LEADERS IN AMERICAN MEDICINE: AOH/National Library of Medicine Film Series, January 1974.

ENGEL

Dr. John Bowers:

The biographical memoirs which you are about to see is one of a series Leaders in American Medicine. The series was inspired and supported by the late Professor David Segal and his wife, Professor Beatrice Segal. It represents a joint activity between the National Library of Medicine National Medical Audiovisual Center and Alpha Omega Alpha. The subject of our film today is George L. Engel, Professor of Psychiatry and Professor of Medicine at the University of Rochester School of Medicine and Dentistry. He is interviewed by Sanford Meyerowitz, Associate Dean for Medical Education at Rochester and Associate Professor of Medicine and Psychiatry at that institution.

Dr. M: George, your career has been really productive, incredibly productive. In trying to size it up, it seems also extremely broad ranging, diverse, such a large number of different topics and areas you have gotten into. It's hard to know what the threads are that pull it together. As I look at it you have touched all three bases; being a clinician, an investigator and a teacher. I think you have also really distinguished yourself by looking at each of these processes, teaching in itself, clinical investigation, an activity in itself, and also your scientific investigation which has gotten you into chemical, physiological, psychological areas. How did it get to turn out that way? What got you into medicine and what do you think led to the career unfolding the way it did?

Dr. E: Well I think maybe you ascribed a little too much to me in terms of how deeply I have touched many bases. Sometimes half in jest, more than half in jest, I let my internist friends think that

I am an excellent psychiatrist and my psychiatrist friends think that I am an excellent internist. In a way that touches on something because my own feeling is that I have touched many bases but I am not really expert in any. There is a reason, I think, why I have found myself in this somewhat unusual position of being involved in a great number of areas, why I have avoided ascribing a particular identity to myself in terms of any categorical area, while at the same time have aspired to accomplish something in a great number of areas. Your question as to how I got there isn't an easy one to come to grips with and I think I'll not try to respond at this point in any systematic way.

Dr. M: Well how about just getting into medicine to begin with.

Dr. E: Some of the issues will come out as I come along. Getting into medicine was almost inescapable. I was brought up in the home of my uncle, Emanuel Libman, a noted physician, pathologist, clinician of his era, that is the first 45 years of this century. Along with my two brothers and my parents I lived in his house. From my earliest life the expectation was that one or more of us would go into medicine. Indeed as a small child visitors would say, "Are you going to be a doctor like your uncle?" My older brother, Lewis, resisted. He refused to have anything to do with medicine, he said. Actually he went on to get his Ph.D. in biochemistry and ended up at the Harvard Medical School nonetheless, eventually becoming Chairman of the Department of Biological Chemistry.

Dr. M: All three of you went that route?

Dr. E: My twin brother, Frank, ended up in medicine as well. The aspects of expectation were profound and I think the influence was there right from the beginning. It was my parents' expectation and it was my uncle's worry that I would go into medicine. That was one of the interesting conflicts I had with him. He was always concerned that his nephews would disgrace him if we went into medicine. But his library was there, his pathological collection was in the basement, there were visitors who came by from all over the world to see him, I learned names of many noted medical figures of the day. But there was also another aspect of this which I think was very important, namely there was a great deal of illness in my family.

Dr. M: I didn't know that

Dr. E: Yeh, my mother was chronically ill with many kinds of complaints. My older brother, Lew, had multiple mastoid operations which were a significant part of my childhood because this surgery was done at home. I remember the bathroom being prepared as an operating room, the smell of ether. But even at that time it was impressed upon us the significance of the people who were involved in his care. For example, this was just towards the end of World War I, I was about four years old, one of the treatments of wounds was the so-called Carrel-Dakin solution, actually sodium hypochlorite, ordinary bleach. And Alexis Carrel, eventually a Nobel Laureate himself came over from the Rockefeller Institute to administer the treatments.

