Dr. Saul Rosen, former acting director of the Clinical Center at the National Institutes of Health
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The subject of this interview is Dr. Rosen's reflections on the NIH Clinical Associates Program. Interviewer: Melissa K. Klein.

Klein: First of all I'd like to start by telling you I will be recording this. Is that all right with you?

Rosen: Sure.

Klein: Perhaps you could begin by giving me a brief background of your childhood, where you attended college and what made you decide to pursue a career in medicine?

Rosen: I grew up in Boston, Massachusetts. I went to high school where Dr. Harvey Klein went to high school, Boston Latin School. I went to Harvard College. As an undergraduate, I happened to come in contact with a fellow in the community who was then called a diagnostician, and what we now call an internist. It seemed like very interesting work and I thought that I would want to do that. At the same time, I was very interested and intrigued with chemistry and biochemistry which I studied in college. At that time, I had this notion that I would go to medical school and then get a Ph.D. afterwards. It turns out that I graduated college very young and the medical schools to whom I applied advised me to do it in the opposite direction. So I went to Northwestern and took a Ph.D. in chemistry and then I went back to medical school at Harvard in Boston. This was at the time of the doctor's draft. All through college we were given deferments if we were in the sciences and so on, and as a payback I decided to join the Public Health Service

for two years. After an internship and a year of residency in internal medicine at the University of California, I came here to NIH for my two year service as a Public Health Service Officer.

Klein: Now was this under the Berry Plan?

Rosen: Yes.

Klein: Could you explain how exactly the Berry Plan worked?

Rosen: It's been a long time and one's memory is always imperfect. My recollection was that the Berry Plan arranged for people to be deferred if they were bonafide scientists or physicians who would be able to contribute to the 'war effort' in a professional way after they had finished their training. For me, that was two years in the Public Health Service after my first year of residency.

Klein: What year did you come to the NIH?

Rosen: 1958. I was here for two years doing that military time. I enjoyed it so much I decided to sign on permanently to the Public Health Service who then paid for my last year of residency. I went back to California and was a senior resident in internal medicine. I then came back to the NIH and was here for 35 years after that.

Klein: What position or positions did you hold at the NIH between the years 1963 and 1975.

Rosen: Between '63 and '75, I was a senior investigator in the Clinical Endocrinology Branch of the National Institute of Arthritis and Metabolic Diseases. That was the sole job I had.

Klein: Could you explain the purpose of the Clinical Associates program and why you think it was established at the NIH.

Rosen: The Clinical Associates Program seemed to me at the time and even on subsequent reflections, would have been established to enhance the supply and performance of physician scientists doing clinical research in the United States. I think the people who set up the program and who maintained it saw the NIH CA Program as a seeding ground for sending people out to establish loci of excellent clinical research all over the country, indeed to a certain extent, the world.

Klein: Can you tell me about your interactions with the Clinical Associates while you were here?

Rosen: Well I was one for two years. After that, I was a mentor directly to a Clinical Associate every two years. All senior investigators, as one of the, I guess, the operative word is perks of the job, were able to select from the applicant pool by mutual consent one of the Clinical Associates who would do his or her clinical work within the institute. The laboratory work, however, would be conducted in the laboratory of that investigator. I had a laboratory and the senior investigatorship entitled you to be a lab technician, and a Clinical Associate who was sort of like a postdoctoral fellow. So I mentored some of these people during my tenure as a senior investigator.

Klein: So you spoke with them regularly and you could probably get a feeling as to why they applied.

Rosen: Yes. What would happen would be that there would be a group of them who would apply to the NIH to work here, often as an alternative to military service

because of the distinction of the NIH and the panache and the cachet and the notion that they might want to go off and do similar things elsewhere. So we would get several applicants for the open CA slots and I and other members of the branch would interview them. Then we would have a group meeting and we'd decide who the most attractive candidates were. There is a wonderful anecdote. There was a fellow who applied for a CA position, who came from a lesser well known southern medical school. I don't remember exactly which one. He had a glowing letter from the chair of the department saying that he was the best person he had ever seen. We were presenting this at the meeting and I was saying, 'Gee, that's quite a recommendation.' One of my colleagues said, 'Well, the key issue is how many has seen?' This fellow was actually going head to head with some people who had tremendous records, and actually didn't make the program. The anecdote was interesting because in those days, perhaps less so now, there was a 'seller's market' here. We got, in Halberstam's words the 'best and the brightest' who applied to us. Now, there are loci of excellence all over the country, all over the world and so I suspect from what I hear from casual conversations the general level of talent in the applicant pool has decreased and I don't think that's just my gray hair speaking. I think that is the general perception.

