I had not intended to make a prepared statement. I had some ideas that by indirection Dr. Nordby has already uncovered the point I was trying to make as well. I had just completed a manuscript which did not obviously bear on the issues before this Committee, but I will show the way in which it does.

This is a discussion, an attempt at prophecy as to what the next 50 years of human evolution are going to look like, and my prophecy includes an assertion that much is going to happen in evolutionary terms to the human species in that length of time, as happened in the last 100,000 years. The line of argument that this comes from is the application of recent new knowledge in molecular biology and more than that in developmental biology in its actual application to the structure of human beings.

I don't want to go into detail of this, because in detail my prophecy may be wrong. I have certain specifics about the particular things that are going to happen.

This discussion is addressed mainly to a rather technical audience. It may not be very comprehensible. I apologize. It wasn't written for the present purpose. But I think it would be difficult not to believe that some very profound powers are being arrived at by human beings with respect to control of their own nature, whether they have to do with modifying existing human genes, whether they have to with the long-term actions of new psychedelic drugs -- like LSD -- whether they have to do with modification of development in the uterus, which is my own favorite speculation as to the line of most impact.

The point I want to come to is that we are reaching a point where the applications of scientific principles in a very literal sense is coming right into the home. It has come right into doctor-patient relationship, that large numbers of human beings are going to have to participate in some very tricky moral, ethical, religious and biological decisions on a scale never before required.

Now we already see that to a considerable degree, when a person is asked whether or not he might be the doner of a kidney, he faces an issue that he has never, that his ancestors have never had to face before in the history of the species, and it is one which is brought up by advances in medicine. I just use this as an existing example of the new sorts of relationships between human beings that are uncovered by advances in science.

As we get into areas of control of human reproduction, not merely in a quantitative sense, but in some fashion in a qualitative sense, I think it is perfectly obvious how pervasive these are going to be.

Now I don't believe even this room contains sufficient wisdom to define the policy that ought to be entertained for the species in dealing with these matters. These are questions that are going to have to be left on an individual basis, if we are to survive with any vestige of a democratic effort in any of these fields. It is perfectly obvious what the response to this must be. We need to think very aggressively about the level of philosophical, humanistic and scientific education that our next generation needs, and where we fail, we face problems incomparably smaller than our children and our next generation are going to have to come into.

The mere support of pure research per se, as magnificent as it is, can only exacerbate these problems, if we don't provide an underlying base for higher education to the maximum that our society is capable of generating.

I would like to put it that in a sense, if we are to face up to the responsibilities of the moral balance of science, if science is to be used in a moral fashion, that there is an intellectual overhead that should be attached to any kind of mission directive especially, but even to basic science research projects.

The specific approach that I would like to recommend, I haven't thought about this a great deal, I don't understand all the wrinkles, is that rather than quarrel with universities about cost sharing, for them to find funds from where, and I meant the word "impoverished" when I said it and I will illustrate what I mean in a moment, participating in the cost sharing of basic research, that if we are to have science in a big way we must have higher education in an even bigger way. And rather than demand a token three percent university contribution on top of each project, I think there ought to be a two or three hundred percent overhead attached by way of support of general university institutional functions that it can decide on for itself, where you have a quality control that is built in by the fact that these additional funds go to where the projects go.

Now this concept is not very different from the base grants that have already been started in NIH, and they are extremely important. They haven't spelled out very much beyond science, but even within science there are many imbalances that have got to be ironed out. The project system is a magnificent one for quality evaluation. It gives one's peers a chance to criticize what is going on. I think it is fundamentally a good one but I also feel it is being pushed too far and I think we have to think of a few parallel schemes.

One of the things that is very difficult to do on a project system which is where most of our university support is coming from at the present time is an organization of a new program, is to try and start some new kind of activity, which isn't yet popular enough or yet well understood enough or the people haven't been identified enough or the pieces haven't been put together in such a fashion that it can be attractive. I really think that we are not going to solve the fundamental moral issues that surround scientific activity in any centralized fashion whatsoever. We can only do this in a peripheral way. We can only do this by multiplying the sites of excellence so that these are responsible institutions that have the resources to cope with the broad range of problems that they have been attached to responsibility for; so that the scientist works in a context of other kinds of scholarly activity where he can be, from the point of view of his own philosophical approach, in what he is doing tempered by his colleagues. It is only this indirect sort of process that I can visualize as manageable as a way of introducing socially important considerations into the day-to-day activity of the scientist, because no one individual, no one group is ever going to be smart enough, even if he were smart enough today, so much will have passed this afternoon as to make obsolete the decisions which he made this morning to attempt to attempt to direct from the center.

