

Dr. R. Cohen
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EAR What I want to do is look at the decision making process that took place over the 25 years. I really am going to stop as of 1971. And I have already talked at considerable length to Bob Felix, I spent 2 full days with him in St. Louis. I spent a full afternoon with Dave Shakow. I just spent about an hour and a half with John and we just began really. And I spent a number of hours with Stan.

RC Unfortunately, I have to leave.

EAR I know you have to leave at 3:30. So we are just barely going to get started. But I will come back. And I really want to get some of the key people to tell me in their own point of view some of their experiences and their insights as to what happened. In your case, of course, you have been here from even before the beginning of the intermural program. So what I would like...let me say again I am not going to write a history of the intermural program. As I told John, it seems to me in the total history of the first 25 years the intermural program is very important because on the one hand it was the most important bridge to the relationship of the NIMH with the NIH, and at the same time served as a kind of internal stress because the intermural program was not fully concent with the extent of the NIMH extramural program. I think in that sense it is important. It is important, of course, in its own right. But in terms of the total NIMH program it plays a unique role. But maybe in the few minutes we have you could talk a little bit about the early days and what your role was and how you saw the early development of the intermural program as it related to the total NIMH.

RC Well first I could say my own acquaintance with what was going on then in the very early days of the Institute was not very great. I met Bob

RC(cont) Felix first when he reported to Washington and he had joined George Raines and Frank Braceland. He had given the examination for the American Board of Psychiatry and Neurology and I was a member of Frank Bracelen's team and later George Raines took over that team. Bob came out to participate in giving the examinations with the rest of us. I had known Larry Cobb very well, we met originally at Johns Hopkins. He was in neurology and I was in Psychiatry and we were both taking a brain monotor course by Adolph Meyer in 1937. And then in 1941 when I was already on active duty at Lanork Naval Hospital he reported for active duty and we were around the same service there for about a year or so. And then he ...after the war we shared a part time private practice office together while he was actually working half time at the Institute. So that I heard a little bit of some of the ideas about the beginning of the program. And then I was Clinical Director at Chestnut Lodge and Al Stanton and Morey Schwartz got the ..one of the early grants from the extramural program. I was one of the people who was referred to the book on the mental hospital. I participated as a subject and object study there. And I remember several times at NIMH when Bob Felix came out with some of the members of the staff to tell me, a few of the things...all of us a few things that were going on in the Institute and than in addition to that I became acquainted with him because we were both Green Acres parents. Our three year old daughters were in the same nursery schoolclass. And so we would meet in an entirely different context and got to know each other in a variety of ways. I suppose that the thing that might conceivably had a little bit to do with their coming to.. with Bob Felix asking me if I would come here was the fact that I had just become a training analysis at the Washington Psychoanalytic Institute

RC(cont) and in addition to that did have a Ph.D. in physiology. And on the one hand it made me a little more respectable just to have an _____ and Wade Marshall was already here and as a matter of fact in our graduate school days Wade and I had shared a laboratory. He was building a cathogrey oscillogram to study nerve conduction and I was studying the effect of asphyxia nerve conduction galvanometer. And sometimes I would have it black and he would twiddle his thumbs and then he would turn it white and I would twiddle mine. While we proceeded with our projects...so that I had the personal acquaintance with some of the people. And as a matter of fact I was also the Chairman of the Program Committee of the Washington Psychiatric Society. And for one of the meetings I had asked Ralph Gerard who was my old professor and my wife's old professor to come for a talk and then with Seymour Kety who was just here and invited him to come to discuss Gerard's paper. Bob Felix spoke to me in July of 1952, just before I went on vacation about my possible interest in developing a clinical research program. And a week before I went I came down here on 2 days and spoke to Seymour and talked to Wade Marshall.

EAR Seymour had been here about 2 years.