Dr. M: You talk about the expectations and the great figures that the kids encounter, I am including you and your two brothers. But what about the kids, you don't tell us what they were thinking about. Yourself and your brothers, well yourself, we can't speak for your brothers. How did you feel about, what did it mean to you, these expectations and seeing these figures?

Dr. E: Well, it was a complicated situation. There was a story told of my older brother, who was four years older than I. Libman used to be proud of him and have him come in to meet these people, but he would get very annoyed at being called away from play. The story, apocryphal or not in the family, was that at one point, I think he was about six years old, he was brought in to meet some distinguished pathologist from Vienna and his response in my uncle's office was suddenly to start shouting, saying, "I'm crazy, I'm crazy". Then he left the room and that was the last time he was ever called in to meet any of these people.

There was an interesting kind of conflict that went on with us because Lewis very early interested himself in chemistry. From the time he was about ten years old he said he was going to be a chemist. Many of the members of the family thought he was going to be a pharmacist and have a drug store, but he was serious about it. He started a chemistry club with three members. He, Frank and I were the other members. We used to carry on chemistry experiments in the bathroom with chem-craft sets and so on.

There was a great pressure, and aspiration, it wasn't just an external pressure, it was an internal pressure to aspire to scientific eminence. I remember even as early as 12 or 13 years old going into my uncle's library and pulling down textbooks of medicine and trying to decipher them. When I went to college at Dartmouth I very early got involved in scientific matters. My brother had already gone to Harvard and had majored in chemistry, and when I started college he was starting graduate school in biochemistry at Columbia. And he put a tremendous amount of pressure on us to foresake medicine, to be "real" scientists. There were many conflicts, some quite serious, between us, because physicians were looked down upon in my older brother's eyes as very unscientific creatures. Dewitt Stettin, a well known figure now, and my brother were contemporaries and friends and Stettin and Lewis used to belabor Frank and myself on this. At one point we both seriously thought about going into mathematics, which would really one up him.

Dr. M: Even purer than he.

Dr. E: Even pruer than he. The idea that I would go into research and do research was a strong thread so that by the time I was 18 (I started college early, only 16) Frank and I together applied to Woods Hole Marine Biological Laboratories as volunteers. Incidently, it became very important for us around this time to dissociate ourselves from our uncle. I think it was mutual. He was also eager not to sponsor us in any way and not to have any connection with us. This went on for a long, long time. It actually went on virtually his whole life. But both of us got summer jobs at Woods Hole and this was a crucial and important experience. I got a job working in a laboratory with Ralph Gerard, who at that time was Professor of Physiology at the University of Chicago, a neurophysiologist. He was a relatively young man, must have been in his middle thirties, although to me he seemed an old man. He got me started on a study of metabolism of lobster nerve. It was a great experiment because we ate the lobsters. We took out a few nerves and cooked the lobsters. I also worked with muscle. When I finished that summer's work and wrote the experiment up and sent it to Gerard, he was very upset about it. He said it was wrong because it came out not as he had predicted it would and he made me go back the next summer and do it over again.

Dr. M: What happened the next summer?

Dr. E: The next summer it came out as it had the first summer. Actually it was, using very crude and primitive methods, one of the early demonstrations of the role of ATP, except then it was called adenylpyrophosphoshate. It was years later I realized that adenylpyrophosphate and ATP were the same compound. It was published in the Journal of Biological Chemistry in 1935. As far as I know it has never been read by anybody. I got 50 reprints, I autographed and sent one to each of my brothers and one to my mother and I guess I sent one to my uncle.

Dr. M: I was wondering that

Dr. E: Yeh, and I still have 45 copies

Dr. M: You and your twin brother Frank then both applied to and went to Hopkins together.