Klein: According to an article in the May/June 1964 edition of the House Physician Reporter, the CA position was highly prized because the two years of service required by the program satisfied a participant's military service obligation. Do you think the program would have been as popular had this not been the case?

Rosen: I think that there is probably an element of truth in the assertion that we would have been less popular. By the same token, I think the number of competing centers of excellence of clinical investigation was sufficiently small so that we would have been strongly competitive. Not perhaps to the level we had with the added delta, the added dividend of immunity from military service, but I think we still would have been quite competitive. I take the point that military deferment was an inducement to some folks, no question.

Klein: Are you familiar with the term *Yellow Berets*?

Rosen: Sure.

Klein: Do you by any chance know how it originated?

Rosen: Well I'm interested in lexicography and these matters and my hunch is it's analogous, that the green berets as these sort of macho, clandestine, special forces operatives in Southeast Asia were called, in contradistinction to these people were the classic sort of more scholarly, nerd-like, less macho, kind of people who were avoiding military service and I guess originally the term was probably pejorative to just contrast, yellow obviously the color of fear. So I suspect that is probably how it originated.

Klein: Was it a joke among the associates and among the staff?

Rosen: To be perfectly honest, I only heard the term much later. But that may be because I was insulated from the world.

Klein: Can you describe the feeling on the NIH campus in regards to President Johnson's Vietnam Policy.

Rosen: I think most of the people with whom I came into contact were less than happy with it. There is a very substantial generation gap I think. I was of the generation that recognized World War II as a veritable crusade to rid the world of the astonishing antics of people who were just destroying all concepts of justice and humanity, leaving aside killing 6 million people. And consonant with that notion was a united country. The numbers of people who were opposed to American participation in World War II you can practically count on the fingers of an amputated hand. There was of course a major rift in the society about Vietnam. I happen to have myself been a member and very active in Americans for Democratic Action a liberal centrist group that banned the Communists from participation in its activities. For me, Joseph Stalin and the Communists were not exactly agrarian reformers. I had serious concerns about Soviet hegemony and all the rest of it, and so from my stand point I did not take that much issue with the notion that there could be substantial truth in the domino theory. Pasteur once said that chance favors only the prepared mind. My mind was probably not prepared to be enormously perceptive to the notions that it was an unjust war and that we should be out of there and so on, when people whose competence and intelligence I respected, including McNamara, made what I thought were convincing cases for our staying in. There was substantial division on the NIH campus and I really can't be more precise than that.

Klein: Why did you want to do your military service through the PHS?

Rosen: The notion of being, forgive the expression, a 'pecker checker' in the military did not appeal to me because I really wanted to do science and research. I felt strongly

that I could contribute more to the national effort, which I felt then and still feel a certain obligation to, again stemming from the World War II ethic. I felt I could contribute more to the national effort doing research than I could as a doctor in some hospital in Vietnam. It wasn't a statement that I disagreed with governmental policies, nothing like that at all.

Klein: I was told that there was a protest outside of Building 1. Did you know about that? Did you participate in it?

Rosen: I don't remember that. That's a window into my soul because I was not nearly as disenchanted with the administration's policies on Vietnam as many others on the campus were. That was probably to a certain extent generational.

Klein: Jane Fonda came to speak on campus. Do you remember that?

Rosen: I do not. I have always viewed Jane Fonda with a great deal of cynicism. In fact, I remember the wonderful comment by Roz [Rosalynn] Yallow [who was awarded a Nobel]. She said once that she was very nervous about people who learned their nuclear medicine from Jane Fonda. I thought that was charming.