RC (~~cont~~) Seymour had been here about a year a little over a year. At that time and he was intermittently here actually. I think he was still sometime going to Pennsylvania for some things. And then to John Clausson because John had been at the Institute for Juvenile Research not at the same time I had been there. I had been there for a year in 1939 to 1940 and knew Clifford Shaw and several other people who were there at the same time. Among them Harriet Reingold who ultimately came to join out intermural program. Any event, I

RC(cont) met in those days everybody at NIH. That is they took me around to meet the scientific directors who were on board all the Institutes. I knew Wilder who was then Director of the Arthritis Institute his younger brother had been a student in physiology when I was an assistant there and he had originally come from the University of Chicago. And I met Shannon who was then the Scientific Director of the Heart Institute and Norm Topping who was the Associate Director and Sebral the Director of the Institute and talked to them about what they had in mind and in a sense really found that as far as psychiatry was concerned and particularly as far as clinical psychiatry was concerned nobody has anything in mind. As Bob Felix in effect offered me the beds that were going to be available at the clinical center and a budget and a number of positions which he assumed would increase. He felt that the budget would probably reach about a million dollars. It was \$500,000 at that time and when I asked what speculations they had had or thoughts they had about conceivably they might be interested in having done here or what the discussions had been that had gone in to the development of the psychiatric facilities here. He said that there were none and this would be a matter that would be completely up to me, if I were to take the job. But obviously they had some thought that there would be children here because they had set up one area with small size toilets so children could use them. But that was the only special equipment ordered. I knew from Larry Kolb that they had moved the psychiatric area from the place that he had strongly recommended. He had recommended that it be on the ground floor in the area where the out patient clinics were. Because he felt it would be important

RC(cont) for patients to be able to move directly from the ward out-
doors rather than having to go through elevators in the center.
And so I having spoken to all of the people and found out in
effect that they were an interesting group of people and found out
that they had no ideas whatsoever about what they anticipated would
be done. I don't mean any of the potential people. I had met Lou
Gatchoff was on the staff he had been ^{sent} off by the Public Health Service
to get analytic training in Chicago. He had come out a time or two
to Chestnut Lodge to visit while I was there and since we had mutual
acquaintances within Chicago in the Psycho-analytic Institute I got
to know him there and G. Ambage also had been sent off to get some
training in child psychiatry and they were presumeably going to be
on the staff when it was brought together. But otherwise there was
no staff available, these were the staff that had been accumulated.
So, I would have complete freedom. I thought about it a good deal
over the summer wavering back and forth for a variety of reasons-
wavering forth because in a way it seemed to be an ideal situation,
not an ideal setting. In that there seem to be the promise of
complete freedom to select areas in which one thought were important
to study and apparently they were going to provide complete freedom
to carry out the studies in the way within the resources that were
available. One felt that one would have particularly in those days
there was nothing that began to compare with it. And the other thing
I think that made it attractive to me, I suppose, is my history which
from one point of view gives me a certain you might say gives me a
certain view from another point of view it is also in a kind of a
sort of discomfort because even though I got to be a training analysis

RC(cont) and Chairmen of the education committee and Director of the Psycho-analytic Institute for a term of office as I was actually an officer of the Institute for a sum of twelve years and also served as the President of the Washington Psychoanalytic Institute. The fact that I had had the background in physiology made me less than a completely devoted and dedicated analysis. That is one that is in a sense too much aware that there are other things that aren't considered in this theory and in a sense perhaps less able to become thoroughly imbedded and involved in the work. And as a matter of fact I didn't because I did practice part-time, I was at Chestnut Lodge part-time where they were really trying to extend some psycho-analytic thinking and theory and in one direction and at the same time I wasn't a completely dedicated biologist because I was too much aware of what I felt was of important. Certain psychological aspects which I felt were not to pursue. And then my experience at IJR sort of brought me into much closer contact with sociologists and psychologists and actually most settings were most people in either psychoanalysis or biology ever had occasion to do. And then the experience at the Lodge I think we were all very much impressed by the Stanford Study and what sorts of things came into it. And in addition to that my wife had become editor of "Psychiatry" after Sullivan had died and we use to discuss the papers that would come in and I was the person who refereed with Erving Goffman's paper and he was later introduced....as I later introduced him when he was here on the staff for awhile my favorite purveyor Harry Schurtz the Psychiatrist. Not that I felt he either had the whole story but it seemed to me that he had a all to penetrating idea for some of the things that went on that we tended to ignore