The only possible answer to this is the strengthening of our institutions of higher scholarship, and I think there, unquestionably, we can from the point of view of their capacity to use their own initiative in the organization of the programs, and this applies both within the sciences and outside.

Now we hear this complaint about the opportunistic way in which universities behave. This, of course, is true and I introduced the word "impoverished" to show why it must be true. How else can the university respond to the opportunity of getting funds, against matching money, when it has no other openings? Where else is it going to be able to build anything if it doesn't use whatever leverage it can possibly find from what is offered it? It is all very well to think Congress may become interested in strokes one year, it may be interested in mental retardation another year, it may be interested in cancer research in a future year. One can point to positive accomplishments in each of these areas through positive action. So Congress may believe that if it decides that another problem area is important and it pumps money into it that it will get a positive response to it. It has forgotten that there is not even now an adequate base for peripheral initiative right across the board in medical research, in basic scientific research, for the ingenuity and the social conscience of each of the people doing this work to be properly exercised before we even ask the question whether enough is being done on strokes.

Of course, another $100 million in any subject area will provide some return in that subject area. It will do so because we have at least a few people left in administration who are wise enough not to take Congress that seriously and that literally that it will demand a report from each investigator to give some support for what is going on there. "Exactly what did you do for how many patients on strokes this particular year." This is the wisdom that Mr. Webb has talked about. The most important virtues of NASA's space program may end up being the by-produces, the cases where it was wise enough to realize that it was building the technological and scientific underpinnings for one project of important, of spectacular significance in the international political sphere, and for all the other reasons.

Now Mr. Weinberg brought up I think a legitimate question. I might have a different answer to it than he does, but I think it is well worth discussing. If these ancillary effects of a space program are so important, is the mainline mission to which the space program has been directed the most efficient way of getting these ancillary effects? It may prove to be. But I have not seen enough discussion on whether this would be the case.

On the other hand I think we should be very cautious about rapidly building up and tearing down important creative activities. When the Appolo Program was first being discussed, I resisted it vehemently. I did not feel that landing a man on the moon was the most important, the most significant, the most creative way in which we could use even our rocket and space capability. As long as it was a question of fundamental decision about which way to go, I felt I had to fight it tooth and nail.

The time came -- I was overruled. You know a few other people disagreed with me at that point and I felt slightly bitter about it, but one accepts that. At a point when this had become a going concern, when it was going rather well, when we were going to achieve some missions, when things were reasonably well organized, I refused to resist it. I made what appeared to be a change in stance but it wasn't because I felt that we were in a stage in the development of that program where it would be merely destructive even to take potshots at it, and rather than even take the potshots at it, I hoped that there would be enough creative insight in NASA that there would be enough spillover that there would be a sufficiently padded program, that it could do all of the side effects as well as the main job.

Now some of my scientific colleagues acted more in principle and didn't agree with me, and, of course, you know all the static that they have raised all along about the lack of scientific virtues of landing a man on the moon. So, look what happened. They convinced Congress that space isn't really that important that one ought to by principle cut it by 10 or 15 percent every budget year. And, of course, what got cut out? Congress and the nation were already committed to landing a man on the moon and we all know that what got cut out was to a large extent the important by-product activities that NASA was supporting in a rather creative way, and I am very sorry about that.

But with a too subtle point to convey to my colleagues in the scientific world and even more so further down the line. Well, we are at a new point of decision. I am talking more than I should about Apollo, about post-Apollo. I think it is a valid question what the next major step should be. I winced a little bit when Al said that manned exploration of the planets has been strongly recommended. I thought he was hinting at it from the scientific community. As far as I know, the scientific support for manned exploration of the planets is trivial. There is a lot of interest in unmanned exploration. There is also a lot of argument about exactly what one might hope to find and how valuable it will be, and perhaps one ought to find out more about what is on Mars before one makes permanently irretrievable major commitments in that direction, but there is clearly an area very well worthwile of investigation.

This is something certainly that can be done somewhat more economically than a complete manned program would entail, and if we play our cards right we can have it the best of all possible ways because we can be continuing our investigation of the potentialities of deep space, we can maintain some of the technological machinery to go along and, of course, there are a lot of other things to do besides continued manned exploration for which there are plenty of social, economic and political benefits.

There may be also plenty of extra-scientific motives, that is non-scientific motives to maintain a large manned commitment, and if they are not scientific I don't feel especially qualified to deal with them, and I fully expect to be over-ruled again on an appeal of just that kind.

But if that happens, if commitments are developed on other kinds of grounds, then let's recognize what they are for and let's recognize the additional sort of ancillary support which is needed to get the most out of that kind of a program and I think we will have a more creative outcome.