Dr. E: Yes, we went together all the way through school. Because of my brother's illness, my mother became phobic about us going to school and catching infections and what not. This of course was long before antibiotics. So it ended up that until we were 12 years old, we hardly went to school at all. I think this was a very important thing in a number of ways because it threw us together in terms of literally self-education in those years. Our family got a little bit panicky when we were getting to be about 11 years old and still hadn't gone for more than three or four weeks or month or two to school any year. They got a tutor and we did all of the grade school work in three afternoons a week that one winter and entered high school at age 12. I think the importance of that, aside from the extent to which we were thrown together, was that it generated a great deal of self-educational activity, the pressure for which came from not only the family atmosphere but particularly from Lew who constantly was expressing concern that his younger brothers would be illiterates. I remember his pressuring us to read Dickens and Scott and other kinds of literature which we were reluctant to read at that age.

Dr. M: Well you know that is interesting because later on you never follow a conventional career track. You never went through any of the usual training period or you never concerned yourself about specifically being absolutely certified or identified as one of the tracks that most people in academic medicine go through.

Dr. E: Right, that's correct. I went to medical school at Hopkins and internship at Mt. Sinai. Frank and I were together.

Dr. M: What made you both choose Hopkins?

Dr. E: That was Libman's counsel.

Dr. M: He was still behind the scenes then?

Dr. E: He was still behind the scene and that was the only significant occasion that I can think of in which he intervened in an active sense. I remember him recommending several people whom I should see for interviews over at the Rockefeller Institute. I was rejected at the other medical schools I applied to. I won't mention them now. But he thought Hopkins was the best school. I am not sure it was, as a point of fact, at that time, because during the summer of my third year I went up to the Boston City Hospital and I was enormously impressed with how much more lively, exciting and interesting Harvard was.

Dr. M: Well what were these years now when the year you entered medical school?

Dr. E: I entered medical school in 1934, graduated in 1938. It was the summer of 1937 that I went to the Boston City Hospital. But I am very glad I went to Hopkins for one very important reason. That is where I met Evelyn. She was a student in art as applied to medicine, studying under Max Brodel. I first saw her across a line of cadavers; very romantic.

Dr. M: She took anatomy with you?

Dr. E: No, she was in another room with the art students and the medical students were elsewhere. I was a very shy and inhibited adolescent at that point and it took me a long, long time before we made any contact and then that was because someone arranged a blind date for me. He thought it was blind, but it really wasn't.

But to come back to your question of avoiding a defined career, this touches on another issue which I think is important in how I have evolved. That has to do with my twinship, because being a twin also raised very important issues about identity and being differentiated from my twin. It became extremely important for me to be different, not to be confused with anyone else. Because actually we were so identical that our parents confused us repeatedly, our friends confused us and this was true our entire life. This continues to this day even though Frank has been dead for ten years. I am still called Frank by some of his former friends or former students.

Dr. M: And here you were still together in medical school.

Dr. E: We were together in medical school, we were together in our internship.

Dr. M: You came to New York for your internship.

Dr. E: Came back for internship. But the summer in Boston was a very important one in terms of my subsequent career because that is when I first encountered Soma Weiss. Soma Weiss was then at the Thorndyke at the Boston City Hospital. I had heard about him. I had read some of his work. He was one of the real pioneers in the development of clinical investigation, really the application of physiology to patients in an active sense. This appealed to me very greatly. It tied in with my first interest in physiology which started with Gerard. During medical school physiology seemed to me to be the pathway, more so than biochemistry at that point. So in that summer Frank and I both went to the Boston City Hospital and it was a very exciting experience. And I was determined at that point to come back and work with Soma Weiss if I possibly could. He was the attending part of the time. I had contact as a student, with a number of other very noted figures. Hale Ham was a fellow, Franz Inglefinger was the intern when I was there as a student. Keefer, Jackson, Jones, ---

Dr. M: It sounds like one of the turning points, one of the phases of excitement in medicine when a number of people get together usually around a figure and they are all going to take off from there..