RC(cont) and to me at least was persuasive and indicating that we were important and considered significant. And so in a way I think that what the Institute seemed to offer me was an opportunity to indulge in all these interests. I sort of day dreamed of possible... first of the importance of clinicians of really trying to apply the same studies that they carried out on the patients to individuals who were not patients because we simply tended to project backward from the current situation and the things we identified necessarily gave rise to the psychopathology without ever having an opportunity for similar contact with others and an opportunity to bring also to the clinic, if you will, the biologists and anthropologists and psychologists who for the most part were studying the normal but really had not considered the pathological behavioral disorders. And way back in my physiology days, as a matter of fact, the first lecture that I had given --Kleitman was the professor at the time. I was an assistant in his course and he had me give a talk on the physiology of emotions and found that the...I could read about all that was written in an hour. I could have condensed it in the lecture to less than an hour but I had the whole hour to take up and began to be rather intrigued and as a matter of fact had sort of approached psychiatry with the idea of trying to speculate on whether some of the studies in which I did my thesis on brain metabolism could conceivably be tied to mental patients. And so from that point of view this seemed to be a way of doing it. The reservations that I had related largely to the setting because my ideal of a place where you brought psychiatric patients was not a building like this. It looked really terrible when it was empty to and these long, long corridors and cold surroundings. The fact that

RC(cont) in the early days they were affraid of bringing-particularly bringing psychiatric patients down through the center lobby and so the nurses had done a trial run of how long it might take to get children out of the building outside and the fastest that they could do it was nine and a half minutes as I remember. And so it seemed to me that in a sense if someone said lets go out for a walk that is what it would take in order to get people bundled up, herd them in a group that could be closely supervised, take them down a long corridor, wait for an elevator, get on, get out, etc. So that it seemed to me this is ...wasn't a ..the ideal clinical setting and I wondered seriously if that wasn't important that is I feel that it is in terms of some sorts of things at least. And then I think the other thing that I was concerned about was the salary sclae of the year before I came I had made 30..my income for that year was \$39,000. But they were offering \$15,000 with the statement that you couldn't do private practice. It wasn't a particular problem for me in a way, not that I was so well of or not that I wasn't concerned about money but my wife was also in psycho-analytic practice and so between the two of us we really had an ample amount of money even with the \$15,000. Our children had just started school full time so that she was moving from the half time to a three quarter time practice and so it was feasible thing to do. And we...what I didn't know is how feasible it would be in terms of recruitment. I did go around and speak to people actually I approached people who were millionaires first. It might be easier for them to come then someone who was the bread-winner in the family. So I spoke to Jerry Frank to see if he might be interested. I spoke to Bill Meyer to see whatever he would conceivably be interested. And

RC(cont) they, so though they were interested but not really but it was such a new venture and so strange and the idea that you weren't even suppose to participate in teaching much and I think also the fact that one would have to engage in full time research was a rather frightening prospect to someone who hasn't been whose life hasn't been a research life. I think that this holds a lot of problems and I approached a few people who....although this was after I actually decided to come finally, who had done research and who I thought might conceivably feel more comfortable about devoting themselves to full time research. George Saslow like me had a Ph.D. in physiology and a M.D., he was a non-psychoanalytic psychiatrist but I really wanted to have people from various schools of persuasion. It seemed to me that we could be a happy family with quite different ideas deciding who was..what was right. In a way, I later found out that nobody was as much interested as I in finding out what was right. At least not those that were established figures. I also spoke to Leon Saul who had been working in, infact he and Wade Marshall tried to do some papers...he had worked with Hål Davis at Harvard and then came over to the department of physiology while he was at the same time in the Chicago Psychoanalytic Institute. And he wasn't..he actually came here and we met with Shannon and discussed possibilities. But he decided that he wasn't and went out to see Jergen Rouche and he thought that the idea of leaving San Francisco to fly to Washington. Even under the delightful circumstances would be offered and for the salary we offer would be utterly and completely ridiculous. I went over to speak to Sarah Tower who before Mabel and I got our Ph.D.'s in physiology as a matter of fact, she was a contemporary of Ralph Gerard's and Margaret Gerard and had gotten a Ph.D. in physiology

