

AN INTERVIEW WITH DR. KENNETH M. ENDICOTT

BY STEPHEN P. STRICKLAND, PH.D.

ON THE OCCASION OF

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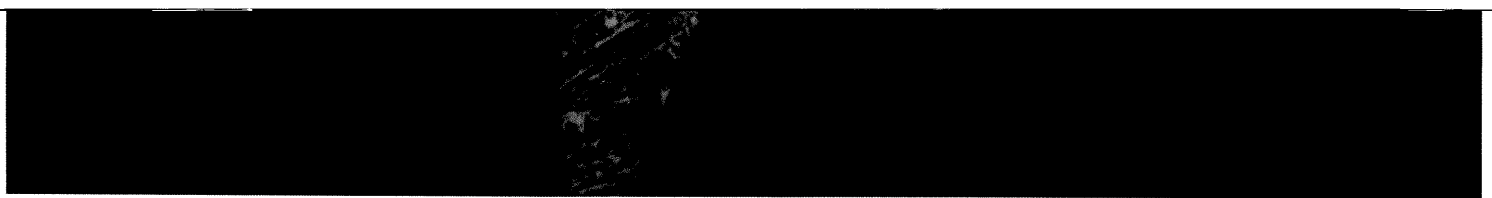
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Introduction and Biographical Sketch

This interview with Dr. Kenneth M. Endicott is one in a series of "oral histories" focusing primarily on the origins and development of the extramural programs -- most especially the grants programs -- of the National Institutes of Health, beginning with the establishment of the Division of Research Grants in 1946. Like Dr. Endicott, most of those interviewed had critical roles in the development of the extramural programs.

The grants program constituting the largest component of the NIH, the interviews also reflect judgments and perspectives about the impact of the grants programs on health and science.

Dr. Kenneth Endicott's 38 years of service in the U.S. Public Health Service, much of it at the National Institutes of Health and the National Cancer Institute, is one of the longest and most remarkable records in the federal health establishment. He began his career as an intern at the U.S. Marine Hospital in Seattle, Washington in 1939. During the second World War he worked both in Maritime Quarantine and at the Medical Center for Federal Prisoners in Springfield, Missouri. In 1942 he was appointed Chief of the Metabolic and Degenerative Diseases section in the Division of Pathology at the NIH. In 1951 he became Scientific Director of the NIH Division of Research Grants, remaining there until 1955.

Dr. Endicott in effect began the National Cancer Institute's program in cancer chemotherapy in 1955. He was made Associate Director of NIH in 1953 and became Director of the National Cancer Institute in 1960, in which position he served until November 1969. For the last period of his federal service, he served as Director of the Bureau of Health Manpower Education of NIH; Administrator of the Health Resources Administration of the Department of Health, Education and Welfare; and Administrative Director of the Lister Hill Center of the National Library of Medicine. Dr. Endicott retired from federal service in 1977 and since that time has served as Executive Officer of the American Association of Pathologists and Universities Associated for Research and Education in Pathology. Honorary degrees have been conferred on Dr. Endicott by the University of Colorado, St. Norbert College, Temple University, the Medical College of Pennsylvania, the Pennsylvania College of Optometry and the Iowa College of Osteopathic Medicine and Surgery. He was awarded the Distinguished Service Medal of the Public Health Service Commissioned Corps in 1964, the American Cancer Society National Award in 1965, and the American Academy of Family Physicians Meritorious Service Award in 1975.

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WASHINGTON, D.C.

Interview by Dr. Stephen Strickland with Dr. Kenneth Endicott

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SS: Dr. Endicott, I would like to talk with you about three different areas. The first, because I don't know it -- although I'm sure it's on record somewhere -- is your own professional education and how you got into the Public Health Service, where you served longer than almost anybody else. Then about the grants program, including the grants program of the Cancer Institute early on and later. In the third part, your perspective on your entire period of service: the Public Health Service and the National Institutes of Health and the National Cancer Institute.

I. Personal and Institutional Background

SS: I don't really know, even though I have known you a long time, how it was you came to join the Public Health Service. How did that happen? Where did you go to medical school?

KE: University of Colorado, and one of the department chairman had formerly been in the Public Health service and got me interested in it. I wanted to get married and I was looking for an internship that paid a little bit more money. And it turned out that the Public Health Service and the Marine hospitals offered a handsome stipend of \$1444 a year. Wow! This was 1939, so I graduated and got married the same day, and started for the U.S. Marine hospital in Seattle where I had my internship. And it was such a satisfactory experience that I competed in the examination for a regular commission in the Public Health Service. I was commissioned in 1940 and didn't retire until 1977.

SS: So you and Ernest Allen actually have just about the same length of time, almost 37-38 years?

KE: Yes. We started in different places, but stayed with it.

SS: What was the size of the Public Health Service in those days?

KE: Oh, I'm sure there were fewer than 1,000 officers in the Public Health Service.

SS: One has the impression that nonetheless in those days it was a more important and extensive network than today. I'm not sure.

KE: No. It's much larger and more extensive today. There have been many reorganizations. Some functions, such as those at the hospitals, have been terminated; others such as the FDA have been incorporated; others such as ADAMHA -- the Alcohol, Drug and Mental Health Administration -- have been created, and others have grown. The NIH alone is now larger than the whole Public Health Service in 1942.

SS: In those days the Public Health Service ran the Marine hospital system and did other things like work on widespread public health problems. There was a lot of work going on for finding a cure for venereal diseases.

KE: The research arm at the time in the early part of this century, was the Hygienic Laboratory. And that was part of the Public Health Service. It was located at 25th and E down in the gas house district of Washington, D.C. near where the modern State Department building is.

SS: And that dates from 1887, as I recall?

KE: No, I think that date refers to the establishment of a tiny laboratory at the Marine hospital in Statten Island, New York. I am not clear as to when they moved to Washington, but it was around the turn of the century. It was at a time when emerging new science of bacteriology was being imported into the United States from Western Europe. And since the Public Health Service was concerned with quarantinable diseases, it was quite natural for them to develop laboratories, primarily bacteriologic laboratories. In the early work of what is now the NIH, the emphasis was mostly in the area of infectious and parasitic disease and then in nutrition, particularly vitamin deficiencies.

SS: So that was an active laboratory, but of the whole Public Health Service, just a few people were engaged in that kind of research?

KE: Yes. When I came to NIH in the middle of '42 I think that the total staff was probably under 800. Now there were some operators out in the field, but that was the principal number at the headquarters.

SS: One of the interesting things is that the Public Health Service, which ran this little lab and had all these hospitals, was in a way the parent of the National Institutes of Health and the impression one has, I think it's accurate, is that NIH became bigger than the parent over a 20-year period. Is that generally correct?