Klein: I spoke with Dr. Harry Kimball the President of the American Board of Internal Medicine. He was a CA during that time and he made it quite clear in his interview with me that he did not feel that the NIH was a very political place. Do you agree?

Rosen: Alexander Pope once said that no man's knowledge goes beyond his own experience. The set of knowledge in research is large. The set of knowledge in clinical medicine is not as large, but it's large. I was very focused on learning all

that I could and I didn't pay an enormous amount of attention to much else, so he may very well be right.

Klein: In 1967 *Science* reported, "NIH is different… it really isn't like a government research establishment." However, just two years later *Science* reported that "For better or worse, federal policy making on health matters and therefore on biomedical research is being politicized. And this, as well as the Vietnam War budget squeeze, has abruptly brought to an end the decade of remarkable growth in biomedical research which is already being remembered with nostalgia as the good old days at NIH." What do you think caused this shift in opinion? Do you believe that this view was the general consensus among NIH researchers at the time?

Rosen: There was a politicization of some of the top appointments, the 'War on Cancer' and other such matters. There was also the fact that the quality of the applicant pool was now being diminished by the lack of military deferment and the seeding of other centers of excellence. If we have people here who were superb and we send them out to Mass General and they start programs, then the person finishing his or her assistant residency in Medicine and deciding on a career in medical research is subject to pursuit, blandishments, embraces, locally, and it is a little hard for us to compete. It is a combination of all these things but do I think that the quality of the place fell a little? Yes, I think more in terms of the applicant pool than in terms of the senior investigators. In 1969, I didn't see any signs of major exodus of people, the kind that later came to be a cause of some concern.

Klein: Dr. Kimball also said that he felt that the draft concentrated all of these young, enthusiastic researchers at one institution and that that could never be repeated/ In my opinion it could never be repeated unless we had another war. Do you agree?

Rosen: I take the point and I think it's a fair comment. I think the other thing that concentrated them was that the NIH was the primary cancer, if you will, of superb clinical research. There were not too many other metastases to compete. So I think it was an element of both.

Klein: Then could you say then now, the NIH is shooting itself in the foot?

Rosen: Well I've heard the argument and I can't really use the expression shooting itself "in the foot." I've heard the expression and when I've heard it I've thought it sounded very parochial. One has to go back to the basics and ask what is the mission statement of the NIH. If the mission statement is to make the level of clinical investigation and basic research in the country better, and maybe even in the world better, then how are you shooting yourself in the foot by having good people out there? What you have got to do is ask yourself what kinds of unique things do we have here in the clinical center that they cannot have elsewhere. A concern widely brought to the foreground now by managed care and the loss of support and so on. So are there some things that we can do here that they can't do any place else? You bet! I suspect that the powers that be, composed of many wise heads, are thinking very long and hard about maximizing our strengths. But the shooting oneself in the foot analogy I have a lot of trouble with.

Klein: Did Congressional interference and the Vietnam budget squeeze in any way hinder you ability to conduct top quality biomedical research?

Rosen: No. When I was a senior investigator I was insulated from budgetary problems to a certain extent by my branch chief and then by the scientific director of my institute. I did not have a large laboratory, it was a small operation. So, except for the fact that on a couple of occasions it would have been nice to have a sexy piece of equipment that I wasn't able to get right away, maybe I had to wait some years because of budgetary priorities, the overall answer to your question would have been, not in any major way. But again, I was a small player and I think that one has to look at people who had big laboratories.

Klein: The Congressional elections of 1966 resulted in a dramatic shift in Congress. The NIH lost much needed Congressional support due to what I want to say is hawkish Republican domination. Additionally, this new Congress had a new agenda for biomedical research. They wanted a more direct approach because they thought that this would provide more substantial results in a quicker period of time. In my opinion, the U.S. government was perhaps trying to divert the public's attention away from the failing war effort by providing a cure for Cancer or other harmful diseases. What do you think about this theory?

Rosen: The first thing that comes to mind, I forget what famous figure in history when confronted with a major accusation, pondered and said 'you may be right'. It's the *soupcon* of the conspiratorial history theory. But, sometimes that's correct. I wouldn't feel that I have standing to offer an informed opinion.