Dr. E: The young people, the interns and the young fellows; Dick Ebert was an intern. He subsequently became Professor of Medicine at Minnesota. Gene Stead, many of these people were there just beginning at that time. When I came back to Hopkins for my fourth year Hopkins seemed very dull to me. I probably was a pain in the neck because I kept talking about what went on in the Boston City Hospital. There was one memorable occasion when I made the diagnosis of beriberi heart disease about which I had learned from Soma Weiss at the Boston City Hospital. Weiss' paper had not yet been published. I presented this case on rounds to Longcope, the Chairman of Medicine, and he was utterly mystified at what I was getting at. Finally he said, "For heaven sake, Mr. Engel, what are you thinking of?". I said, "Beriberi heart disease". He was a very mild mannered man but he excoriated me for such a nonsensical suggestion. If it weren't for the fact that Barry Wood was the assistant resident and had been at the Boston City Hospital that same year, I would have been in trouble. But Barry backed me up and we gave the patient thiamine, it had just become available, and there was a very dramatic response. I am not absolutely sure now that that was the correct diagnosis. I think this was more likely an alcoholic with cirrhosis with the wide open circulation that one sometimes sees in cirrhosis, which is probably not beriberi heart disease.

But be that as it may, what I am trying to communicate is the impact of that excitement of physiology. And this was coming at a time, and I think this is important in terms of my own development, this was coming at a time when clinical instruction was strong; clinical observation, physical examination, attention to clinical phenomenon. It was my aspiration to put these things together. And I had the heritage of Libman as a superb clinical observer.

The experience with Soma Weiss also was important in inculcating in me an investigative attitude towards everything I did. This happened to Frank too. When he came to Mt. Sinai we both made our daily work a scene of investigation. A number of very important things happened. The principle is just as valid now as it ever was, namely, that at the bedside you can make observations of an investigatory character, of a research character, without equipment, without instruments and pursue them to productive results. For exampl I learned about the carotid sinus from Soma Weiss. I introduced stimulation of the carotid sinus as a routine part of every physical examination in the course of which we discovered that there were certain clinical conditions in which carotid sinus, hypersensitivity particularly the vagal type, was prominent.

For example coronary artery disease, gallbladder disease, obstructive jaundice, acute rheumatic myocarditis. We actually published in The New England Journal and

Dr. M: You and Frank?

Dr. E: Frank and I, a paper on carotid sinus sensitivity in gallbladder disease. That appeared in 1942.

Dr. M: Of course you went on later on to a whole series of studies about syncope, going beyond the carotid sinus, looking at all the mechanisms systematically.

Dr. E: Yes, right

Dr. M: And you kept at that for a good number of years.

Dr. E: Kept at that for a number of years. I started the first studies of syncope at Mt. Sinai as an intern just by paying attention to the history of fainting, which I got very intrigued with. I used to measure blood pressure lying, sitting and standing on everybody and turned up all sorts of interesting things. It was also at that time that I got involved with electroencephalography, and again that was purely by chance.

Dr. M: This was at Sinai?