KE: Well, the principal duties of the Public Health Service started off with the care of merchant seamen. That was I think 1789 or thereabouts. The marine hospitals were created to take care of merchant seamen so that they wouldn't become burdens on the port communities. Later on, the Public Health Service became involved in the control of epidemics. It was a natural relationship because some of the worst epidemics were cholera and yellow fever, which were introduced into the country in the ports by sailors. Then as the federal government assumed greater and greater responsibility for interstate commerce, then for health affairs, especially the spread of epidemics, this got the Public Health Service into the area of general public health.

At the time I joined, a very substantial part of the Public Health Service was called the Bureau of State Services. This bureau provided experts to state governments to help them with their problems in sanitation and infectious disease, more or less typical things that health departments do. So there was direct provision of health care, support of state public health efforts, preventive medicine — both international and interstate — and research.

SS: But the research component was the smallest part.

KE: Oh yes. I think that when I came to NIH there were probably more person-

nel involved in maritime quarantine than there were in NIH.

SS: There is one thing I have never been quite clear on. When the war broke out there was this unified, government-wide effort to marshal and encourage and supervise medical research: the Committee on Medical Research of the Office of Scientific Research and Development. But what did the Laboratory and NIH do during the War? Did it cooperate? Did it expand? Did it reduce its own efforts?

KE: Several things happened. People, particularly those in the Cancer Institute who were interested in radiation, radiation biology, radiation injury and so on, vanished. They were taken to a very secret place down in Oakridge, Tennessee and they sort of disappeared from the scene. They manned the biology division at Oakridge. The people interested in infectious disease devoted a lot of attention to the development of vaccines for tropical diseases or diseases likely to be encountered in combat zones. There was a very active program, for example, in malaria control, and development of substitutes for quinine because the source of quinine was largely cut off by the submarine activities. So that those who had been involved in the laboratory of industrial hygiene became involved as did some of the rest of us, in various toxicological studies related to the war effort. There was a project on toxicology of DDT; and I remember working on projects in the toxic effects of some of the cutting oils that they were using in war production facilities. But one of the surprising things was that many of the basic research people at NIH during the War just kept right on doing basic research. The scientific staff was very predominantly commissioned officers, and they were part of the armed forces. So they stayed where they were.

When the war broke out I was a quarantine officer in the port of San Francisco and received orders to join the Coast Guard. But before I could carry out the orders they were cancelled and they sent me to the penitentiary to do a research project on homosexuals. In Springfield, Missouri at the Medical Center for Federal Prisoners, they had collected 100 passive homosexuals from the military and federal prisons and had them waiting for me there to set up a study to find out if there was something wrong with their sex hormones. So I really started winning the War right after Pearl Harbor with this research project at a federal penitentiary! Then six months later I was transferred to NIH specifically to do pathology in connection with research on nutrition and during much of the war my publications had to do with nutritional essentials for blood formation. Some of the dietary factors in liver disease, especially cirrhosis. But on the side I was involved in some of the vaccine programs and toxicology programs and so on. So I would say that NIH's intramural research was probably disturbed much less during World War II than that in the academic community.

SS: On the other hand the Office of Scientific Research and Development made biomedical research contracts and I assume some of those were with medical schools and universities and people in them.

KE: Oh yes. But there weren't very many people off there to have contracts with.

II. Origins and Development of the Extramural Program

SS: That's exactly the kind of background I was hoping we could explore. And that brings us to the question of the origins of the grants program. You and I

talked about this a little bit last time. There are several statutory authorities which permitted research and even research grants. For example, the Randall Act itself, which in 1930 created the National Institutes of Health, gave NIH the purview to do research and I suppose to make research grants.

KE: I think that's right. You could obviously check it out, but I think it was section 301 of the Public Health Service Act, which came into being at that time. It gives very broad general authority to NIH which they rely on when they create a new institute which is not mandated by law or when the authorizing legislation for the Cancer Institute or the Heart Institute temporarily lapses. Then they operate under this section 301. So it was in being for quite a long while before it was actually used to make grants.

SS: Then the Cancer Institute was created as a separate institute in 1937 and the Cancer Institute had grant authority.

SS: Yes. The National Cancer Act of 1937 gave very, very broad authority to the Cancer Institute. They had authority for grants, for fellowships; authority to loan radium for the treatment of patients, to develop preventive programs and support education and cancer both for the public and the professions. Very broad authority. But in fact the war intervened before any very substantial extramural programs could be created. There was a limited program of grants. My recollection is that the appropriation was on the order of \$100,000 a year and that doesn't go so very far.

SS: That sounds right. In fact I have a note that says in 1946 when the OSRD research contracts were given over to the new Division of Research Grants, soon thereafter the Division took oversight of nine research grants of the National Cancer Institute which represented an investment of about \$85-86,000. So that probably was the order of magnitude the whole time. That's a very small number.

KE: Now the activity at the NIH extramural activity before the post-war era was really limited to fellowships, and a very limited number of fellowships, and to collaborative arrangements. There were some NIH explants or satellites in several universities. Notably Hopkins. Harry Eagle had an enterprise at Johns Hopkins, I think there was another one in California. NIH had satellites, intramural operation at the Rocky Mountain laboratory in Montana. Helena, I think it is. We set up a study of Rocky Mountain Spotted Fever.

You mention venereal disease. That was of course a very major health problem at the time I was a student. A huge percentage of the population had syphilis, the general population; not just the merchant marines. The treatment for gonorrhea was improved very rapidly in the mid-30s with the discovery of sulphamamide. But syphilis could only be treated with heavy metals until after World War II with the use of penicillin. Heavy metals were arsenic and bismuth and mercury. It required weekly treatments for several years. They alternated courses: intravenous arsenic for about six weeks and then intramuscular bismuth. When I was a student at Colorado, the VD outpatient clinic had operated 4 days of the week. One day was ladies' arms, then ladies' hips, then men's arms and men's hips. And you'd spend the whole morning giving intravenous arsenic or shooting bismuth in the buttocks. Hundreds and hundreds and hundreds of patients. At the time I was an intern, out of ten floors in the public health service hospital, two were devoted to venereal disease.

SS: Amazing. I knew there was a big program, but I thought there was maybe a

more strategic reason for a research effort on it, namely, of course, the merchant seamen.

KE: Of course, they were involved a lot with prostitution, so they were as infected as just about any population you'd want to find. But no, the big problem was really the civilian population. And the Public Health Service, during the war, had a very active research program which was not part of NIH. It was operated as a separate division and their public health measures involved especially "case finding". That is, a case of syphilis would come in, or gonorrhoea, then the public health officer or the public health person involved, would try to find out who the principal contacts were. So that you could chase the thing down and treat the necessary people. They were working on an intensive course of treatment to try to get away from having to have weekly shots for a couple of years. They were doing research on intensive shorter courses of treatment and they had experimental facilities in various parts of the country. My predecessor as director of the Cancer Institute, Rod Heller, was the chief of the V.D. program which operated out of the PHS headquarters downtown.

