

Seymour S. Kety Oral History Interview – JFK#1, 05/02/1968
Administrative Information

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Biographical Note

Seymour S. Kety (1915-2000) was a member of the President's Panel on Mental Retardation. This interview focuses on Kety's time on the panel and the panel's difficulties with coming to a consensus on recommendations, among other topics.

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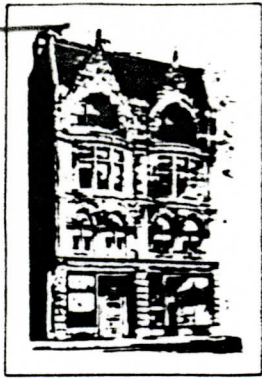
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Oral History Interview

with

^{So}
SEYMOUR KETY

Boston, Massachusetts

May 2, 1968

By John F. Stewart

For the John F. Kennedy Library

STEWART: You say that you weren't involved to any great extent in mental retardation before the Kennedy Administration?

KETY: Not specifically. I am a physiologist by training. *[President's Panel on Mental Retardation]*
But ten years before the panel was organized I had come down to the National Institutes of Mental Health as its first scientific director. At that time the intramural research program of both institutes, mental health and neurology, were combined and I was the director of that program and had organized it from the beginning. My

interests were widespread, in essentially the nervous system and in behavior, but with the long term goal of making contributions to the various mental and neurological diseases, of which mental retardation was one. But I had not worked specifically in the field of mental retardation.

STEWART: Because one of the things that I think is of interest from an historical point of view is the situation in January of 1961 when the Kennedy Administration took office. For example, the criticisms that were being made of NIH's [NATIONAL Institutes of Health] effort in this whole area, were there any criticisms of the direction that they were taking researchwise?

always
KETY: There has/always been criticism of the NIH.

STEWART: But I mean as far mental retardation.

KETY: The criticism in the field of mental retardation of the NIH's efforts were similar to criticisms which could be leveled, not necessarily appropriately, by any of the groups which felt that more conspicuously

relevant programs were called for. Now, ⁷I at the time that the panel was organized, I welcomed my invitation to serve on it because I always feel that a recognition on the part of the public of the contributions which could be forthcoming from research is welcome. And I didn't necessarily agree that with the funds which had been made available to the NIMH that the research effort in the long term directed to mental retardation had neglected that field anymore than it had neglected schizophrenia or depression or senility. But that if the public were to be aware of the problem of mental retardation and were to in recognizing this, were to make available larger funds, I felt that these could very appropriately be used. But I did not share in the criticism of the NIMH as having neglected mental retardation.

STEWART: Then exactly, do you recall, how were you chosen, or through what processes were you chosen to be a member of the panel? Do you know who selected you?

KETY: I think you'd really have to ask [Robert E.] Bob Cooke and Eunice [Kennedy] Shriver⁶ and Leonard Mayo. I suspect that it was because of the role that I had played at the NIMH in organizing the scientific program there¹ and that I had a general reputation in the field as a basic scientist who was¹ nevertheless¹ concerned with important problems and one who didn't shy away from specific health problems. And I suppose another factor which contributed was the fact that at the time that I was asked to serve I had left the NIH and was the ^{Henry Phipps} professor of psychiatry at Johns Hopkins. Bob Cooke was a colleg^aue of mine at Hopkins¹ and I believe he appreciated the fact that I represented a scientific, biological approach to psychiatry. He probably felt that that kind of approach would be valuable on the panel.

STEWART: Did you feel at the start of this thing, you mentioned the increased public awareness that would undoubtedly come from such a study, did you

feel this to be the major goal or the major objective of the panel, rather than any significant new ideas as to organizing research or as to organizing services?

KETY: Well, my convictions are that one can't successfully marshal research or organize research on a particular target. However, one can increase the awareness of scientists and of the public of the importance of a problem; one can foster and stimulate thinking of scientists about a problem; and since there is no answer to the amount of money which is appropriate for health research, the only answer is ^{how much} ~~what~~ the public feels it wants to spend. That is, there is no scientific answer. If one were to have ten times as much money, it could be wisely spent. If one had complete freedom, that is, spent in support of available projects, but also in the training and the recruitment of people who would then come in with more projects. I suppose the only limit is the number of talented individuals in the country. But I don't think we've

come close to tapping that. So that it's entirely, at this stage of the game, how much the public feels health research is necessary. And² therefore, I welcome any process which will make the public aware of the importance of a problem and the possible value of research in that problem. On the other hand, I view with a certain amount of concern the attitude on the part usually of people who are not themselves scientists and have not had the personal experience with the processes by which scientific discoveries are made. The attitude that crash programs are the way to get at things. So that I welcomed the panel¹ at the same time I had a slight feeling that a crash program might emerge from it¹, but² nevertheless² I felt that my place was still on the panel to discuss these issues and contribute to their solution.

STEWART: Did this idea of a crash program, was this substantiated as you went along and as you talked to

more and more people from the federal government?