Dr. E: This was at Sinai. Much stimulation came from a very memorable patient, who is now long dead. He has been reported five times as five different conditions by five different people. I reported him once. He was a patient with Addison's disease and was one of the first patients treated with adrenal cortical extract by Rowntree, who was then in Philadelphia. He appears in Rowntree's series. But this patient came into the neurological service when I was an intern on that service and I made what proved to be the correct interpretation, namely that hypoglycemia accounted for his periods of unconsciousness. This led me to get EEG's. Up to that point epilepsy was the main condition for which EEG was thought to be useful. We discovered he had abnormal EEG's even when he was not hypoglycemic. My brother Lewis was working with George Thorn at that time and he had a supply of desoxycorticosterone. Frank and Lou Soffer, who was an endocrinologist at Mt. Sinai at that time, were studying one of the early series of Addison's disease patient treated with DOCA. So we got EEG's on them and then discovered that the EEG was regularly abnormal in the Addison patients. But this was one of those decisive serendipitous events which changed my career because, tho I didn't know it at the time, we were studying varieties of delirium. We extended the studies and demonstrated abnormal EEG's in a number of medical patients who I now realize were probably delirious. I had the excitement and also the trauma of reporting this at the Academy of Medicine in New York, at a meeting of the New York Neurological Society, and having my uncle sit in the front row. He was a white-faced man at best, but I was terribly anxious about this my first presentation. I was misled by Israel Wechsler, who had arranged for me to present this paper. He told me I had twenty minutes to deliver the paper but when I got to my feet the Chairman told me I had ten. You can imagine the occasion was a shambles, I spoke too long and the Chairman called me down and my uncle told me I was a disgrace and that my career in medicine was at an end. Actually Wechsler was a real gentleman and he explained it to the chairman and then arranged for me to give another paper a month later. But this opened up what in fact was the area of delirium and when I went to the Brigham to work with Soma Weiss

Dr. M: You left Sinai in New York in the early 40's, 41. That's when Frank and you probably split and took separate paths.

Dr. E: That is when we took our separate paths, although I had gotten married in 1938, which was indeed a separate path. But professionally we were together until then. He went to Yale as a National Research Council Fellow and worked with C.N.H. Long in Physiological Chemistry. He continued working on the adrenal. I was involved with Addison's disease, Frank got involved during the war in work with the adrenal cortex with C.N.H. Long, and Lewis was in steroid chemistry. He had of course preceded both of us in steroid chemistry by quite a few years.

Dr. M: And you had had it in your mind to get back to Boston and work with Soma Weiss.

Dr. E: I had in mind to get back to Soma Weiss, not just to Boston. Where Soma Weiss was was the place to go. I think probably the most disappointing day I had was when I arrived at the Peter Bent Brigham Hospital and went in to see Soma Weiss to discuss my plans. He asked me what I was interested in. I had clutched in my hot sweaty hands several manuscripts which had not yet been published. They had to do with EEG work. Soma Weiss called me in two or three days later, after looking over this material, and said the man I should work with is John Romano. Well at that point I had no idea who John Romano was. To be told after all of the effort I had made to get to the Brigham to work with Soma Weiss that I was not going to work with him after all but with John Romano was a terrible blow. Even worse, I learned that John Romano was a psychiatrist. At that point I had little interest in psychiatry, although I must say I had been significantly touched by Adolf Meyer at Hopkins, a man I greatly admired but didn't understand. I had the feeling Meyer's heart was in the right place and he must have some good ideas, that what he had to teach must be important, but I just couldn't grasp what he was getting at.

I remember my first encounter with John Romano. He kidded me about it for many years. I walked into his office with a copy of Muncie's textbook of psychiatry. Muncie was one of Meyer's men. John asked me what the book was. I showed it to him. I remember he made a face of great disdain. I had just bought the book because I thought I had better learn something about psychiatry. I never opened the book. I put it aside and I never looked at it again.

At that point John was in his early thirties, 31 I think, I was 26-27. When I discussed with him what I had been doing John, who had been working on delirium clinically, pointed out that I had been working on delirium also, only I didn't know it. I had brought a physiological method, namely the EEG. So we paired up and together evolved a study of the EEG of delirious patients. Our only problem was we had to get an EEG and that was a difficult thing. I heard that Forbes in Physiology had an old two channel machine which he said I could borrow if I could make it work, which seemed impossible. I had no idea about any of these instruments. I remember carrying it by myself across from the physiology lab over to the Brigham. On the way I tripped and dropped it. When I got it into the lab and set it up and plugged it in it worked! That's when I first learned how you make electronic equipment work, you kick it.