SS: Penicillin of course changed all that. A big wartime effort was involved in that. But obviously it had begun before the war. Did the Public Health Service research support that particular remedy?

KE: No, I think that you'd have to give the OSRD the main credit for that. The Department of Agriculture made some interesting contributions. In one of the regional laboratories they discovered that you could cultivate the organism on spent corn liquor. You know, when you make corn starch out of corn, the spent liquor really makes a very fine culture medium for the organism. Because of that, plus some of the work in the pharmaceutical companies supported and stimulated by OSRD, I think they'd have to get the credit. NIH really didn't play a major role as far as I'm aware.

SS: That was, I suppose, one of the major medical advances of the war-time biomedical research effort, the testing of penicillin that had been discovered earlier.

KE: Actually what the Americans did was to devise means to produce it in quantities.

SS: So that by the end of the war it was being used not only as a treatment of syphilis but for all kinds of things; for example, pneumonia.

KE: It took quite awhile to find out all the things that it was good for. It was initially used primarily in the military to prevent infections after trauma; staphylococcus and streptococcus and so on. It wasn't until after the war its use in syphilis was discovered. It was not generally available outside the military until after the war.

SS: After the war is when the consolidation on the civilian side of government of a major biomedical research enterprise took place. The event that people usually refer to is that when the war ended, something like 66 wartime contracts that the Office of Scientific Research and Development had, with who, by the way, with universities or medical centers?

KE: They were mostly with medical schools, with universities and their medical schools.

SS: The war was over in 1945 and these contracts were still going on and the Office of Scientific Research and Development was going to close out. The question was, who was going to manage these research contracts.

KE: The ones that were really only of military consequence were pretty promptly terminated. What they picked out was the projects they thought were potentially important to the civilian population, and that's what NIH inherited.

SS: There were 66 of them and the investment was about \$1 million. Dr. Allen says that Dr. Eugene Dyer was very anxious to see these contracts come to NIH. And that he was the NIH representative on the Committee on Medical Research for OSRD and he essentially talked Dr. Parran, the Surgeon General, into supporting him in a request to get them.

KE: That is, I think, quite accurate.

SS: So this makes Dr. Dyer a real entrepreneur. Somehow reading about him in other contexts, I thought he was rather conservative and didn't want to move too fast and didn't want to build too much of an enterprise, but that was a very enterprising initiative.

KE: I think he did not really envision the magnitude of the consequence of this move. And the reason I'm quite sure he didn't is that after I left the laboratory and went to the Division of Research Grants, the total extramural program was beginning to grow and it had gotten up to \$18 million. I said to Gene Dyer, "You know, this thing is beginning to get out of hand, how big do you think it will get to be?" And he said, "It will plateau at about \$25 million."

SS: And what year was that?

KE: That would have been '52 or '53.

SS: So, in six or seven years it had gone from \$1 million to \$18 million, and he thought it would level off at \$25 million, but instead it just kept growing.

KE: Interestingly enough, he recruited to look after the program two people who were at the time in the V.D. program; Casius J. Van Slyke and Ernest Allen. And they came out and started up the program.

SS: One thing that I am so impressed with is that immediately upon getting this program together in '46, they instituted a peer review system and they immediately, in the first year, set up study sections. What's fascinating to me is that, at least in others' recollections, they didn't do that because OSRD had had a formal study section. Or was it common practice in the private medical research world? Foundations didn't do it as far as they knew. They just had this idea that if it were going to be good research, it needed to have the judgment of peers before it was supported. Do you know of any practices that they had in mind, if not models that they followed?

KE: At the time I became involved in the grants program, the first year it was there, there were two advisory councils; one was the National Advisory Cancer Council, which was made up of experts in the field of cancer. And so that council had the technical capabilities to do a pretty good job of reviewing the applications. The advice for the NIH program all came from the National Advisory Health Council, which was so broad that it really didn't have serious technical

competence in many areas. The members were primarily public health experts.

SS: Was the National Advisory Health Council advisory to the whole Public Health Service?

KE: It was advisory to the Surgeon General.

SS: And it included obviously some physicians, I assume.

KE: Oh, yes, but they were primarily people interested in public health. They were drawn to a considerable extent from schools of public health, or state health officers and that sort of thing. It became apparent that to get competent technical review they were going to have to have some special committees. I think there were 6 originally. I was involved in the one which was supposed to deal with blood substitutes and shock. It became later the hematology study section, and I organized it and my chief organized one in pathology. Henry Sebrell organized the one in biochemistry and nutrition. There was one for antibiotics, one for malaria, which really sort of covered the gamut of the things that we had inherited from OSRD.

SS: Yes. I was just given a picture yesterday of some of the earlier study sections; a 1947 picture, and I think I made a note of how many study sections there were; and I think there were 18, 19 study sections in 1947, so that was within a year.

KE: That's right, but I think we started off with maybe six. You see the genius of Van Slyke. I think he probably should get the credit for this. He envisioned this as a real opportunity to not just carry on these things, but get something more significant. So an announcement inviting proposals was sent out, and there were a surprising number of them that came in with applications for a grant. Then when he and Dyer went down to testify before the appropriations committee, they could tell this sad, sad story about how many applications they had, splendid applications, and how little money. So the backlog of recommended but unfunded grant applications was one of the major leverages in building up the appropriations during the early years. And they came in on all sorts of scientific areas and they obviously had to get more study sections.

SS: It was the right time to do this, wasn't it? Because my impression is that the American public was greatly impressed with the nation's wartime research effort; that a lot of good things had come out of it.

KE: Oh, yes. You know the atom bomb was pretty impressive as to what comes from R & D. It was opportune also because there were a lot of university types whose scholarly work had been interrupted by the war and they were eager to get back to work. So that the applications came out of the wordwork so to speak.

Another thing I wanted to mention was that even those who had stayed at home and worked on defense-related research wanted to forget all that and plunge back into basic research.

SS: You and I talked briefly last time about the small problem of running study sections, because these applications came in in much greater numbers than anybody had anticipated, and you found out you couldn't do it with your left hand. You needed somebody as an executive secretary of the study sections full time.

KE: We started off using in-house scientists, NIH intramural scientists as executive secretaries. It became fairly time-consuming. My impression is that the reason that they went to full time was at least in part because the NIH intramural people whose term was indefinite came to have what was regarded as excessive influence over the recommendations of the study sections. Van Slyke and Allen did the selecting, selected executive secretaries who were not expert in the particular field. For instance, the executive secretary for the surgery study section was an insect physiologist.