RC(cont) and had taught in the department of anatomy and then had gone into psychoanalysis. Thinking that she might be interested in research and an opportunity, but she wasn't. And so the net result really was that it became clear that it was not going to be possible to recruit established figures in the field of psychiatry and my first plan had been to try to bring the psychiatrists together and then to move out from there to the other disciplines. And so in a sense what I did was to completely reverse tracks. I had been speaking to Dave Shakow about his suggestions to the psychologists that I might conceivably approach. I knew quite a few psychiatrists because during the last year and a half of the war I had been in the OSS selection program with Harry Murray, Jim Miller and Elliott Star and Mo Stein and a whole group of others who had been...Hobert Morrow and Ed Valachi and others. So I kept up some contact with them and had been very much impressed by Dave and in those days we were thinking just of psychiatrist...psychologists for a clinical program particularly. Seymour had been trying to get an experimental psychologist to come and approached a few of them. He had gone out to Wisconsin and tried to get..

EAR Harry Harlow ?

RC Harry Harlow to come

EAR Who was this?

RC Seymour

EAR Oh, Seymour.

RC He was looking for those people

EAR Right

RC And in general what Seymour was trying to do if you will, to get people who were going to work at the organ or tissue level. And I was looking for people who were going to work at the organism or

RC(cont) organ level. And we agreed that there would probably be some overlapping but the clinic..anything connected with the clinic would be mine. And so I kept talking to Dave went out to see Mo Stein and spoke to Elliott and spoke to a number of the others and found that they weren't completely...and then went to see Seymour with the proposition that why don't we both approach Dave Shakow and get him to have one department of psychology and if we did that we might get someone who is more distinguished and Dave indicated some interests in doing that. And he actually was my second appointment my first appointment was Fritz Radel who was quite interested in coming. I was quite interested in having him because his approach to hyper-active children was very similar to the one that we used at the Lodge for psychotic adults and in addition to that because they were children you had to prescribe an educational and an activity program that was geared to their capacities as opposed to the sorts of programs we had for adults. And Fritz in his own way had some theoretical speculations about how these things should be integrated. And so it seemed to me an extension of analytic theory. He had worked with Ike Kline early on and was a very good friend of Erik Erickson and you know Fritz, I guess a little bit and so that he came. And then Dave agreed to come and in the meantime what I tried to do withphysically in psychiatry was to get...we were getting ^a lot of applicants because of the war and people had come into the military service. Early on it looked like we would even get some fairly distinguished people who had the misfortunate not to have been in World War II that is Al Stanton said he would come and Gil said he would come but then before I actually got on the scene, before the clinical center opened Eisenhower

RC(cont)went over and stopped the war and so they all called up and said we don't really have to do we? And I said of course not and so they wished me well and I wished them well and started with a younger group and what I tried to do is to get people who in a sense, had been like myself that is who were in clinical psychiatry for the most part as residents they hadn't even completed residency many of them. But in addition to that had other kinds of experience, Normal Goldstein came had a Ph.D. in bio-chemistry and he had actually completed his residency in neurology and completed his residency in psychiatry and had served for 2 years and so on. And set him up in what we called the laboratory of psychosomatic medicine and ~~Evarts~~ because I always have a special affection for because on the first afternnon, December 30, 1952 when I was here wondering why in the world I had ever come he wondered in my office and said he had heard about the program that NIMH was going to develop and at that time he was a resident in psychiatry at Cornell and we chatted a bit and he said he thought he would be interested in coming and his wife Josephine Simms was with him and she would be interested in coming. And so I thought well gee if he might come. Well Ed had worked in experimental psychology and had been down at Yerkee's lab for a while. Rikoff had been at Chestnut Lodge with me before he went into psychoanalysis. He was a graduate social worker and actual social work supervisor and then went to medical school. Lyman Wynne was in the second year of psychiatric residency but he was also working for his degree in the department of human relations. Goodrich came around he was more or less straight psychiatry at the time but had written a sort of practical pocket hand book of psychiatry with George and seemed like a promising fellow.