But it was a very inadequate piece of equipment; months went by and we did no work. During that time I think I read every piece of literature on delirium. I finally got very unhappy and I went back to Soma Weiss and I told him it was worthless, we weren't getting anywhere, I was wasting my time. Soma got very distressed about this and immediately offered to put me on another project which had interested him, namely to study the arrhythmias that were developing in patients having pneumonectomies. These were the first pneumonectomies being done by Churchill. But just about this time, however, the EEG that we had ordered from Grass arrived and John and I began our study. That was in December, 1941. The first experiment we did worked and we were in business.

At this point John accepted the appointment as Professor of Psychiatry at Cincinnati and was to move in the spring. He invited me to come, which I declined because I was not interested in psychiatry. I went to Soma Weiss to ask his advice as to what I should do. He recommended that now that we were at war I should go into the blood fractionation program with Edwin Cohen and drop the other project. But before anything could be implemented Soma Weiss died suddenly; he was only 42. He had a subarachnoid hemorrhage. So I was left hanging in mid-air not knowing what I was going to do. I assumed I would go into military service. It was at this point that Eugene Ferris appears. Ferris had worked with Soma Weiss on the carotid sinus in the early thirties. He came to Boston as one of the people who delivered an address on the occasion of the memorial service for Soma. We met one evening. It was in Gene Stead's apartment. It was one of those extraordinary circumstances where two people of very different backgrounds, Gene coming from the Missippi Delta around the Vicksburg area and I coming from the streets of New York, immediately clicked. I don't really remember what we talked about, but something clicked between us and it was very soon after that, maybe a month, that he invited me to come to Cincinnati in the Department of medicine. I accepted. Then John renewed his invitation to come in the Department of Psychiatry which I also accepted. It was by that route that I became an illicit psychiatrist for I received a faculty appointment in psychiatry at Cincinnati without ever having had any formal training in psychiatry. My role actually was to be a consulting internist.

The Cincinnait period was probably the most formtative. It spanned four years, the war years of 1942-46.

Dr. M: Very productive years

Dr. E: Extremely productive. The main task was an aviation medicine project involving high altitude decompression sickness but we also got into antimalarial drugs and a variety of other things. It was a very exciting time with an extraordinary group of people. I came to Cincinnati I would have to say as an eastern snob, from Harvard, Hopkins, Mt. Sinai, all those distinguished institutions, and I really must confess to having had a slightly contemptuous attitude toward this place Cincinnati. But in point of fact it turns out that the group at Cincinnati in the 40's was the most exciting group that I have encountered anywhere before or since. They included people like Arthur Mirsky and Gene Ferris in Medicine; John Romano, Milton Rosenbaum and Maurice Levine in Psychiatry; Charlie Aring in Neurology; Albert Sabin, Sam Rapaport, Ashley Weech and George Guest in Pediatrics; Bill Bean was also there part of the time. It was a small, intimate group. We were all involved with each other in teaching and in research. It was in this setting that my opportunity to diversify was realized.

Dr. M: I assume it was here too that you first really came to grips with the interface between medicine and psychological medicine.

Dr. E: Yes, that came slowly. John Romano was very wise in that he never pushed me. He let me develop at my own rate. I resisted psychological matters for a long time. When I was in Boston I had become the consulting internist at the Boston Psychopathic Hospital (now the Mass. Mental Health Center) and I used to go over there, like many internists do, eager to catch the psychiatrist missing important things. I did the same when I got to Cincinnati. That was my first approach to psychiatry, I going on the psychiatry wards and demonstrating clinical findings that the psychiatrist had overlooked. But little by little as we became involved in our research work together, Ferris, Rosenbaum, Romano and others, the interface began to enlarge. This process was a very interesting one. Gene Ferris was a very significant figure here. There was one very important thing that Gene did which was a turning point for me. When we started the decompression sickness work, I think there were six or seven or eight laboratories during the war also working on high altitude decompression sickness. Nobody really knew anything about decompression sickness before the work started. Each lab started out with some theoretical notion, generally predicated on the physics and physiology of intravascular bubbles. I remember the first experiment we did in Cincinnati, which was to measure the pressure in various parts of the body during high altitude decompression. We found exactly what Paul Bert had discovered back in the 1860's. At that point Gene called us together and suggested that instead of doing physiological measurements without really knowing why we are doing them, why don't we start out as if we were in the middle of Africa and were encountering a new disease. Because that is what we are doing. None of us had ever seen a patient with decompression sickness. Why don't we behave as clinicians, why don't we just go into the decompression chamber ourselves and just do clinical observation. Let's just observe everything we can see and see if we can make any sense out of it. That is what we did and it worked.