SS: So, he couldn't have an ax to grind.

KE: Right, and that became a part of the tradition that the Executive Secretary, even if he was an expert, really studiously refrained from influencing the recommendations of the study section.

SS: Over a few years, then, the grants program grew and grew and in fifteen years, you said, it went from \$1 to \$18 million. Meanwhile, what happened to the intramural science program?

KE: The intramural science program was pretty much limited by the size of the physical plant. It wasn't until active construction programs on campus got underway that it was possible to expand it to any great extent. In 1946 it was the administration building and five of those four-story red brick buildings, and that was about it. The first big increment in intramural program really came with completion of the clinical center, which is a huge building with a lot of research space in it.

SS: How many scientists does that mean were working at the end of the war in the intramural program?

KE: I think the total staff was essentially all intramural at the time. The total staff was under 1,000 I believe. I wouldn't want to have to swear to it, but I'd say there were probably not more than 300 actual scientists. The rest were support persons.

SS: So, we're through the war and the establishment of the new grants program at NIH. I thought rather than just repeating exactly the same questions I would see if I could get you to help me think through certain strategic questions. The first is: Was there actually a strategy and a pattern, or did the development of the grants program of the Cancer Institute and NIH simply follow opportunities that presented themselves from the outside? You said in earlier that once the grant program was set up that an "all-points bulletin" was sent out to the American biomedical science community, "NIH and the Cancer Institute will have grant money." But you then just sat back and waited for the response? Or were you more directing than that in the bulletin and the announcements?

KE: I think there was probably a prevailing preference to stimulate basic research, but for the most part, NIH reacted. Historically the Public Health Service had been rather opposed to the categorical institute approach. And I don't think that the events that followed rapidly, the pawning of one categorical institute after another, was centrally planned and orchestrated. There may have been one or two exceptions to that. For the most part, I think that external pressures really had a lot to do with determining the growth pattern of NIH. There were some very powerful figures outside who had specific disease targets in mind. They pressed for the development of disease-related institutes and I think there was less than total enthusiasm for this evolution internally.

A good example, I think, is what started off as the Arthritis and Metabolic Disease Institute. The year it was created there were all sorts of bills introduced in the Congress for diabetes, arthritis, etc. And (this was in the 1950s) the Public Health Service came up with the bright idea of a kind of a catch-all. They selected the title "Arthritis and Metabolic Disease" which covers a multitude of sins. They bundled a group of basic science-oriented laboratories together, already in existence of NIH, and redesignated them as the intramural program of arthritis and metabolic disease. And I think that the staff of the Arthritis and Metabolic Disease Institute for years afterwards maintained a primary interest in basic research. External pressures to emphasize disease-oriented research increased year after year. Ultimately, after a number of efforts, they finally ended up in splitting up the Institute into two. Outside communities still insisting that they wanted to get on with specific disease problems.

Perhaps unconsciously this preference of NIH for fundamental research set the stage for the pattern of dual review. If you look at the structure of the study sections historically, they've been structured around scientific disciplines, and applications are selected on the basis of their excellence from the standpoint of the scientific discipline, much more than on the basis of their relevance to a specific health problem.

SS: There are so many strands there I want to pick up. On the last point, would you repeat exactly how that dual system works; that is, how the study section looks at scientific merit first, the intrinsic merit of the particular proposal but then how it gets assigned to an institute. The funded money comes from the institute's budget.

KE: The first thing that happens when an application is received, is a decision with regard to where it will be assigned for review. The decision is based on which study section is technically most competent. The selection of an institute is based on a set of guidelines in which each institute defines its area of interest. In many cases it could go to one of several institutes. Occasionally it is assigned to several institutes. But by and large the institutes obviously have special interest in particular fields. The Mental Health Institute would look to study sections in behavioural sciences for example. And Allergy and Infectious Disease would look toward immunology and microbiology and virology, and so on. But this decision is made by the DRG staff at the time the applications are received. Now, if an institute has been stimulating proposals in a given area, they notify the Division of Research Grants which sets the stage then to direct them to that institute.

SS: And was this dual review created at the beginning? Was it there from 1946 with the taking over of those wartime contracts?

KE: Yes. The Cancer Institute was the only one that provided its own review before that. But yes, from the very beginning there was dual review. There was only one other council besides the Cancer Council, and that was the National Advisory Health Council. They did have dual review.

SS: And that pretty well continues today.

KE: There are some proposals that come in for which no suitable section has been created and they make special arrangements, special study sections, to handle those.

SS: What you say about the outside factors and forces pressing for more attention to more work on specific diseases certainly accords with my recollections and my research. But my impression is, with respect to another kind of growth, that the idea that the grants program could help build a national, far-flung biomedical research enterprise, came more from the early leaders of the NIH and its grants program, that is, the post-war period. Dr. Van Slyke himself may have had this idea. Certainly by the time Dr. Shannon became Director, he espoused the idea of building a national network of excellent scientists in many institutions.

KE: I think that those of us who were involved early on recognized the potential of developing strong research programs in universities other than those that had them at the time. And I think conscious efforts were made to do that, and particularly to get additional funding to make it plausible. There came to be the recognition that although the basic scientists might prefer not to have categorical institutes, such institutes served as splendid devices for getting money, and that one could justify generous support of basic research within the framework of almost any categorical institute.

SS: When did you go over to the Cancer Institute?

KE: I went first in '55 I guess it was, to set up the cancer chemotherapy program. And then in '58 Shannon asked me to come to the front office as associate director and I was there for two years and then when back as director of NCI and stayed as director until 1970.

SS: Even at the Cancer Institute, given your outside supporters and interested parties, wasn't there consistent pressure on both ends of the spectrum; some people saying you ought to do more targeted work on chemotherapy, and other therapeutic methods, and others saying you ought to get to the bottom of the causes, the etiology of cancer?

KE: That tension persists to this day. There was a reluctance for developmental research at NIH and those of us who attempted to put together a broad targeted program in addition to basic research had an uphill time of it at first. My first exposure came during the Korean War while I was still in the laboratory. NIH accepted responsibility for research and development in the field of blood and blood substitutes during the Korean War and the director asked me to take charge of that program. At the time I had no mechanism other than grants.

SS: This is when Dr. Sebrell was still director?

KE: That was when Dyer was director. We had a lot of developmental work to do, and it was like pulling hen's teeth to get the necessary parts of it through study sections. So I promised myself I'd never get in that kind of a predicament again. Meanwhile I had moved from the laboratory to the Division of Research Grants, and Congress put on pressure for an engineered-directed program in the cancer chemotherapy field. Henry Sebrell, who was then Director, asked me to take charge, and one of my conditions was that I had to have authority for negotiated contracts. The Congress actually wrote it in the appropriation act the first time around.