MEY: Yes. I think it became increasingly clear that my initial concerns were valid ones. The panel-- and I should really speak largely for the research task force--included two very different groups of people with entirely different motivations. One the one hand, there were a number of people who had been eminent in the field of mental retardation per se, whose whole careers had been tied up with mental retardation, largely in the psychological, pedagogic, and training areas, and many of these very esteemed individuals. They knew a lot about mental retardation, and they also were aware of the neglect of the public of mental retardation, and also felt that there had been a neglect of mental retardation by scientists. ^{It} The other group which was on the task force on research consisted of very distinguished basic scientists who had not specifically ^{addressed} ~~adjusted~~ themselves to the problem of mental retardation. And this included Joshua

Lederberg and Wendell Stanley, both Nobel laure^{as}ts;
[Hon^{Wb}ce^{Tid} Magoun, an outstanding neurophysiologist;
Oliver Lowry, an outstanding neurochemist; and
myself. This represents these two poles on the
research committee, and I think whatever differences
of opinion we engaged in and had to eventually
reconcile, represented these two divergent points
of view which to me had been quite obvious at
the moment that I was asked to serve, ^{-- that is --} ~~As~~ I'd had
enough experience with this very delicate problem
of trying to do the best research that one could
which one was convinced would contribute most
effectively and most practically to the solution
of ^{health} ~~our~~ problems, but still having failed, as the
scientific community has failed, to explain to
the public the rationale of that approach. And the
inapplicability of target research. ^{It} So from the
outset, our task force started with two quite different
goals in mind. I believe the people who were
committed to mental retardation as a career saw the

issue largely as mounting a huge crash program. Those of us who were more basically oriented scientists saw the problem as focusing more public attention on mental retardation, the possible providing of more resources and funds, but their utilization in a way that we ^{saw} ~~thought~~ would be the wisest way to ^{spend} those funds. And so there was a constant difference of opinion on crash program versus just more unrestricted research with a focusing of attention upon mental retardation as an area of public concern.

STEWART: The panel held its first meeting, as you may recall, on October 18 at the White House with a good deal of fanfare as to what was going to be done and so forth. Do you recall this meeting, were you . . .


KETY: I recall this meeting very well. And there we had a chat with President Kennedy in the Rose Garden, I believe. We had our first meeting together, and I think it was at that first meeting that we broke up into task forces, or that may have come at

another meeting which ^{occurred} ~~took place~~ shortly afterwards.

STEWART: I think that was later.

KETY:q Yes. The first division of the group was in a manner which worried me. Someone had the idea-- and I'm not so sure that I know who it was-- that ^{one} ~~we~~ could best achieve the goals of the panel by dividing people up not by their primary field of expertise, and motivation, ^{by} but mixing everybody together. So that in that shuffle I was asked to serve as a member or even as chairman of the task force on service. And this seemed so completely inappropriate to me, not only did I know nothing about service, ^{but} but I didn't feel that I could make any contribution. ^{It} On the other hand, my whole career had been spent in research, and it just seemed an inappropriate use of whatever abilities I had for me to serve on a committee which was quite outside my own field of competence. I called Leonard Mayo and pointed this out to him, and I think he appreciated this. Perhaps other

people had also called. But after that we were reshuffled in a more logical way and then I became chairman of the task force on research.

STEWART: As I understand this was a method or a technique just to get something going, and presumably these two groups, the research and the services, would ^{decide} ~~side~~ on the ultimate breakdown of the panel. But you don't feel it really worked out? 

KETY: Well, it didn't last very long, that is, the initial random shuffling. Because we had only one meeting like that, and then we were ordered more according to our own competences and disciplines, which ^{just} ~~it~~ seemed to make more sense.

STEWART: On the other hand, did you pick up much, do you recall, from the group that was working on research from the--I think Dr. [Edward] Davens from the state of Maryland was the head of that group. Do you recall picking up much that they had done when you finally got into the research task force?

KETY: There were really two research task forces; one

on biological research and one on behavioral research. And I believe we met separately for most of the time. But at the very end we were asked to pool our reports into a single chapter on research, and it was there that we had most of the confrontation and the divergence of ideas and the working through ^{of} some of these differences. But I don't recall, in our first session where we were shuffled up randomly, I think there we were largely breaking the ice, each of us ventilating some of the reasons why he was happy to serve on the panel. ^{It} I didn't recall that at that meeting any substantive information came about. But as an indication ^{of} ~~that~~ my attitudes, at that first meeting I did write a statement of my philosophy with regard to this which I think indicated the kind of ^{concerns} ~~things~~ that I've been telling you about. That statement never actually became part of the report, but I know that it represented at least the opinion of

a number of us on the research panel. If I could-- just let me read this to you. It's simply two paragraphs, but I think it puts into a nutshell what was one position in the research panel.

7 "An analysis of the process by which research has in the past made discoveries resulting in the prolongation of life or the ^Lalleviation of suffering or handicaps, reveals a highly consistent pattern involving certain essential ingredients. These include the acquisition of knowledge concerning the clinical problem which permits the posing of specific meaningful questions; findings, often in fields removed from the clinical problem but which relate to it, sometimes by design but more often by chance; creative minds appropriately trained and motivated to perceive and exploit the relationship, and a vast fund of knowledge in many areas which was the source of that training and will provide the tools and materials to make that exploration possible; the ^Ninnumerable men through whose

work that fund of knowledge was acquired,[^] and the means whereby information is freely and reliably communicated at every stage of this process.^u

"To facilitate that process and to accelerate the acquisition of information leading to the diagnosis, prevention, and treatment of mental retardation, it is not enough to encourage and strengthen those components which appear to be most relevant to the goal. Where the resources are marginal, as they are today, a substantial emphasis on one aspect of the system will be at the expense of others equally essential."