Dr. M: It paid off.

Dr. E: It paid off because whereas none of the other laboratories could make any pattern out of who developed bends and who didn't, by simple clinical observation we quickly discovered that one man, the man who read the manometer in the decompression chamber, always developed bends in his knees. What did he do that was different? He kneeled. Every two or three minutes he had to kneel down to read the manometer. From that point on we discovered we could produce bends virtually 100% of the time simply by having people do deep knee bends. This was a major breakthrough in the research because it meant that you could now really test denitrogenation and other measures to prevent decompression sickness, as well as get a handle on how to approach physiological mechanisms involved in the disease. But much more important in terms of my development was that this sold me on clinical observation as a legitimate and fruitful scientific method.

Dr. M: Including behavior, you were watching what that guy was doing. And I assume that it had something to do then with making it possible for you to overcome your own stubborn resistance to the psychological

Dr. E: Yes, that came gradually Milt Rosenbaum played an important role here because Milt, in conrast to John, used to gibe me about my resistance. He used to refer patients to me for medical judgements. One day he called me up and referred a patient with head pains. Only this time he said he wanted me to do psychotherapy. When I protested that I had no competence he accused me of being unpatriotic. The patient was an engineer in an important war production plant and if I didn't help him the plant would be in trouble. When I still demurred he asked me whether I didn't think that I could do better than another psychiatrist who was known as the most incompetent person in town. I of course protested that I should even be compared with such a man and that was that. And that began my interest in pain. I took care of this patient for more than a year. Milt supervised me. I made my first presentation to the psychosomatic conference, I think it was in 1944, and my colleagues were astonished. They didn't even know I was interested in this area. It was after that that I began to see many patients with pain.

Dr. M: A whole series of studies about pain.

Dr. E: A whole series of studies evolved from that. Joe Evans, the neurosurgeon, referred these patients to me, most of them.

Dr. M: By the time you moved to Rochester you were really, you had overcome your resistance to psychological things and were really getting into psychological approaches. I might add that you have been as stubborn and as tenacious in locating and identifying and studying the role of psychological factors ever since as you were once stubborn about not seeing them.

Dr. E: Yes, the move to Rochester represented a very important, not just professional decision but also an ideological decision. This related to the structure of the psychosomatic conferences in Cincinnati. Actually these conferences were by far the best psychosomatic conferences of the time and maybe of any time. But they had one characteristic which was disturbing to me. Each conference was conducted by an internist or a neurologist and a psychiatrist. After while it struck me that what they were saying to the students was when you grow up we expect you to do what it takes two professors to do. So that was one spur for me to leave, to try to evolve more effective teaching approaches. There were many other inducements. John was a very dynamic and exciting teacher and figure. I had become very attached to John as a person, to John's way of thinking. I was very intrigued with his capacity to interest people, to involve people. That and the prospects of good facilities and support at Rochester were appealing. The final thing that settled it for me was that when I came to see McCann, who was then Chairman of Medicine and indicated my wish to be in the Department of Medicine because I was not a psychiatrist, he accepted that. But there was an interesting administrative issue which evolved around that which was fateful. McCann was very happy to have me in the department and he was a very strong supporter but he was delighted to have the Department of Psychiatry pay my salary, or much of it and provide office space. As a result my primary appointment was in Psychiatry not in Medicine. Yet my only psychiatric experience had been what I have described up to this point. Thus by administrative fiat I became a psychiatrist.