SS: But you suggested it?

KI: Oh, I insisted on it.

SS: And furthermore you formulated the mechanism for doing it.

KE: With plenty of interference and meddling from higher up in the department; there was a lot of anxiety about it. We had to borrow our concepts from the Department of Defense. The first year I had no authority for negotiated contracts; I mean for cost-type contracts, they were all fixed price contracts. So we and the contractors had to engage in a guessing game as to what it was going to cost, and then fix the price. I actually borrowed the procurement officer from NIH to help me set up the contracts at the outset, and for quite a long while the Cancer Institute had the only research contracts of NIH.

SS: The Cancer Institute had contracts all along as well as grants?

KE: Yes, from 1955 on.

SS: This is especially interesting in two respects: one, of course, when at the end of the war, again that pivotal point, the transition of wartime contracts to NIH, at that moment in time the bulk of the work of medical research was done through contracts.

KE: My recollection is that it wasn't until years later that Defense was given a grant authority. If the Office of Naval Research was going to support research, they had to do it with a contract. That's the authority they had. And this led to quite a lot of confusion, because one government agency was using grants for the same purpose that another agency was using contracts. And there was a tendency to confuse the two. In my mind they were very distinct. Contracts represented research and development conceived of by the government and purchased by the government. Therefore not a grant at all.

SS: A grant implying pursuit of an idea but some leeway in pursuing it.

KE: Yes, self-sponsored research; investigator-sponsored research. But, like anything else, there's kind of a spectrum and there are things that fall into a grey area.

SS: Even if you and those working with you on the blood program or later on the chemotherapy program had an idea and some knowledge that you knew could be deduced and fixed, was there some sort of peer review when you made contracts?

KE: Oh yes. In the chemotherapy program when we started off I appointed the equivalent of study sections in screening, pharmacology, clinical trials, and so on. The staff and these committees decided what it was we needed done, got out requests for proposals and those same committees reviewed them then. Later on we got terrible problems with conflict of interest, because most of my outside advisors were also consultants to pharmaceutical companies, I abolished the external committees and used intramural committees to review the contracts. There has always been some sort of system for peer review.

SS: The other strand I wanted to pick up on is this: There has sometimes been a notion that the National Institutes of Health including the National Cancer Institute was pretty rigid in the way it went about things. Everything had to go through study sections, and of course still does; the leadership of institutes waited until scientists decided what they thought was important; that you could only use grants. The image these occasional critics tried to present was that NIH was a very passive operation; that the leadership wasn't activist enough in

trying to encourage research on specific things or work hard on disease targets. But what you're suggesting is looking at it a little more long-term, and you created new devices. You consistently supported basic research in its own name and as it might relate to diseases, but you also had very targeted programs using a variety of mechanisms to keep them going.

KE: There were probably two institute directors who had a lot to do with getting on with applied and development types of programs. I was one in the Cancer Institute. I really had a lot to do with planned programs in the Cancer Institute, but never in my time at the expense of grant support because we always had plenty of money for that.

SS: And you've always recognized the reality that it was possible that work in basic research on genes or something else would have a definite relationship and definite impact?

KE: Oh, sure. But my feeling was that if this was indeed a health agency, and our purpose was to solve the cancer problem, we ought to do the best we could with the information at hand to get some practical solutions now, you know, and not wait for a hundred years. The other was the Mental Health Institute. They were not particularly research oriented, but the director for a long time, Bob Felix, after looking at the problem decided that the most urgent need was to get more people to look after the huge numbers of people with mental illness. So his emphasis was on training, not training for research, although that was supported, but the big emphasis was on training psychiatrists, psychologists, psychiatric nurses and social workers in large numbers to man the barricades out there. I think that the Heart Institute got off in this direction, particularly when there began to be an interest in the artificial heart. And, you know, it spread.

SS: Was it the Heart Institute that supported the work that you did during the Korean War, that you supervised on blood and blood plasma?

KE: Yes. Because they said, "Blood's our field." In fact Van Slyke once defined the Heart Institute's area of interest as any part of the body that's bathed by blood. Yes, they supported it, and we got our developmental work done in the end through a chairman's grant. A very substantial chunk of money for those days was given to the study section, to the chairman of the study section, and that's the thing we used to do developmental work.

SS: But again, that's evidence that there is some flexibility, that it's not an absolutely rigid categorization that you can't penetrate.

KE: If you're determined to do it, you can use almost any device.

SS: I want to come back to other impetuses for the direction and the emphases of the biomedical research enterprise after the war under NCI and NIH. But just on that, while expanding the grants program, holding peer review sacrosanct, multiplying the number of study sections, having dual review of scientific merit and institute interests; that mechanism grew and the money for it expanded.

Nonetheless, what you're saying in a nutshell is if a director of an institute or a program within an institute or any other leading science administrator saw a need and a problem and an opportunity, there were ways to push scientists to address those problems?

KE: Sure. Even in the Division of Research Grants you could do it.

SS: How could you do it there?

KE: You create a study section and give a chairman's grant and you have a heart to heart talk with the chairman and the study section and tell them you're convinced that this field needs stimulation. It's an important field, it ought to be developed, go out and do it. And we did this while I was in DRG. In the field of biophysics and molecular biology, and F. O. Schmidt from MIT was the chairman we selected. And they did a lot to stimulate this field.

SS: Now, how did they get the word out, how did you get the word out; another all points bulletin?

KE: They held a bunch of meetings on university campuses, as I recall it. Letter writing and contacts at meetings, encouraging people to submit proposals and so on. One of the devices that I think has been very important in developing neglected fields has been the training grant mechanism. While I was in the Division of Research Grants we felt that a lot of the proposals that were coming in were weak from the standpoint of experimental design and the publications were weak from the standpoint of statistical analysis, so we felt that there was a big need for training statisticians. So we started training grant program. Perhaps the outstanding example is the field of neurology. When the Neurology Institute was created, there were very few full-time neurological faculty in all of the United States. Most of them were part-time or volunteer, not in a situation to do any research. They didn't have time, or facilities, or training. And the Neurology Institute, largely under the brilliant leadership of Fred Stone, who was their extramural chief at the time, got a neurology training grant program going and they spun off into neurophysiology and neurochemistry, and neuropathology was training a generation to fill full-time slots at the medical school. And later they used the training grants to provide the funds to hire them.

SS: This is important to get on the record because there is that strand of criticism about NIH passivity. One of your best friends was also one of your keenest critics -- when I say "your" I mean the whole Bethesda campus -- Mary Lasker. She periodically said, "Get on with solving disease problems and don't be so fastidious about all these little devices to make sure you have the best scientists working on the long term basic research..." Something like that.