Well, I think the other point of view might perhaps be represented by a statement~~which~~ appeared in one of the preliminary drafts from the behavioral science group which we discussed at our^{first} effort^{to} ~~merge~~^{meld} these two reports together, and which eventually did not become part of the report. And this report started with a great feeling of hope and encouragement that finally there was this recognition by the

President, and undoubtedly by the Congress and the public as a result of that, of this neglect^{ed field} of ~~the~~ mental retardation, and that now it would be possible to mount a crash program to attack the problem researchwise. ^{It} And as an example of this, there was the old example, which has time and again been used inappropriately in this area, the Manhattan project or putting a man on the moon as the example of how a crash program can achieve results quickly and effectively. Of course, the inappropriateness of that model for biological science is that in both of these instances one was dealing with a simple engineering feat. The basic knowledge had been acquired, and it was simply a question of ~~their~~ there is no major new concept which is required to put a man on the moon; ^{it} it is simply a more effective and efficient utilization of the knowledge which we have; ^{it} it doesn't require developing a new law of gravity or any new concept of acceleration;

It's just getting the power and getting the thrust
and taking care of the environmentⁿ during the travel,
and soft landing, and so on and so forth. That's
quite different ~~in~~^a medical problem or a problem
that has to do with human behavior, like cancer,
like schizophrenia, like mental retardationⁿ, where
we don't know how the answers are going to come
out. There is no way of planning the attack because
we don't even know what questions to askⁿ in some
ways. Well, we succeeded as a result of the ^{is} dis-
cussionⁿ. We did ~~succeed~~^c in having the crash program
emphasis removed from our combined report. But
as a matter of fact, what really emerged was a
chapter that simply stated two quite different
points of view. There wasn't really a coming
together. I can't say that that was inappropriate,
as a matter of factⁿ. As long as we were able to
agree on an overall philosophy which did not run
counter to either side's fundamental philosophy.
There ^{are} ~~were~~ certainly two problems here. One ^{is} the

basic research which is required, and the other is the application of research which we already have to the problem. And I think that our chapter, the research chapter, emphasizes these two areas. They don't necessarily have to coincide in every way. On the one hand, when one is looking for knowledge, one is looking for new facts; on the other hand, one is seeking to apply the facts we already have. And they're not necessarily contradictory.

STEWART: In the meetings you did have with President Kennedy and other meetings with Mrs. Shriver, and possibly Leonard Mayo, did you get the impression that they fully understood these problems? And what was their reaction, do you recall, to your point of view and the point of view of others who were . . .

BETTY: Oh, I don't think that our point of view ever prevailed. I think that the report itself, important as it was, and even more significant the activities following the report, which the report presumably

stimulated, were selective in some way so that the program which emerged, the program which Congress then proceeded to fund and support, was--well, as a matter of fact, it was a compromise. It was not a crash program, but neither was it simply the funding of more basic research without any kind of direction or coordination. So in a way ^I suppose ^{our} point of view ^{did} prevail to some extent in modifying what emerged.

STEWART: But you don't recall getting any real indication as to the President's knowledge of the ^{is} whole situation or his feelings about it? I suppose there were only two or three meetings ⁱⁿ ~~at~~ which you met the President, ^{were} ~~weren't~~ there?

KETY: We met the President in a number of meetings, three meetings. The last time we met him was a most impressive time. This was the time, I believe, at which we submitted the report and at which we also had returned from various missions abroad.

STEWART: I wanted to ask you about ^{that}

KETY: And I had been chairman of the group which went to Russia. And the President was very much interested in our reaction to ~~the~~ what we saw in Russia, and I remember speaking ^{at} ~~in~~ considerable length at that meeting about what we saw in Russia. What impressed me ^{retrospectively} ~~of~~ course, what impressed me at the time was ~~his~~ the remarkable ability he had to comprehend a subject that he wasn't engrossed in in a daily sort of way, the quickness with which he comprehended these things, and ^{his} ~~the~~ ability to penetrate to the roots of the problem, and so forth. Later I learned that that meeting was held at the time of the Cuban ~~crisis~~ before we on the panel knew what a tremendous load of responsibility and concern was on his shoulders. The remarkable thing was how he ~~nevertheless~~ engaged in a remarkable, insightful dialogue with us on the subject. ^{Now} I ~~frankly~~ don't know how the President felt about this issue of crash programs,

target research. As an intelligent layman, but as a layman, nevertheless, I would have thought it would have been almost miraculous if he would have agreed with those of us who felt that crash programs were not the way one does the best and the most productive and the most practical research in the medical sciences. It's most unusual for a layman to come to that conclusion on his own, and he certainly would have had much pressure from many other sources against ^{that} ~~us~~. Of course, I would have had equally the conviction that if one could have discussed the problem with the President at great length, ~~that~~ one could have convinced him of the wisdom of that particular approach. Just as I had^{ve} the feeling that if one had the opportunity to discuss this at great length with the Congress, one could justify this kind of approach. Because this is not a question of self gratification of scientists, or not a question of asking the public to support the curiosity of

scientists. It's a conviction that the best way for the public to get what it so desperately wants and deserves is by such a process as opposed^{to} frittering away the ^ufunds on what looks like relevant research. However, I think that to some extent the President's attitudes were. . . . No. To those of us on the committee, the only indication which we had of the President's attitudes were the responses from the staff at the White House, [Myer] Mike Feldman, primarily, whom I knew many years before because we had both grown up in Philadelphia and he was at the Law School of the University of Pennsylvania while I was at the Medical School. And so we had had some acquaintanceship before this. I don't think we ever quite got across to Mike the validity of this point of view, and I'm not sure that I know why that was. But there did seem to be a tendency on his part to mold the report along the lines of certain preconceived

notions. ^{It} I remember being a little annoyed at this at our very last meeting with him. When the final draft was submitted, we had a meeting at one of the hotels near the Capitol, and Mike Feldman was there and some other people, but Mike essentially to represent the White House. I believe this was one of his delegated responsibilities, this particular panel. And Mike had made some changes in our draft. And I remember being sufficiently concerned about this to have indicated at the meeting that I was somewhat surprised, that it was the committee's report, and ^{the} a committee should submit its report. The White House didn't have to accept the report, but that somehow for the White House to edit the report before it was submitted seemed to me surprising. Perhaps I was naive and perhaps the White House usually edits reports of its advisors before they're submitted. ^{It} But these weren't terribly serious,

and I think we finally prevailed and the report as finally submitted--there were many telephone conversations between Leonard Mayo and me and I ^{as} ~~presume~~ between Leonard Mayo and other task force chairmen until we finally got the final version into a form which was acceptable to everybody. It certainly would not be correct to say that ^{what} /was finally submitted did not have our concurrence it did. But it was arrived at by this particular process.