Well, the main point I would really like to stress again is that Dr. Nordby was perhaps acting rather gently and I would like to do it more vehemently. Maybe it is not asking the right question to ask is there a proper allocation when we speak about more basic research activities as between cancer research and stroke and mental retardation, and so on.

Among other reasons it is not a proper question as you would have very grave difficulty in making a really adequate classification of what money is being spent for where. I can very honestly get money from different institutions of the NIH under all of those respective labels and end up doing exactly the same thing with them when I come to the laboratory to do my work, because in any fundamental area of investigation, it takes only a gross lack of imagination to see possible ways in which there might be the most important impact of some fundamental activity on what you want to do.

Now let me point to one very explicit case, and no central wisdom would ever have known that this was worthwhile doing. One of the most outstanding advances in medicine in the last five years has been the discovery that a very important disease in man, it has been called Mongolism, but some of my friends think this is a very dirty word, so we will call it down syndrome or an extra chromosome, this has nothing to do with the Mongolian race, it is the effect of a distribution of a chromosome during the formation of the egg. In a sense the mongolian child is another species from the human being. He really differs by almost as much in his genetic constitution as many other species do from one another. It is an astounding discovery, not only for medicine but also from the fundamental issues of human biology.

Now how did we get to know this? What were the routes in scientific investigation that led to it? The most immediate foundation for these studies was a study o f the chromosome number in wheat which is motivated by strictly agricultural research purposes. If there had been a strict line in 1920 about applying money for mental retardation, and be very careful, you must spend it for mental retardation and not for any other purpose and if this had resulted in the inhibition of investigations on the chromosomes of wheat, we would not know this most important single factor that there is about mental retardation at the present time.

Well, science is mindful of this, there have been more undirected discoveries of great import of cases where the investigator himself was not wise enough to know the importance of what he was doing than of the converse. Almost nothing has been discovered in science by somebody who is looking on any large scale important issue by somebody who is look for it.

I look back over my own career, and nothing I ever did of any importance was what I looked for. It was always the things I wasn't looking for in the course of it. Now, I think administrators, Congressmen, everyone not at the laboratory bench must have a much deeper feeling for this than is ever expressed by any of the kind of language in a program that comes on.

Now behind this there is a confusion that has been alluded to a couple of times but which absolutely must be erased, and that is the difference between the pursuit of new knowledge, which is basic science, and which simply cannot be directed. Try and direct it and you will not succeed. Try and direct it further, be disappointed in it and you will simply set up a bureaucratic machinery that will hound us all to death, and you will raise your costs by five and reduce your productivity by ten, which is sometimes happening through here. But this is just, of course, not the $16 billion we were talking about. I doubt if it even belongs to a figure of $1.6 billion, where we are talking about pursuits of new knowledge.

Now obviously on the other side of the fence, when we are talking about technical advances, when we are talking about the application of existing knowledge to social and human ends, this must be programmed. You must know what ends you are trying to solve before deciding what kinds of machines to build, what kinds of facilities to set up, whether you should have more of one kind of hospital or another, and so forth. But let's keep these straight for gosh sakes.

They are not the same kind of activity at all. By definition basic research is what is completely nonsensical to attempt to program. But do realize that there is a very large area, and the most creative area that certainly falls in that category. And to pursue that, there is simply no mechanism that can possibly work except building up the strength of those institutions that are dedicated to this which have the, I don't mean to call it the by-product activity, whose central core is the education of the next generation.

When you remarked, for example, about the pay-off in science, and you speak about the present technical stature that the United States holds in the economic world of today, a very large part of that is not scientific information per se. A very large part of that is the ability of our institutions to train technicians, to train engineers who will have that additional creativity, that additional route, that additional ability to keep abreast of the times, because they have been educated at institutions where basic research was going on.

Our present policies support this by indirection. They are beginning to erode the rest of the game. When there is any question about impoverishment of institutions, just go into the policy making of any major university at the present time and find out what it is able to do in terms of the resources that it has at its own disposal and answer the question in those terms whether you think it is impoverished or not. You don't answer that question by finding out whet it is $5 million or $100 million worth of endowment. You find this out by finding out what the responsibilities of this institution are in relation to their resources.

I sat at a meeting yesterday afternoon. We have a creative opportunity for building a new biology department. There is about $180,000 missing in the budget next year that cannot be found anywhere. It is not the kind of money that is available in any form at all from Federal sources. We have used up all of our resources for it that can be matched in any conceivable way for this purpose, and when you see this day after day after day, and every university administration office is full of this, you have a picture of this.