KE: She and I fell out over something quite different. I think I was one of her shining lights as far as getting on with trying to solve disease problems. We fell out over who should control contracts.

SS: You had increasing authority for contracts at the Cancer Institute. Was there ever an occasion, or more than one occasion, when somebody didn't pass muster for the study section to get a grant and then tried to come through the contract door?

KE: Oh yes. That happened. But occasionally it went the other way. They would be unsuccessful in getting a contract and turn around and fund the same basic project successfully with a grant. It happened both ways.

SS: The latter makes more sense and is more natural, isn't it? Your review saw that the proposal could produce what you thought was important under a contract, was more related to your specific program objectives. Then a scientist or group of scientists might say, "Well, it has broader use and we'd like to come in and get it supported under a grant." I wanted to focus on that because the point really is that quality control was consistent even when you had a targeted program and you were using contracts to move along faster toward program objectives.

KE: Yes. In a properly run contract program, one of the key elements with regard to a specific project is to whether it fits into your grand design. If you are seeking better methods for screening drugs, or a better experimental animal or something of the sort, a major criterion is: does this move you forward in that direction? Is this a good man, can you expect productive work to come out of it, and is the price right? On the grants side it's much less related to some preconceived target. But rather whether this is an important area for gaining new knowledge. Is this a good way to do it and a good man, and so on.

SS: The sequence of grants that the Division of Research Grants was in charge of was of course research grants, then training grants, then contracts? Or did the DRG have control of contracts? Was that left to the individual institutes?

KE: Unless this has happened in recent years, DRG has not been involved in contract review.

SS: Contracts in a certain way added to your balance wheel approach didn't it? It gave you additional authority to do things you felt were important. But the grants program itself in a certain way acted as a balance wheel, the Division did. Earlier it had some of its own money that it could use in that way.

KE: Until what is now the National Institute of General Medical Sciences was spun off, DRG was a kind of non-categorical institute with its National Advisory Health Council and some money to spend.

SS: You were saying that the traditionalists in the National Cancer Institute didn't want to start a chemotherapy program. They thought science wasn't ready for it, that you didn't know enough.

KE: Yes. There were people there working on it in the intramural program and there were some grants in the area. But the feeling was that there were not enough scientists available yet to justify, to make possible an engineered-directed program. Others didn't agree with them. I was asked to take charge of the program because they really weren't doing it. Didn't want to.

SS: So then you came in and set up that program and later you became director of the National Cancer Institute. When you became director, what did you do to expand the battle lines so to speak?

KE: The very first thing I did was to look at everything we were doing in the light of what might be exploitable leads. One area which I felt was very neglected was the field of chemical carcinogenesis. So I set up a targeted program

program in the field of chemical carcinogenesis which is now part of the National Toxicology Program, but was in the Cancer Institute for a number of years. The attempt there being by animal experiments to detect potential human carcinogens and get the chemicals out of the human environment. A little bit later as discoveries began to pile up in the area of cancer virology, I set up a special cancer virology program. The objective of which was to determine whether, as in the case of a number of animal cancers, a number of human cancers were not of viral origin and if they were, to come up with some vaccine or way to control that. It took a long time but now we have the hepatitis vaccine which is actually hopefully going to help reduce liver cancer around the world.

SS: When you took over the Cancer Institute, how many clinically distinct forms of disease did you judge cancer to be? I take it that in the thirty years before when the Cancer Institute was created, you just thought of cancer as cancer.

KE: We used to talk in terms of a hundred or more, but the morphologists keep subdividing and subdividing until it's kind of an endless sort of thing. From the standpoint of morphology and clinical behavior, there are many different kinds of cancer.

SS: And in a way the certainty that there were many different kinds, therefore probably a number of different causes, was the impetus to broaden the way you approached it?

KE: It was clear from laboratory work that viruses can cause cancer, as well as chemicals, radiation, metabolic defects, genetics, and maybe some others I didn't mention. You could approach it from the standpoint that you'd have to find many therapeutic and preventive modalities.

SS: At what point was it recognized, was it accepted that diet could even contribute to the development or emergence of cancer in subsets of the population?

KE: There was some evidence in this direction long before I became involved. For example, stomach cancer had been attributed smoked fish, and so on. There had been associations for quite a long time. It's still a controversial area. The one thing that I felt was that with so many different "causes" there must be one central thing that was important and these all contributed to it. The one that seemed most likely to me was virus. We're obviously talking about a different kind of virus that behaves in a different way from measles or the acute diseases. I think the current research is focusing now more and more on the role of viruses and oncogens and genetic growth factors that under some circumstances turn on at the wrong time and cancer develops. I think we're getting closer to finding out what the key factor is.

SS: So you still believe that there will at some point be identified a common triggering factor?

KE: Really a common phenomenon.

SS: Regardless of other factors that are currently recognized to be associated with cancer, whether it's chemical or radiological or viral?

KE: Primarily viral. There is an interesting argument right now as to whether cancer viruses aren't cancer viruses because they picked up part of an animal

genome that's in the animal maybe to regulate growth or something. Genome is really the sum total of the DNA genetic apparatus. It's sort of a chicken and egg situation. Did the cancer introduce the virus into the genome or did the viruses accidentally pick up the cancer genome and incorporate it into its own genome and then transfer it to other cell? And I don't know the answer. Who knows?

SS: One more thing and then I want to ask you about progress against cancer and progress against disease. Do I remember that at some point there was a group or a way mechanism for the review of unusual proposed approaches to cancer research? Did you have special study sections to look at these?

KE: Not during my time. There may be one now, but I'm not familiar with it.

SS: Maybe that was much earlier.

KE: At the time Andrew Ivy was associated with the Council, they did draw up a set of criteria -- the Council and staff did -- that must be satisfied if NCI was to consider a proposed new cancer cure.

SS: Was this a sort of a screening device?

KE: It was a set of criteria. People to this very day, quacks of all sorts, propose new miracle cures, and put pressure on the Cancer Institute to test them. This was a set of criteria which had to be met before NCI would test one of these reputed cures.

SS: Did this set of criteria satisfy Dr. Ivy?

KE: He was the primary architect of it. But when he became involved with Krebiozen he refused to abide by the criteria. As a result, we never did test Krebiozen.

III. Impact of Research

SS: What's your impression -- 15 years out of the Cancer Institute, but keeping close touch with biology and science generally -- as to what kind of progress we're making with respect to cancer? President Nixon, just at the time you were leaving the Cancer Institute, called for a new war on cancer.

KE: In the fields that I'm most familiar with, chemotherapy and chemical carcinogenesis, I think progress has been quite slow. But there had been real progress and I think it's safe to say that we've actually seen some cures from chemotherapy in certain, particularly childhood, cancers. On the chemotherapy side I think this field has gotten almost out of control.