9 But, again, it wouldn't be surprising if Mike or Mrs. Shriver or the President felt that the way to tackle this problem was with a highly organized, mission-oriented program. This would be what almost any intelligent layman, especially one who is very strongly motivated, feels about it. It takes a long time to appreciate that these scientists who are apparently boondoggling on something that

apparently I can't understand is nevertheless

doing the thing which is eventually going to
solve the problem for ^{us} ~~him~~.

STEWART: Going back, you, I assume, were originally
assigned to head the task force, well, in
December or January. Do you recall what your
feelings were, and generally how you ^{proceeded} ~~proceeded~~
to organize the task force on biological
research?

KETY: Well, we would have meetings of the task force
at which we would discuss what was needed in
the way of biological research in this broad
field of mental retardation, what were the
problems, how could one come to grips with
them? Here we had relatively less dissension
because we were more of a homogeneous group,
and so we never really had to argue the issue
of crash program versus anything. It was as
if we met with the assumption that ^{as well as} ~~while~~ a
crash program was not indicated, but what was
indicated in the field? And so we proceeded

to meet and to present our own ideas of what the problems in the field were. What could be done, what could the public do, what could the President and the Congress do to facilitate the solution or the prevention of this problem.[?] And so in these meetings we were debating, discussing these things which eventually became part of our report, except for those parts which dropped ~~off~~ by the wayside. But I believe we emphasized the manpower needs in the field; and again, from our point of view, it wasn't enough just to train people to do mental retardation research because one was not going to get the best people in the country to say, "I'm going to do mental retardation research." Scientists just don't do that. Scientists work on a substantive problem of knowledge, the acquisition of knowledge, not on a goal. And we laid that out. We also indicated that there were needs for laboratories

and facilities. We pointed out quite properly, that although there had been a large expenditure of funds by the NIH, much of this research was being done in basement laboratories, wholly inadequate, and what a small proportion of the total research expenditure represented what would be represented by the building of adequate research facilities.

And then we discussed some of the substantive issues of what research could one stimulate that would contribute to mental retardation. And here we ran into some differences of opinion, because Josh Lederberg, I suppose, represented the most, the strongest position that one can't organize research, that one can't marshal it or direct it, and even for a committee to suggest that these are areas that are important for future research to him smacked enough of a national program that it ^would be said to be contributing to that.

STEWART:

^{is} That was a point I wanted to ask you about. Did

I don't see anything wrong with a committee of experts sitting down and speculating about, "well, you know, given a million dollars, ^{in?"} -- What basic area of research do you put this ¹ ~~Somebody~~ has to make decisions of that very broad nature. ^{And} But I have no objection to that. But I think Lederberg was correct that if we had laid out a program, if we, ^{the} members of the President's panel, had laid out a program in that report, ¹ it could have very easily become the national program, the crash program for marshalling research toward the. . . . And since the Congress was in a position to select which of the recommendations it would support and which it wouldn't, anything which we would ^{would} have/have made it possible to have unwittingly laid out a national crash program. ^{It} I don't think ^{at} at worst, ¹ it would not have been a crash program. It would simply have been a suggestion of what areas of basic research would eventually ¹

in our opinion, contribute to mental retardation.

So we spent some time on that. In fact, we even

drew up drafts of such programs. I remember

Tid wrote one -- oh, Tid
Hugh McGoun, ~~or~~ Hugh McGoun was in the behavioral

sciences group as a matter of fact. He was not

a member of the biological group, although he

was the man who certainly crossed the line between *them*

since he is an ~~noted~~ eminent neurophysiologist but

also interested very ~~well~~ much in behavioral concept.

And I wrote a rather ~~extensive~~ outline of the

kinds of basic research which could be stimulated

and which, in my opinion, would eventually contribute *to*

understanding more about intellectual development.

It These never became part of our final report. For

one thing, we heard one time that President Kennedy

read these programs and didn't understand, they

were quite technical, and didn't understand them.

And he may have indicated that he really didn't

see the validity of including such technical

material in ~~the~~ a. . . . And I think this was probably

research submitted a report. And when it was decided that both of these reports would have to be somehow combined into a single report on research.

STEWART: Why, because there were ^pduplications or conflicts?

KETY: I don't know. It may have been felt that it would simply emphasize the isolationism of the field if one had a separate report on biological research and a separate report on behavioral research, and that something was to be gained by pooling them. It was there that we had many discussions between the members of the two task forces, and much working late at night and revising and revamping, and so on, on the part of just a couple of us. I remember being down at the HEW [Department of Health, Education, and Welfare] until 3 o'clock in the morning a couple of nights trying to straighten out some of these problems. Now, at one point Joshs ^rLederberg got sufficiently disturbed by all of this that he ⁱresigned from the panel. I'm not sure what it was although I have the distinct feeling

it was an unwillingness to accept this crash program kind of orientation. I finally prevailed on him--I think he did literally submit a letter of resignation to Leonard Mayo, not to the President--and I prevailed on him to stay with us on the ~~XXXXXX~~ ^{usual} argument, you know, "You're more effective if you're a member than if you leave." And also on the assurances by Leonard Mayo that his point of view, which was fortunately my point of view and a number of the others of us, would somehow be given sufficient prominence ~~in~~ ^{could...} the report that ^{it} he would write the introduction to our chapter, which in fact he did. ^{And} the introduction to the ~~chapter~~ on research is a very fine document. It's similar to the kind of thing that I was reading here before which I had written, but I ~~think~~ Lederberg did an even better job of laying out the philosophy of the problem. The interesting thing is that having made that statement, ^{that} the rest of our chapter doesn't fully support