Klein: Can you explain why the few government grants that were issued for biomedical research during the mid 1960's, were awarded with the intent that the grantee should produce optimal results in a relatively short time?

Rosen: I don't attribute any Machiavellian intent to that. If I were wearing the hat of a steward of the public money, I would say it is not totally unreasonable to ask the people who use the money, recognizing that basic research is a slow process with long payoffs, to be accountable to feedback and to try to do the best they can. I don't attribute any necessary Machiavellian intent to that. Maybe a little naiveté. The War on Cancer is a classic example, it's nice, it's a slogan that energizes the country, people get involved, you get more funding, but at bedrock there is a certain naiveté about it. I consider it more naive than Machiavellian.

Klein: In my research I came across an interesting quote by Dr. W.N. Hubbard, dean of the University of Michigan's medical school in 1965. He said, "The scientific community fears the price of rigidity that must be paid if this stable support is to be related to a categorical area of research. The unpredictable component in creativity can be readily smothered by a soothing mass of mediocre effort if accounting for time and effort is allowed to substitute even in part for scientific excellence." What do you think about this?

Rosen: That is a fair comment. There is supposed to be built into the system peer review of grants in the extramural community and there is peer review, a different form of peer review, but there is peer review, in the internal community. The internal program is now enormously strengthened by the changes that Harold Varmus is bringing in. He is from the outside and aware of some of the deficiencies. The point of the peer review process was to try to circumvent that kind of outcome and to really critically examine the scientific content of applications and then give the money to those who have the most scientific promise. Take, for example, the

National Heart Lung and Blood Institute. If there were a paucity of really dynamite proposals for looking at sickle-cell anemia, the National Advisory Committee could say, as tax payers, as people interested in minorities and so on, we think that it would be in the nation's best interest to fund a couple of these proposals even though they may not be absolute red-hot science, this is such an important field that it ought to go forward. So there are some built in checks and balances. I think, in general, the peer review system was designed to protect the institution from politicization and mediocrity in grant applications.

Klein: In my research I've found that before the war you have the government leaning towards basic research. Then during the Vietnam War era things change and the trend is towards more directed research. It is almost too coincidental.

Rosen: I hear you, and I go back to my comment that 'you may be right.' I tend to take a, forgive the expression, a less conspiratorial point of view.

Klein: Could you evaluate the CA Program. What do you think this program has to offer its participants, the NIH and the medical community? What influence did it have on U.S. biomedical research in general?

Rosen: As far as the participants are concerned, having been one, it was the opportunity to see patients in a particular specialty area and gain clinical experience. But more importantly, to be involved in taking care of patients either from one's own research or when was serving the larger group. For example, when I was a CA in the Arthritis Institute and was on the ward, I wasn't just seeing the patients I was studying, I was seeing the clinical investigations going on with other people's patients and how different investigators approached clinical research. One was

gaining a certain amount of experience by seeing broadly how a number of people approach areas of clinical research and that kind of broad experience was useful. From the point of view of the CA, you have the opportunity to be exposed to specialized medicine, see first class senior clinical investigators with their approach to research, and to do your own clinical investigation with a mentor in whose laboratory you were. As far as the NIH, if you think that one of the mission statements of NIH is to raise the level of clinical investigation in the country, I think from that standpoint it was good. We seated a number of people elsewhere. From the standpoint of the medical community, if you think that the medical community should value and does value clinical investigation, then I think the CA program in contributing to a higher level of excellence of clinical investigation served not only the Clinical Associates and the NIH, but the entire medical community as well.

Klein: What influence do you think it had on biomedical research in general?

Rosen: If one could get the names of the people who have been through the program and somehow follow their careers you could see what type of impact their careers have had on biomedical research. My hunch is you can finger a number of really classy, first class people who have come out of the Clinical Associates Program. My biased answer to that would be strongly positive. I think there might be objective ways of seeing its impact. Perhaps look at people coming out of Residency programs, not having been at the NIH and going into research and look at people coming out of the NIH and use some objective measure to compare and contrast.

Klein: It seems today that the number of CA applicants has dropped dramatically and I wondered why that is the case since former CAs are now holding high positions at the NIH as well as at other esteemed institutions all over the country?