SS: How so?

KE: I think it has to do with the public attitude demanding no risk. We have a large screening program which constantly turned up carcinogenic materials in rodents given very high doses for a life time. And it has become popular to extrapolate that to very low doses in man, and say there are going to be six cases out of a million and so on, all based on the questionable extrapolations, and resulting in all kinds of regulatory measures, in my view many of which are

which are not justified. I think we have created kind of a Frankenstein, in a way, which now is going to take a long time to settle back down to the problem of identifying major causes and not pursuing all of these "will-of-the-wisps" at very great expense. You know, the bulk of the "Super Fund", which is a very expensive program, really is based on fear of cancer. We've just completed a big study in this area and we were able to find almost nothing where you could show a cause and effect relationship. There is an amazing bulk of legal actions. It's expensive, and I don't think it's getting us very far.

SS: Is it your federation that has done this study?

KE: Yes, UARED. A book will soon come out in Pergamon Press. I'm expecting it any day. But back to the virus thing, there was a period when it fell into some disrepute, but now it's smelling like a rose. I think that there has been an interaction between the strong push in the virus area on the part of the Cancer Institute and development of knowledge of DNA, which had a lot to do with creation of the genetic engineering approach. I like to think that we may be getting pretty close to getting a grasp on what is the central nature of the cancer process from a biological standpoint. Whether out of this anything miraculous is going to emerge in the way of controlling the health problem, is a matter of some speculation, but I think we're getting to the point where we're beginning to understand what it is we're up against.

SS: Is the seeming important progress in understanding the nature of AIDS a reflection of this? In today's paper for example there is a report of two studies, a Harvard and a Pasteur Institute study identifying an associated virus out of Africa, basically. Is it your impression, as it is mine, in the case of this terrible disease which came on the scene relatively suddenly and relatively recently, causing deep concern generally in the population and panic in certain subsets of the population, that and also in relatively short order we've made significant strides in understanding it and therefore moving toward being able to control it? Would you share that sense of it or not? Let me tell you what my bigger question is. My larger question is: Has all of the work which has gone on in the last three decades, on cancer and other diseases, put us in a good position scientifically and medically to cope with dramatic new disease forms that suddenly are unloosed on the population?

KE: We're dealing here with a very subtle form of virus, not the kind of thing you think of ordinarily with a viral infection. With acute infection you either die or get well in a few weeks. There's no accident that the etiologic virus, in AIDS, was discovered in this country by an investigator at the Cancer Institute; it was an outgrowth of his studies of viruses that cause certain forms of leukemia, and the techniques were developed in that connection. There appears to be a group of viruses which are slow viruses in which the serious disease consequence appears years after the initial infection and in the interim nothing much seems to be going on. I believe that it is in this category of new diseases that the cancer approach may offer some significant advantages.

SS: Are we in a "Golden Age" of medical science?

KE: I have thought that we've been in one and maybe we're getting into a slump. I'm sure that there are many discoveries to be made yet. But the interest of the public as reflected by the administration seems to be turning in other directions dollar-wise. We are not seeing a lot of new miracle drugs coming along. Physicians themselves are staying away from research careers in droves.

v SS: Even though the physician population has grown enormously and some say overgrown?

KE: Yes, and I think this has to do again with where the money comes from. There was a period of time when the way you expanded a medical school and paid the bills and became famous and prestigious was through research. That's where you got the money to hire the people. The growth of research support didn't keep up with the expansion of medical schools. We went through a doubling of enrollment in medical schools beginning in the late '60s and continuing until the mid-'70s, maybe a little longer. Because they couldn't continue to turn to research money, medical schools went increasingly into the business of making money by taking care of sick people. The criteria for recruitment of faculty in the clinical departments began to de-emphasize research and emphasize ability to attract patients and take care of them. Take my own specialty — pathology. If you look at a pathology department, the big faculty is involved in patient care: clinical pathology, surgical pathology, hospital pathology. A relatively minor part of it is in research and this is true in the clinical departments as well. It takes a fellow who is really interested in research to forego a clinical career even if he's academically oriented, to devote most of his time to research.

SS: But does this also reflect a slowing up of research progress?

KE: In some areas I think it does. In areas that are primarily the province of the clinician, I think that things are slowing up. We continue to build capability to do good basic research, and I think Ph.D.s rather than M.D.s are driving the system now. We're making fabulous strides in some areas as you are well aware. In genetics, immunology. So there are very exciting things going on with basic science. But if we see as a result of Graham-Rudman, not only a cessation of growth, but a roll back to a lower level, I think you're going to see what's already happened on the M.D. side. Young people are not opting for careers in that area. I think you're going to see young people not opting to become Ph.D.s in the basic sciences because where the hell are they going to get a job?

SS: I don't know what they're going to do. My hunch is that there are going to be some young people who, when they started college might have thought they wanted to be physicians, but with a whole new set of economic conditions, lawsuits, malpractice suits, etc., who will change their minds.

KE: Yes. We've dropped down now to something like two applicants per available slot in medical school. It was close to four. And I think it will go down even more because they're taking powerful steps to cap physician incomes and even roll them back. Everybody now agrees that we already have an over-supply and it's going to get worse. I think we're going to see school after school cut down its entering class, and I think some of the late-comer schools may not survive.

SS: The NIH grants program which is of course the central feature in all of these oral interviews that I'm doing has been a major vehicle for the growth and expansion of medical schools, as you said, in the last quarter century generally.

KE: I would say that that began to diminish in importance by 1965. The job I had after I left the Cancer Institute represented an interesting phase in federal policy when high priority was given to increasing the number of doctors. We

had a big program designed to cause existing schools to expand their enrollment and also to create new schools. We built buildings, we subsidized operations, we had fellowships, scholarships, curriculum development grants. It was for a time a \$billion a year program. In about '75 that pretty much came to a screeching halt. Now if you look at what federal money really drives medical education, the big money is coming out of Medicare, Medicaid. I should have said "health insurance" more broadly.

SS: With Medicare a big chunk of that, yes.

In the last forty years, from '46 when the NIH took over the wartime grants to the present, we've expanded the federal investment and therefore the national investment in medical research substantially. I don't even know what the total budget is for fiscal 1987.

KE: For '87 it's suppose to be cut back to just under \$5 billion for NIH.

SS: And forty years ago it was a few million.

KE: In fact it was about \$500,000.

SS: So from \$500,000 to \$5 billion over a forty year period, if you count for inflation, that's a lot of money, but on the other hand that's still a quite small part of the federal budget. In any case, during that period we've seen life span increase, we've seen in the last fifteen or twenty years, decline in certain kinds of diseases; particularly cardiovascular disease as a killer has gone down. We know more about cancer; people are more health-conscious; we know more about or have at least re-focused on the connection between diet and exercise and health. Do you see any direct association between our aggregate investment in medical research and the clearly improved health status of the population?