EAR Sy Perlin?

RC Sy Perlin didn't come until considerably later.

EAR Dave Hamburg?

RC Dave Hamburg didn't come until 1957. And so we started that way. I set up 3 laboratories. There was no branch chief in psychiatry. We had gotten a branch chief for psychology and a branch chief in child research. And Norm Goldstein was the acting branch chief in what we called the laboratory of psychosomatic medicine. And then in addition to that John Claussen was already here. He was under Seymour and so I suggested why don't we do the same thing we had done with Dave. I will put money in that laboratory and then they recruited Bill Cardil then for this position. I had known Bill previously because he visited the Lodge. This was the time he had been admitted as a patient to the Yale clinic for the study he did it was the same time as the
was going on. So we got quite well acquainted.

EAR Did Seymour begin jointly as scientific director of both neurology and.... ?

RC No, he was the...there wasn't a....I gather that what they had tried to do originally was to work on that basis that Seymour would be Scientific Director for neurology and for mental health. And then they found that I suppose, before they approached me that they couldn't get any clinical neurologists or clinical psychiatrist who wanted to come and work for someone in that capacity. At any rate, they never offered me that. Seymour was the director of the so-called basic program in neurology and in mental health and my job was set up as parallel to his as clinical director in mental health and Milton Shy was set up parallel as clinical director in....and so while Seymour went to the meetings of the Scientific Director and neither Milt or I did. Whenever issues came up about the clinical programs we

RC(cont) would go to them to represent ourselves and in an effect I reported directly to Bob Felix as did Seymour and then he and Milton Shy reported directly to what's his name...the director of neurology...Pierce Bailey. And then actually I tried to do was recruit a psychiatrist and I literally I guess saw some 25 people and made serious efforts to get them and have many of them come here and was turned down and by that time I think we...they agreed to let me go a little bit lower than people who were already full professors. I know Dave Hamburg was for some time and we first had to get permission to approach him because at that time he was at the ~~Think~~ Center and he had been offered Associate Professorships and they still weren't spattering the full professorships all around and then assistant professor and in effect I was offering them a professorship here. I suppose that also the thing that nobody else ever came, that is nobody came to the program. As far as I know, at least in those days, I was the only one who came for less than he was getting. Seymour was , of course, a professor in the biological department which in those days didn't pay asmuch as we were getting. Dave Shakow wasn't getting as much as we offered him. Fritz Radel wasn't getting as much as we offered him. And in a sense we were forced into what I think ended up as being a much wiser thing. In 1955 we...Norm Goldstein went back to Mayo clinic in 1954 and in 1954 I had gone abroad for the first trip and met Joel Elkin and was very much impressed by him because it seemed to me that much more than I, well all of these fellows as my physiology had changed in 15 years that I was getting psychanalytic training and requiring some skills as a clinician. Well Joel then was aware of more current physiology and pharmacology but also it seemed to

RC(cont) me that he had a sort of a interest in the social or behavioral sciences as well, more than any biologist. And so we had him come over here and he actually accepted the job of chief of the laboratory of the what we then called the laboratory of psychomatic medicine. And then he went back and we didn't know it at that time that the medical research council had just set up the Arthur Cohen clinic for him