Well the formula that I have mentioned of respecting the intellectual overhead should possibly be only one of very many. This says our centers of excellence are not on that firm ground that you can forget about them. They are in trouble. They will not stay excellent if you forget about them while you are trying to build up the rest, and this is not resisting the idea of more general support. We need more centers as well.

I think these issues can be faced squarely and directly, that they can be separated in terms of what the functions of Congressional appropriations are, and there ought to be money specifically allocated for building new forms and new institutions. I can't answer your question of where we decide these kinds of allocations should go. I think it is not preposterous to think of this in essentially a "pork barrel fashion." I think just giving money to the States will filter down eventually to the right places but it ought not to preempt the strengths that we already have, and it is the notion that we are going to back off from recognition of existing quality which, of course, most of us feel is so threatening, because we will destroy a good thing if we are in the process of doing that.

I had a number of very general remarks about other items and I think I will save those for later on.

(Senator Harris: Delightful!)

(Dr. Lederberg continues) I hope some of you do read this. I would like to get some of your reactions to it. There is one thing I really want to bring up. It is so apropos, so timely, it came out this morning. I want to home in on this. This clipping came out on the artificial heart which illustrates a number of the isses we are concerned about here. It is part of the concern as to what national science policy should be. I think one issue -- there is one prediction about the economy of the United States that Ralph Lapp brought up that I think he is not taking into account, and some of you may wish to refute it.

I make the assertion that medical technology inevitably must become the major industry in the United States over the next 30 or 40 or 50 years. The reasons for this are in human psychology and what people will spend their resources on. People will spend their resources to stay alive, to stay productive, for their health and the continued viability of their families. They will do this when they know how to at highest priority over almost any other consideration. The amount that a given individual could fruitfully spend for these purposes, that is what you could buy from medicine for a given number of dollars, has always been in a situation of rather inflexible supply, that is above a certain number of dollars, there wasn't much more youcould do for yourself. And this situation is changing dramatically with the importation of new technology into medicine itself. And I think it is dramatized by nothing more acutely than by the artificial heart per se.

Now a number of system studies were done in this last year. This is something that was just finalized after a great deal of resistance attempted as an approach to define this sort of a problem and I will refer you to a remark which is very true, namely that it will be very dangerous to implement a program of the use of the artificial heart at a premature state because, "if patients can only be kept alive in the hospital" by the use of these devices the program will become an economic albatross.

Use your imagination and see what will happen. You will have devices insufficiently reliable, insufficiently portable, insufficiently effective that they can support the normal health work of an individual who has suffered a heart attack. In the past he would have died. He can now be kept alive. But for 15 years unless we do something about anticipating the technological requirements, we will be in the situation where these people will be in exactly the same stage as if they were in an iron lung. We will have to keep them alive somehow, or all possible economic efforts on the part of the involved people will be put into keeping them going, but without the technological response needed to bring this down to the next lower level where it could become generally available as a reliable device.

We need some way in which we can anticipate these kinds of issues. We can't wipe this off by saying the program has to be deferred until that point. As soon as an artificial heart of any kind is available anywhere, there will be a scramble to use it in any possible mode.

This is psychologically and socially unrealistic to think of deferring it in that way. We must think in turn rather of the political technological response that has to be made in order to obviate the very grave difficulty that I foresee in this single area at the present time. I think we are going to be in very grave trouble economically and politically as well as in many other spheres, if we don't attend to this right now. This is only one of a large number of comparable situations . It is the one which is in a way the most evident because of all of the medical technology, the artificial heart is the one most overdue. The technical issue is almost trivial. It is much simpler than building a supersonic transport probably and probably one might say it is even simpler than the fantastic kinds of control problems that go into the space system. We haven't faced up to it because there hasn't been the right interface between Congress, the profession of medicine, engineering, industry, and the NIH to recognize the problem and find some administrative framework in which to begin to think to deal with it.

Dr. Weinberg: It is very interesting that four of the six companies quoted here, Jim, are NASA contractors.

Mr. Webb: It is rather interesting that we furnish financial support to NIH to apply systems engineering to the problem too.

Dr. Lederberg: Before they knew enough to do it themselves.

Dr. Lapp: How did you get the idea I was against that? I thought that was what I was talking about.

Dr. Lederberg: You did not include in your general remarks about the role of technology over the next 60 years the revision of man himself, of which this is a part.

Dr. Lapp: I see.

Dr. Lederberg: And the fact that we can't really think even in terms of the conventional kind of distribution of economic and industrial effort that we are talking about now. Now I say this hoping that someone has a way of refuting it. I don't see any way out of it. I just don't see how, with the availability of these kinds of life support systems, people can fail to put whatever resources they have into them. But this has not been foreseen by Detroit. It hasn't been forseen by California technology. It hasn't been foreseen by the Congress.