Rosen: I've thought about his a lot. My analogy, and I've used this before so it's not coming to me like a great epiphany, is the analogy of courtship. I am a young and impressionable assistant resident wanting to go into medical research. I am a person who is of a marriageable age. The number of people, who marry people close by, is a hell of a lot higher than people who marry somebody a thousand miles away. If I'm at "St. Elsewhere" hospital, and there is local guy who is an NIH trained guy and he sees me around and says, 'hey you look good, I think you could be a dynamite researcher.' There is a certain romance going on and I am much more likely to be seduced to stay in that program around there under the influence of that proximal mentor, than I am to pick up my marbles and go from St. Elsewhere to Bethesda and come to a strange place. The NIH, in the old days, was the only game in town, it was a hell of a game, and you got military deferment. Now, there are lots of games around. Who wants to come to Bethesda? There is a certain culture now that says 'Oh the government, I don't want to deal with interference'. So, I it's mostly a seduction issue.

Klein: So what can the NIH do?

Rosen: I think what we can do, especially in this era of electronic communication and so forth, is first of all have a very clear understanding of what we do better than anybody else. And then, get the message out there! Send your best and your brightest to the various places and try to attract young people to our program. The

NIH has some clinical investigators that are astonishingly good--send them out there! But get the message out, get it out electronically, get it out in the news ads. We are doing that to a certain extent with the ads that come out of our Office of Education. But you always run up against the classic whore-Madonna dichotomy. On the one hand you are a sober, sensible scientist. You are very objective. On the other hand, scientists are people like everybody else. They get grabbed just like everybody else. A little more aggressive, creative marketing would be good, provided that what we are creatively, aggressively marketing is the uniqueness of the institution. This is the Mecca of clinical investigation. If it has fallen off, as it has, it is only because we've colonized places and you get local seduction. But I think Mecca, like the self, can rise again.

Klein: Even without a war?

Rosen: Yeah, I really think so. If you want to do long term patient studies, doing that on the outside is extremely expensive. But here you can keep people in your stable. The other fantastic thing that I used to in the seventies use as one of my selling points to people who would come through is, that no matter what you are doing, no matter what new thing you want to try, there is someone in the next lab upstairs or downstairs who has been doing it for years and may even be the world's expert. You are never stymied to start an adventure. It's so true, talk about the critical mass. Now there are great places of excellence on the outside, but the combination of the critical mass, the freedom, the internal system that protects you from the grant meshuggina, all of these things can be the subject of a good

marketing campaign that doesn't put down the outside institutions but toots our own horn a little bit.

Klein: When do you think the NIH started to lose some of its greatness?

Rosen: For me there was a watershed year because I went away on sabbatical '75-76, so coming back in '76 I became aware of it. It may have been going on before but I didn't notice.

Klein: What did you notice in '76?

Rosen: Two things. A diminution of the quality of the applicant pool of the CAs and a sort of, disrespect is too strong a term but that's the first word that comes to mind, on the part of the basic scientists for clinical investigation. The advances of basic science had been fantastic, no argument. But one of the missions of the NIH, particularly the clinical program at the NIH, is to translate these basic advances to advances at the bedside. I had the sense that a number of the scientific directors in their zeal to buff up the basic science program were giving the clinical program a somewhat short shrift. Maybe they would argue the clinical program here was not so different than clinical programs on the outside, we were getting less good people etc. etc., but I find that as sort of a self fulfilling prophecy. In any event that was a source of concern to me and I think that there are people here who can probably speak more eloquently than I because I left the lab and the bench in '83 to join the administrative staff of the Clinical Center.

Klein: Is there anything else you would like to comment on?

Rosen: I think you've covered it comprehensively. I guess the one thing I could say is in

my branch it was a very democratic system. We did not do monolithic research on one particular topic with different people feeding into it. It was very democratic, very autonomous. We went where the science led us and it was a wonderful feeling of intellectual independence to know if I had something very interesting come up in the clinic or in the lab then I could follow that. I mean obviously it is resource constrained, but at least no one was saying, ‘hey that’s not what we’re here to do.’ Now that may be dinosaurian but at the time it was a source of great reinforcement.