it because we end up with many highly targeted kinds of projects. But I can reconcile that since I think a wise national program would emphasize basic, unrestricted research, but also would provide sufficient funds ^{it} (so) that ~~you~~ could also take care of the kinds of targeted, engineering, demonstration kind of research which was warranted ^a at the time. ^f And it wasn't what we felt that kind of research was inappropriate; ^{it} it was somehow that we had the feeling that that kind of research would get supported at the expense of ^{the} basic research. Bad money drives out good or something, and somehow I think there is a general tendency on the part of the government which doesn't ^{see} ~~seize~~ this issue. At one time it may be very generous and provide enough funds to support everything. But when suddenly there's a constriction ^{of} on funds, inevitably the thing that gets constricted most is the stuff that the Congress doesn't quite understand. I think

and experiential enrichment and so on, will solve much more of the problem of mental retardation than the discovery of a new enzyme. As a matter of fact, I remember once at a meeting of the whole panel making that point. And this apparently impressed a number of people that a biologist would make the point that sociology and psychology, utilization of ^{the} knowledge that we already had in those areas, could prevent more mental retardation than studies on enzymes, which, of course, I believe.

STEWART: Then to your knowledge there was no serious concern by other people that the report would emphasize these things to the detriment of the biological causes of mental retardation.

KETY: I was not aware of any concern on the part of the biological task force. No one ever made that point to me. Of course, it wasn't part of our chapter, I don't believe. And so at first we were concerned primarily with getting a chapter that we could accept and iron out. And then, of course, the

whole report. Now, if anyone had any reservations about this, they would have communicated that to Leonard Mayo, not to me. But, no, I was not aware of any feeling, and I doubt that Wendell Stanley or Oliver Lowry, even though they are very outstanding molecular biologists, would have felt that these other factors were unimportant. And knowing Lederberg as I do, I would ^{have} imagined that he would have positively supported ^{that} this position, the position that I would ~~have~~ positively have supported too in this area.

STEWART: I can't recall where I read this or heard it, but I had assumed that there was some problem in that many people felt that the case for mental retardation being caused in many, ^{in a} you know very high percentage of cases by these cultural factors hadn't really been proven. That it was more a matter of not being able to find the biological causes, therefore concluding that these cultural things weren't . . .

KETY: Right. Well, I know that this is a point of view.

7 And Russia seemed to be the natural for the group that was interested in research because it represented, first of all, a country which was doing a lot of research on mental retardation presumably. They even had an institute of defectology. And also it represented perhaps a different kind of approach to bio-medical research, so that I suppose a number of us welcomed the opportunity to visit Russia, not only because of the relevance of this to the panel, but also because, I suppose with one or two exceptions, we hadn't visited Russia ^{before}. I think it was very worthwhile, and I've used the information that I gathered on that trip further to support my bias, if you will, about the impracticality of targeted research. The Russians are at least up to us on space science, which as I indicated before is an engineering field and one can marshal ^{are} and target. They/woefully behind us in bio-medical sciences. And we could see there

an example of targeted research where there
 is a five year plan, a national plan, ^{--the} kind of
 thing that ^{Jost Lederberg was} ~~Russia~~, of course, ~~is~~ very much
 worried about, [^] and where the different labora-
 tories and the different ~~scientists~~ are doing
 something which is appropriate and which is
 specified in that five year plan. ⁷ Now, of
 course, individual scientists assured us
 privately that it isn't all as regimented as
 that, that they still have a great deal of
 freedom. But yet they don't have all that
 freedom. As a matter of fact, one of the Russian ^{scientist}
 visited my laboratory ^{at} ~~in~~ Bethesda when I was there,
 and ~~when~~ he asked me to explain how I direct the
 laboratory. And he just couldn't comprehend
^{that a} ~~the~~ laboratory couldn't be directed that way.
 I told him ^{the} way I direct it is to recruit the
 best minds that I can who are working in the
 general area of neuro^obiology, neurochemistry,
 neurophysiology, and then leave them alone, ^o sort

of organize seminars and discuss them, exercise a certain amount of intellectual stimulation." But he said, "Do you mean these people are not assigned tasks?" "The only tasks they are assigned is to do good research." Well, he just couldn't comprehend that. Well, anyhow our visit to Russia confirmed the preconception I had that target research was not going to be the answer to bio-medical problems. Because we came out with the feeling that they were at least ten years behind America in terms of basic and clinical medicine.

STEWART: Didn't you run into some problems there in talking to the people who were in this area?

KETY: Well, we ran into the usual problems of organizing an itinerary, and changing an itinerary in Russia. This apparently is a major issue. I'm not sure that I understand why. But to some extent we contributed to this. We were always changing our plans ^{at} ~~on~~ this side ^{of the fence} ~~or that~~. And our State

Department was dragging its heels just as much as the Russian office was dragging its heels in terms of coming up with an agreed upon itinerary. And then ~~when~~ we finally did get the itinerary, some of us wanted to change it all over the place. And the Russians resist change. ~~It~~ Now, one reason for this could simply be courtesy to scientists. I know I would resent it if I were told twenty-four hours in advance that I was going to have a group from the USSR visit me and ask me to change my schedule. I expect to have a certain amount of warning that something like this is about to come about. ~~It~~ On the other hand, it may also be because they need clearances for these itineraries, and so on. We ran into some difficulty ^{there} not a great deal. Not ^{at} ~~on~~ a personal ^{levels} but only at the bureaucratic level, that is. The scientists we saw were not rigid in terms of being willing to see us ~~or~~ or allowing us to change things around.

