.

The problem with multivator was the heat-lability of the substrates we were working on. Apart from that the concept isn't all that different from the other metabolic measurements. Your writing is somewhat ambiguous about the extent to which these experiments were in direct competition with one another. I was never a particular protagonist for any of our own experiments and quite cheerfully joined in an overall look at an optimized system. Many of these ideas arose from collective discussions over a substantial period of time and it would be difficult if not impossible to trace the details of authorship.

If you have the letter mentioned in your letter 51, chapter 2, Urey to Newell, 29 March 1961, I would be curious to see the detailed wording.

Yours truly,

Joshua Lederberg,
Professor of Genetics

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