Side 2

I had been impressed what I had seen of Joel and also had been impressed that he had come to spend 4 months some time ago in the United States instead of going to Harvard or Yale had gone to the Norwalk State Mental Hospital in Connecticut. And so I thought that he seemed to be a person who really would have an interest and he talked a very good line about this interest and working in a real state mental hospital. And I must say he did some marvelous things. As we took him around and showed him the space, Felix carried the day that we were going to have the whole damn building and not a ward. And we showed Joel the building and gave him a set of plans and gave him a government requisition booklet describing the laboratory equipment and furniture and in 2 weeks he sent back a complete sketch with everything filled in including the numbers of furniture and items to be requisitioned. And so finally in 1957 5 years after we started, Dave Hamburg came in September of 57. Joel Elkes did Seymour was here John Clausson was here Dave Shakow was here and Fritz Radel and in a sense what I felt was that I sort of assembled a group of people and I thought that what would happen would be that we would...it would be sort of a senior seminar this is what I look forward to as my the thing that I would be able to participate in. And would

RC(cont) be interested in developing a more powerful theory of behavior that we would meet and discuss issues and that the research that would be carried out would ultimately flow from these sorts of considerations with that group together.

EAR Is that what happened.

RC That isn't the half of it. But a lot of exciting things did happen. But in a sense that is how it got started and the vicissitudes of that you probably are more or less aware of.

EAR External not internal.

RC But it was a congenial group actually no one ever got appointed without considerable consultation with everyone who was here before him. Because I had felt that it wasn't just ideas that naturally flowed together but the people had to have mutual respect and liking if they were going to talk together. And it looked, as a matter of fact, I think we had on the other hand the world was just exploding in a sense the Institute which Bob Felix in one expansive moment had thought might ultimately have a budget 50 million dollars. And the things that made these people attractive to us made them very attractive to everyone else and I suppose that one other thing is I think, would be an interesting speculation. I am not sure, but the fact that they mostly left might not have been a good thing because the fact of the matter is that very few of them I guess with notable exception in a way had been Dave Shakow and John Clausson but even John Clausson had less than he had formerly, really became very much involved in bench research that I think they became director-stimulators in research. That is not completely true, Seymour did do the study with Dave was a full participant, never the less had really not a principle active. And I started to have the feeling that maybe

RC(cont) the people who made their reputations here who might not have had as fully a chance to develop if we hadn't sort of pushed them into a position because we couldn't recruit anymore. The 15,000 by that time was up to 18,000 was at the time we were getting people was close enough to...was actually more than they were getting but then the people who...when they left got infinite more. Dave Hamburg went from 18 to \$35,000, Sy Perlin went from 14 thousand to 35 thousand. I don't know what John Clausson salary actually was at Hopkins and I guess it was not paying quite as much as that but some where in the near 32 to Seymour and then when he came back Joel had first accepted the Stanford job and then changed his mind and then he came and got Dave Hamburg.

EAR What was Seymour's reason when he came to see you about taking a position at

RC I think that he found it rather boring and frustrating to be an administrator of two programs and mental health, etc. And I think he also felt that the people were getting ahead of him that in a sense he wasn't at the bench in that job and all the people who he was getting to know more about chemistry, etc. He thought he might be more comfortable where he was closer to an operation.

EAR You have to go

(NOTE) Dr. Cohen continued for a few minutes to talk about the intramural program and the fact that it had grown because it had been free of any pressures or strains from the outside and because, for whatever reasons, they had been very fortunate in getting the kinds of people that they did get who subsequent to working at the intermural program at NIMH went on to other important positions elsewhere and he felt that it was experience at NIMH which probably served to prepare them for these positions of responsibility elsewhere. This includes people

EAR(cont) like Dave Hamburg and Sy Perlin.

Dr. Cohen also mentioned the fact that Hazel Rae as the Administrative Assistant to the intramural program was responsible for overcoming a number of potential administrative difficulties by finding ways to get monies for a new activities that were not clearly defined in the existing administrative guidelines. Dr. Cohen ended by commenting that they had hoped somehow that the entire program would work as a continuing seminar whereby new ideas would be interchanged and that new research knowledge would be developed by this kind of interchange. Infact, of course, that isn't quite what happened. And the people that did move ahead in their respective areas of investigation did so primarily on their own independent basis as not as a result of some very effective inter-disciplinary activity.