The other problem that you may be referring to was a distinct feeling that there was a strong doctrinaire overlay in the research programs that we saw. As they got closer and closer to pedagogy, to mental retardation, and somehow to interaction with the Marx^{ist} dialectic materialism, they became much more doctrinaire. And this one has noticed about Russia in general ^{is} that a metallurgist doesn't have to subscribe to anything that Lenin said especially. But someone who deals with human behavior ^{is} his philosophy is pretty well outlined for him in Marx and Lenin and Pavlov. And so we saw a definite tendency for people to believe not what their data showed them, but what was good for the state. And the attitudes about mental retardation in general were unrealistic. You know, unrealistic even in terms of communist doctrine. ^{It} The silly thing about this is when a scientist decides that ~~the~~ truth is something that can be determined by doctrine, he's usually not

even wise enough to realize the naivete of that position, because, for reasons which are not quite clear to me, genetics were taboo at the time. . . .

Well, it had to do with the ^{[Trofim O.] Lysenko} business, and

I suppose for some reason or other a communist society has to believe that environment ^{is} is overall, overriding. I'm not sure that I know why.

So genetic factors are unimportant, and, furthermore,

since everyone is born equal and our differences are only in terms of our environment, I suppose

genetic factors ^r can't operate there. ^{if} So, therefore,

genetic factors couldn't play a role in mental retardation, or in intelligence, except that they recognize that there are certain genetic diseases

like ^{phenylketonuria} like ¹. Well, that was sort of

different, they were diseases, ^{and} and they weren't

retardation, and they weren't intelligence. The

intelligence ~~these~~ tests ^{is} ~~this~~ taboo because this

is a means of grading people, and since everybody

is equal and they ^{are} ~~have~~ no differences in ^{innate} potential,

^ethan any attempt to try to measure this thing
that doesn't exist is taboo. And yet they use
intelligence tests, except they don't call them
that. ^H And somehow even enviroⁿmental influences
couldn't account for mental retardation because
that would be a recognition that our enviroⁿment
wasn't ^sso hot after[/]all. So[^] therefore, what are
you left with in terms of explaining mental
retardation? ^H Well, the only thing that is
acceptable, the thing that affronts fewer of
the social doctrines, is the idea that mental
retardation is the result of some damage, not
genetic, but some damage to the nervous system.
So this is their concept of oligophrenia. They
would tell us that if you examine a mentally
retarded individual thouroughly enough, you will
find somewhere[^] even a very small area[^] wh[^]ere the^{ir}
function is not normal. Well, that's silly. You
can do that to anybody and find some--all of us
are not normal in every area. But that was the

thing that offended us more than anything, I suppose, about mental retardation, ^{about} the mental retardation effort there. But as we got farther and farther away from mental retardation into basic biochemical research, that of course wasn't very prominent. [&]Neurophysiology, interestingly enough, did show quite a spectrum. There was the typical representative of the old guard in neurophysiology, ^{who} who was usually the director of a particular institute, who usually had a beard, didn't speak English, and would speak in general terms which were very, very reminiscent ^{sc} of Pavlov. And ^{it} ~~he~~ seemed as if he had not really been moving with the advances of neurophysiology. But then there was a new group of neurophysiologists some of whom had even been abroad and worked in the West for a year or two with some outstanding neurophysiologist in England or America. And these men had quite a different perception. I remember, especially, one instance where one of these younger

men were translating for what the older man was telling us. And when the older man would make one of these cliches, the young man said, "When they say higher nervous activity, they mean such and such." Clearly divorcing himself from that position. The work of the young men was reasonably good, it was still was not as good as the best work in this country, but it was as good as an average neurophysiological laboratory. So that neurophysiology is beginning to emerge from this.

Chemistry has already done so.

BEGIN SIDE II TAPE I

STEWART: You mentioned earlier that President Kennedy had asked you about the trip to the Soviet Union, or at least you had reported on it at this last meeting.

KETY: Well, he was obviously interested and asked me dilate on it.

STEWART: Do you recall any of his questions or any impressions of what his major concern, or ^{his} major interest was?

KETY: I've thought about that since, and I've thought about *it*

when I realized

^ the stress that he was operating under at the times. And I was unable to draw any special significance from the questions he asked. He asked the questions with his usual insight into important areas, but I can't say that I could even reconstruct a hypothesis that would indicate a special concern about certain areas.

STEWART: During the year that the panel was in existence, the NIH created, or the Administration created, the National Institute of Child Health and Human Development. Do you recall being involved at all in the decision to create this, or being in favor of it or opposed to it?

KETY: I believe it was one of the recommendations of our report, *wasn't it?*

STEWART: As I understand, it was sort of an afterthought. The new institute had actually been created *in* as I say, while the panel was in existence. And in the report the panel endorses the creation of it.

KETY: I see. Well, probably what happened, as often happens, is that Congress passes enabling legislation, but doesn't come up with ~~the~~ ^{the} funds in as large ~~an~~ amount as might be required. Yes, I'm strongly in favor of the Child Research and Development Institute, just ^{as} I believe ^{that} the categorical institutes at the NIH represent my concession to targeted research. I think that's an appropriate kind of targeting. It does highlight certain areas of research and certainly pediatric research had tended to fall between ^t schools before that. And so an insititute concerned with the problems of development in childhood was much called for. So that I strongly endorsed that, and I believe that Bob Cooke, whom I admire in many ways, was very instrumental in doing this, in carrying the ball for such an institute.

STEWART: There was opposition within NIH, I believe, to it. Well, as always ^{to} ~~with~~ the creation of any additional institute.

There always is.
KETY: [^] That's right. Because any new institute, to some extent, overlaps the functions and the empire ^{of} existing institutes, so I suppose there would be a natural resentment at that. But if one can divorce oneself from that and look at the national picture, I think in general it was a very good thing.

