



STANFORD UNIVERSITY MEDICAL CENTER

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Dear Bill,

Our mutual colleagues here have called my attention to the white paper which you had put on the AIMADV directory and on which I will give you some comments shortly.

By coincidence I have had somewhat similar preoccupations recently and in fact had sent you a message, of which I enclose a copy here now. I should stress to you that all of this was written without knowledge of your own draft.

I am still trying to think through the basic strategies of responding to this mission for WHO. Of course, it has a very important difference from the challenges for this country, namely that the immediate problem is to bring the rest of the world up to a standard that even nearly approximates that which has already been achieved here.

I understand that you may be visiting here within the next few weeks and I would certainly enjoy having a chance to talk over the draft with you in some more detail at that time.

That would be a better occasion to discuss the details of your white paper, but I might mention just a few comments now.

Of course, it is true that environmental engineering based on rather crude knowledge of infectious process has accounted for the larger part of the control of contagious disease in developed countries. Where it was possible to develop an effective vaccine, as in the case of smallpox, we have substantial hope of actual eradication even on a world-wide basis. But even here we should comment that imperfect knowledge of the vaccine almost frustrated the effort, since it was only when stable vaccine preparations that could be assayed for their effectiveness became available that really effective control on any global basis actually became possible. A great deal of previous effort was wasted owing to the inefficacy of the vaccine in a tropical environment. Probably similar things might be said about the last ditch eradication of, for example, typhoid fever - the efficacy of vaccination programs on the basis of present knowledge there being probably almost entirely mythical!

LT. J. P. KENNEDY, JR. LABORATORIES FOR MOLECULAR MEDICINE, DEDICATED TO RESEARCH IN MENTAL RETARDATION

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The infectious diseases that still remain a serious problem in many parts of the world, like venereal disease, tuberculosis and above all malaria and the parasites, probably can not be efficaciously controlled on any socially and economically realistic basis without additional technology! But technology will hardly be enough if there are not the means and the manpower to disseminate it.

If we pass by those considerations and turn to the degenerative and neoplastic disorders, which are our major public health problem in this country, then of course I agree that we absolutely require much more sophisticated knowledge of the disease process.

My own perspective on the role of artificial intelligence in that arena does not quite coincide with the presentation that you have offered if I understand it. I guess I am not optimistic that artificial intelligence by itself is going to lead to a revolution in our understanding of a particular disease. Rather, I think of it as a major supplementary tool together with any other lines of research to enable us to make much more rapid progress than might otherwise be possible for the solution of a variety of very tangled and complicated problems. In this way, for example, the DENDRAL effort is just as relevant to the understanding of disease by the support that it can give to the comprehension of chemical structure and then of metabolism, chemotherapy and so on, as are the programs that are more immediately directed to the modelling of disease processes.

I do agree with your remarks just before the heading "Opportunities for Technology" about some of the system impediments to the actual solution of disease problems. But I think you should clarify that this is not so much the lack of incentive and motivation within the academic sphere as it is the extreme complexity of dealing with the political and social environment which constitutes the setting of disease control. Academic people are simply not going to be given the power to implement the kinds of changes that will accomplish the goals that we all agree upon. So in that framework it is indeed true that there is not much encouragement or motivation to attempt an impossible task from that particular platform. However, I agree that more could be done than is at the present time to bridge the gap between academic research and practical applications. But for a very good example of how not to do it, look at the cancer crusade with its implication of centralized management knowing all the answers!

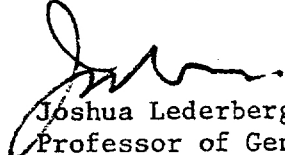
If it were only possible to get across the message that it is the business of the political central management to define the goals and then to get the scientific talent needed to effectuate those goals from the periphery, I think we could make much more rapid progress. I do not think a few people in Washington or even in the Bethesda are going to compete effectively with the scientific and medical insight that is distributed around the rest of the country in the definition of scientific solutions to the problems once these have been carefully stated and the incentives to solve them laid out by political decision.

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Well, I really do think it will be much easier to go over this if we do have a chance to discuss it in person. If that does not prove to be practical, I will try to prepare a more deliberate set of comments.

To return to my original point about WHO, you can see that I am myself engaged in a rather similar exercise in trying to formulate some strategy for the most effective collaboration of technology and social decision in what may be an even more complex environment. So, any help you can give me on that score would certainly be very much appreciated. However, as I may have indicated before, this is to be kept somewhat discrete as I am still not exactly clear what my role will turn out to be when I return to Geneva.

Sincerely yours,



Joshua Lederberg
Professor of Genetics

JL/rr