KE: Oh, sure.

SS: But how direct can you make it?

KE: Well research is certainly not the only factor. There have been a lot of efforts and money gone into bringing the fruits of the research to the public through public health programs, through improved care. You know, our poor people are certainly getting much better care and they're getting better nutrition and housing and so on. But yes, it's had quite a lot to do with providing the knowledge on which to base practical gains. The bill for providing some of the health care that has been ginned up is pretty big, and it's gotten to the point where the public is asking whether they want to spend that much on it. I wish I knew. Political slogans and so on are one thing. What people want and feel may not be quite the same. A wise old owl in economics, Eli Ginsburg, maintains constantly that people haven't lost interest in health yet and we probably haven't seen how far they are willing to go to invest in it.

I think some of our investments now, though, have been in a different direction. We're spending a lot of money and effort on environmental protection now, which is not really within the health arena anymore. It used to be, but it's been lifted out into EPA and the Department of Energy and outfits like that. But there's a lot of money going into it. When they talk about \$5 billion going for the "Super Fund", to clean up old dumps, that's primarily a health thing.

SS: Are we in balance in our approach to health needs and health problems? That is, we're spending \$5 billion a year now from the federal treasury on biomedical research; we're spending many times more than on direct provision of medical care, and on environmental efforts?

KE: I don't think we're in perfect balance. If you look at the statistics, we spend a huge amount of our health care dollar on the treatment of people with terminal illness who will be dead within the year. An amazing amount. We're spending a lot of money on the care of the elderly. Meanwhile we have a lot of children, poor children who I think are not receiving adequate health care. But more important, they're not receiving the basic necessities: decent housing, adequate nutrition, etc. I think we're out of balance spending too damn much on defense and not enough on other things, but that's more an ideological thing I guess.

I'll tell you one thing -- we're a hell of a lot better off than we were when I was a medical student in the health field. In fact, it's a little hard to realize what strides we've made. I have to pinch myself when I go through a teaching hospital now to see what patients they have in there as compared to what we were seeing.

SS: Very good. Have we left out anything?

KE: Oh, I don't know, let me think. We talked about contracts, we talked about training grants, we talked about basic research versus development. I think that there's been a major turnaround with regard to this in terms of NIH attitude. The NIH policy makers now make a strong case for clinical trials, for vaccine development, for jumping onto an emerging problem like AIDS. At the same time they still give a very high priority to doing investigator-initiated basic research. I think it's much closer to a health agency than it used to be. It's not just research any more.

SS: Is it closer to the old Public Health Service public health emphasis than it used to be? We are concerned about health problems.

KE: The thing is that, with the chronic diseases, it's hard to discover an effective community approach. So much of it comes down to personal lifestyle; there are no vaccines for alcoholism or drug abuse. I think that the public health approach is kind of frustrated by the chronic diseases. But the challenge has shifted now from control of bacteria and viruses. Now the big public health emphasis is really, in my view, trying to create a no-risk environment from the standpoint of trauma and substance abuse and diet, lifestyle and control of toxic substances in the environment, which may not turn out to be as productive as one would hope.

SS: And why is that? I think I know what your answer is. We talked about it a little before: the interconnection of factors.

KE: Yes. Anybody with a public health background instinctively seeks out cause and then having identified that, devises a preventive strategy. The current diseases, at least in our present state of knowledge, seem to occur many years after the initiating factor and you're between a rock and hard place trying to figure out what the hell it was. For the most part, you don't really know, and where you do know it looks as though, as in the case of cancer, there appear to be multiple factors. So that a simple, straightforward health program today is really very hard to conceptualize.

SS: That is the new challenge, isn't it?

KE: Yes I think it is. We may be spending too much time and effort on the cause of death and not enough effort on maximizing health during the productive years.

SS: With the demographic trends, though, I wonder if that emphasis is not going to continue increasingly toward care of the elderly?

KE: We certainly are going to spend more money taking care of them. But whether from the standpoint of the long-term welfare of the human race, keeping people living from age 70, — I'll be 70 this year -- to age 90 may not be all that productive. We don't know what to do with all the old people we've got now. But I don't want to die and, you know, nobody does. We're discovering that some of them at least can do productive things after age 65 and we may have to come to that.

SS: There never really has been, I suppose, a rational, logical way of deciding what the national investment in health should be and how it should be divided as between preventive efforts and environmental efforts and direct attention to the diseases and therapies for them.

KE: I think that's not likely to occur in a society like ours. Maybe in Russia, but I'm not so sure that even there it's a coldly, logical, rational thing. It's clear that the founding fathers of the USSR set as a very high priority providing some kind of health care to everybody. It may not have amounted to much but at least whatever it was they got it spread around.

SS: This is a fantastic amount of information, Dr. Endicott. Thank you very much for your time.

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Experience: Executive Officer, AAP and UAREP, 1977-present
Ad. Director, Lister Hill Center, NLM, NIH, January to July 1977
Administrator, Health Resources Administration, August 1973-January 1977
Director, Bureau of Health Manpower Education, National Institutes of Health, November 1969-August 1973
Director, National Cancer Institute, NIH, July 1960-November 1969
Appointed Assistant Surgeon General, USPHS, January 1960
Associate Director, National Institutes of Health, 1958-1960
Chief, Cancer Chemotherapy National Service Center, National Cancer Institute, 1955-1958
Scientific Director, Division of Research Grants, NIH 1951-1955
Chief, Section on Metabolic and Degenerative Diseases, Division of Pathology, NIH, 1942-1951
Medical Center for Federal Prisoners, Springfield, Missouri, 1942
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Intern, US Marine Hospital, Seattle, Washington, 1939

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Special Awards: Distinguished Service Medal of the Public Health Service Commissioned Corps, awarded 1964
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Doctor of Science (Honorary) St. Norbert Col. DePere, Wisconsin, 1967
First PAP Award for Distinguished Service by the Board of Directors Papanicolaou Cancer Research Institute in Miami, Florida, 1/9/70
Doctor of Science (Honorary) University of Colorado, 1970
American Association for Cancer Research Award, April 1971
Doctor of Public Service (Honorary) Temple University, Philadelphia, 5/72
Doctor of Science (Honorary) College of Osteopathic Medicine and Surgery, Des Moines, Iowa, June 1972
Doctor of Medical Science (Honorary) Med. College of Pennsylvania, Phila.,
Doctor of Optometry (Honorary), Pa. College of Optometry, Phila., 5/74
American Academy of Family Physicians Meritorious Service Award, 3/75