STEWART: Did you have any contacts ^{or} any prolonged dealings with people outside of the panel on the work of the panel? Was there, for example, anyone at NIH whose attitudes you sought on the report?

KETY: Well, in the first place, I didn't think it was my function to seek advice ^{via} ~~by~~ the NIH. In a way, this was an outside panel which was examining the functions of the NIH as well as. . . . In a way, it was stepping into the province of the NIH which the NIH had under its jurisdiction, and therefore, I don't recall specifically seeking advice from anyone. I think ^I had a conversation with ^[To me - A.] Shannon at one point. But I've had so many conversations

with Shannon and was having them at that time that it's hard for me to remember whether it was specifically asking his advice about the report.

STEWART: Did you people have hearings_Δ as I think some of the other task forces did_Δ in various parts of the country?

NETY: No, instead of that, we had one or two brainstorming sessions. We agreed that although it may not have been proper for us to lay down a national program, it would be proper and appropriate to stimulate discussion among scientists on problems related to mental retardation. And one of Josh Lederberg's very strong pleas₇--which I think became a recommendation in the final report_Δ but it may have dropped out before that; in any case, I don't think it was ever adopted in the form in which he presented it--was that there be a number of very high level conferences of the highest kind of scientific excellence, sponsored by the White House_Δ in which

the President would invite a number of scientists to come together to the White House and to have the ultimate in a high grade scientific discussion on a problem which was clearly related to mental retardation, even though it might not appear to be. That never got adopted. But I think in the report there was a recommendation that conferences, scientific conferences of various kinds, be stimulated. But that's not a novel idea. In any case, we did feel that it would be useful in our thinking ^{and} to the field if we had some brain-sobbing ^s sessions with very bright and competent scientists. So we had one session at Madison, Wisconsin, where we called together some of the outstanding representatives of cellular genetics and molecular biology. And we had a free wheeling session in which people just freely associated in terms of what they thought the problem required in terms of scientific effort, ⁻ Not in terms of finances or anything, but in terms of ideas and

concepts. That was what you might say was our hearing, but we didn't have any real hearings.

STEWART: There was a certain amount of criticism I think that the panel was only given a year in which to do its job. Do you think if the panel had gone on for two or even three years, it could have produced more, it could have produced something of even greater value? Or would it have merely prolonged all of the problems that. . . .

KETY: I don't think so. I think that the value of the panel was that it focused national attention^{up} on mental retardation[^], that it served as a basis for more support~~ing~~ more thinking in the government on the part of scientists, on the~~part~~ of the public[^], that this was a problem[^], that it made more funds available to the field. And I think one of the major contributions of the report was this emphasis upon the environmental aspects of intellectual maturation. And I think[^] really[^] that this contributed considerably to our present awareness

of these problems of the ghetto and the under-privileged, and to the anti-poverty program for that matter. I can't be sure that the anti-poverty program wouldn't have come about anyhow. But I'm sure that this emphasis must have contributed to some extent and ^{as a matter of} in fact, may even have sparked the idea in President Kennedy's mind. Because I believe the anti-poverty idea was really developed in Kennedy's Administration, was it not? And it may have been that this somehow fed into that concept.

STEWART: At least the Headstart program. People have fairly definitely put the genesis of this in the President's panel.

KETY: Yes. But I could even see how it could have been ^{even} wider spread. Because if you admit that environmental factors are a major cause of a significant segment of mental retardation, then when you begin to examine what you can do about it, it's not just enough to hand out some vitamin pills. After you begin to

think about it, ^{that} you realize this problem is
inextricably ~~inextricably~~ mixed with the whole problem of
poverty and underprivileged. So that I think
it would be a natural, logical step to go into
the whole problem of poverty from that. But
I have no reason to think that that was the
case. President Kennedy was wise enough and
had wise enough advisors that this could have
come about through entirely different directions.

STEWART: The idea for the establishment of the research
centers at universities and pinning it down to
the establishment of ten of these university
^{affiliated} ~~created~~ research centers, did this come out of
your group or was this something that had been
in the works, in the minds of many people before
that?

KETY: Oh, I dare say it had been in the minds of people,
but I think this definitely came out of our task
force. I remember Oliver Lowry, who was assigned
^{the} ~~A~~ task by the task force of writing up a draft on

the the needs for resources, for physical resources and centers and so forth. It was ^aMcGoun in the behavioral sciences panel who came up with a definitive idea of ten centers, and so/and so forth. But this ^{is} not a completely revolutionary idea, and I'm sure that other people must have thought of it.

STEWART: But ^{this} was . . .

KETY: I think it can be traced to the workings of the two task forces in research, and to some extent, I think, to ^{an} interaction between us. I'm not sure whether McGoun dreamed this up first and then Lowry was asked to fill in the gap, or whether Lowry was asked to highlight this area and then ^aMcGoun made a much more specific recommendation ^{out of it}.

I can't be sure.

STEWART: ^{That's just} ~~This is~~ about all the questions I have. I'm looking over a listing of the recommendations, and I'm not sure that--I had thought that there might be some profit in just going over them and seeing if

there was anything that you recall ~~of~~ being of particular controversy. But in looking at them, they're all so fairly general that it. . . .

KETY: I might say that the ones which emerged from the biological panel were relatively few and pretty general, like the research centers. As a matter of fact, it may turn out that not too many of our ideas became ~~expressed~~ encapsulated in recommendations. Most of these recommendations. . . .

Oh, scientific communication was ours, and these are these highly specialized international conferences. Manpower and trainingⁿ was ours. The training of medical students for research careers was definitely ours. In fact, I think I dreamed that one up myself. And this is still a problem, you know, the point that society recognized that the PhD[#] is crucial and therefore provides fellowships and so forth so that a bright man who wants to go into research can get his education and stipends and so on while he's getting his PhD[#].

But until very recently if a man was committed to a career in research, but chose to do that through the ~~MD~~^{PhD} rather than the ~~PhD~~^{MD} because he then became a medical student, there were no government stipends ~~partly~~^{specifically} available to medical students. That has changed. There are now some stipends available.

STEWART: Were considerations of manpower and things that obviously go much beyond mental retardation, was this one of the questions that people in the White House were raising? that why ^{does} ~~did~~ this panel get into these things?

KETY: Exactly. This was a major ~~source~~^{...} although it wasn't a major source of irritation, ~~we~~^{we} didn't have long harang^{ues} about it. I think this was a difference of opinion. And I remember once talking to Mike Feldman ^{to} ~~about~~ this point, my pointing out to him that you can't just focus on the top of the iceberg. If you want more people in mental retardation research, if you simply skim off the

cream, you're depriving some other areas⁶ because there are not enough of those people to go around. And what you really want to do is to support the base and increase the number of high school students going into science, and so on and so forth⁵. ^{and} At the same time motivate some to go into mental retardation. Well, we never came to a . . .

⁷ But Mike would say, "Well, that's important. But it's not your problem; that's the problem of education, or it's the problem of the whole NIH training program." But as far as mental retardation goes, of course, my point would be that you can't divorce these problems. It's like saying basic research is ^{body} someone else's concern, ^{and} all we're concerned about is research that has to do with specifically mental retardation. This was a fundamental difference in philosophy.

STEWART: But these things did get into the report?

KETY: Like the ¹⁰MD business, yes, yes. That's right.

But you see then, following that number ⁵five on
page ³three, there was immediately number ⁶six,
which provides specifically for research
specialists in the education of the mentally
retarded. In a way I suppose these were com-
promises where one of our recommendations would
make it and one of the others. And this ^{is} was
perfectly appropriate. I can't argue with
either one of these as being important. And
then this research careers in mental retardation,
in conjunction with training in one of the basic
behavioral ^{or} social sciences. Now, you see here,
again, you see the difference in the flavor.
Notice the people who were willing to ask
specifically for research careers in mental
retardation were obviously behavioral and
the social scientists. A biologist would never
have said that because I don't think we would
see that a competent biologist would want to have
a career in mental retardation as such. There

are people who have ended up that way and have made important contributions. But somehow you make a career in neurophysiology or neurochemistry and then later on you find that your work is so relevant to mental retardation that you continue in that direction.

STENART: Wasn't part of the problem the fear on the part of perhaps the associations in the field of mental retardation that the big play for mental retardation ^{would be -- that} a lot of people would jump on the bandwagon, so to speak, and obtain funds that just didn't have that much direct relevance?

KETY: Well, there were two attitudes. One attitude was expressed by a woman on the behavioral sciences panel who, since, unfortunately, died. I forget her name at the moment. But she once told me, while we were working on trying to thrash out some of our differences, told me that, "You know, the mental retardation field has waited so long for this, and this is such a wonderful opportunity

that we've got to take advantage of it." And the other attitude is a suspiciousness about scientists; ^{that} ~~The~~ scientist will take money under the guise of being interested in mental retardation and ^{then} fritter it away on something else. ^{Of} Of course, an honest scientist--of course, there are some dishonest scientists who will take money and then work on something that they know has nothing to do with mental retardation. On the other hand, an honest scientist may feel that the basic research which he is doing is the best way in which he can contribute to mental ^{re} retardation. And ~~some~~ layman looking at this may not recognize that. ^I When I was at the NIH, when I was scientific director, ~~I~~ the Congress would occasionally, in the early days, come up with special funds for research in schizophrenia, research in mental retardation. I remember one large appropriation that the Congress gave us for research in mental retardation.

This was before this panel, so that the NIH was not completely oblivious to the problem of mental retardation. And so out of that, I got a substantial increase in the budget, and I used that to increase the support in a number of basic areas where I thought the work was relevant. Now, one of these laboratories that got an increased support was the laboratory of *Cantoni* in which was working Seymour Kaufmann. Well, that paid off because Seymour Kaufmann, a few years later, made a very substantial contribution to the enzymatic mechanisms involved in *phenylketonuria*. I had no ambivalence at all about the question of whether I was being honest. None of that money was actually spent to study people with mental retardation. It went into basic research. But I honestly felt that that was the wisest way to spend that money if one were going to contribute most quickly to the problem of mental retardation.

Oh, on page 4, 8, and 9, still under research or manpower, I think came from our group, especially the one about the process of scientific creativity. It seems rather vague, and I. . . .

[Louis Mo]
Let me see, [^] Hellman was a member of our panel, an obstetrician. So I think that a number of recommendations on preventive measures probably emanated from him.

STEWART: Did you review the reports of the other task forces?

KETY: Only insofar as they were to be included in the whole report. We all of us reviewed the whole report, and accepted it, had the opportunity to accept it. But I didn't review ^{with} ~~what~~ any feeling that I was contributed ^{ing} to the writing of any of the others. Well, you see, the report is so specific and has so many nicely encapsulated specific recommendations, which of course one can do in the areas of service and residential care and vocational rehabilitation, and so on.

Whereas in the area of research, I think the contribution of the research panel to the research report reflect^{feeling} essentially, the ~~falling~~ of the members of the research panel itself. But one can't be very specific about how you go about solving this problem of research, except in terms of these broad generalizations, ⁺ more facilities, more research, more training of a broad base ^{of manpower}, and so forth.

STEWART: Okay, that's all the questions I have, unless there's anything you want to say in conclusion, or summary?

NETY: No, I really feel that I've said everything . . .