Dr. M: Yes, you were never trained in psychiatry although you began psychoanalytic training after you came to Rochester.

Dr. E: When I came to Rochester one of the ideas I had in mind in respect to the psychosomatic perspective was that if it were not possible for me to do this as an internist by myself then maybe it couldn't be done. In other words, I recognized, even though at that point I didn't really understand the concept of identification by the student, that the student has to identify with one person. He can't identify with two disciplines at once. I felt if I could incorporate this discipline in my one person it should work; if I couldn't do it then I almost felt as if it couldn't be done.

Dr. M: Well, you did it and you helped a lot of other people do it because I think that when you came to Rochester another major phase evolved, that is the development of the Medical Psychiatric Liaison training group. I don't know how many of us have been trained in that now through the years.

Dr. E: About 100 I think

Dr. M: Up to 100 and not only did you work through that it can be done but you helped a lot of other people find this out. Most of the people that came into it were internists who left with a feeling of confidence that they could deal with psychological factors and medical factors.

Dr. E: Yes, we came with the idea of developing a training program. As a matter of fact the training program originally was to be in Cincinnati; it was to be supported by the Commonwealth Fund and Rockefeller Foundation, but we transferred it to Rochester. At the beginning in Rochester I had great trouble establishing myself as an internist because everybody assumed that I was a psychiatrist, as they did Pete Hamburger who also was an internist.

At that time in Rochester there were only two psychiatrists in the department besides John, so I found myself in the situation of having to do psychiatry without formal training.

Dr. M: Until John could build this department.

Dr. E: Until John could build the department I was the only new full-time person. Dick Jaenike had been there for many years before we arrived and the three of us manned the department. That meant that in those early days I was making rounds in psychiatry. I can assure you I was barely a hair's breath away from the residents. They didn't know that I was also making rounds in medicine with the house staff. I also made rounds with the students and we had our own fellows. We established a clinic in the department of medicine which we called the special medical clinic, which was really the first out-patient psychiatric clinic. Over the next number of years we attracted a variety of people into the program, internists, obstetricians, pediatricians and a certain number of psychiatrists like yourself whose interest were in these areas. It meant extending myself over many dimensions simultaneously. I got interested in psychoanalysis toward the end of my Cincinnati stay because it seemed to me to be an extraordinarily productive and promising way of understanding human behavior. And so I began psychoanalytic training. When I arrived in Rochester I began analysis with Sandor Feldman and later on began traveling to New York and then Chicago for psychoanalytic trainin I was looked upon I think somewhat dubiously by the analysts because they weren't sure what I was. I bluffed my way through the Boards in Psychiatry. I never wrote down that I had a residency but I did write down that I had been an instructor of psychiatry and assistant professor in Psychiatry at Cincinnati and then Assistant Professor in Rochester. Somehow or other they didn't even notice that I never said residency training. I bluffed my way through the exams including one comical incident when the examiner asked me to discuss the EEG and delirium. That helped!

Dr. M: He didn't know who he was dealing with.

Dr. E: He saw my name but I am sure he thought that it couldn't be the same person.

Dr. M: That is the same theme again that you, you don't do it the conventional way.

Dr. E: No. No sooner do I get in one setting when I want to move into something else lest I become too identified. This I think, as I said earlier, goes back to the issue of my twinship.

Dr. M: Trying to figure out ways earlier on

Dr. E: To be unique

Dr. M: To be yourself and not just part of a pair

Dr. E: To be unique, not be confused with anybody else. I think that has been an important underlying motif which is complicated and confused but also has been a tremendous impetus for development.

Dr. M: You and Frank differentiated in terms of both having productive careers and remained distinct right up until the time of his death.

Dr. E: Yes we differentiated in our early professional careers, when Frank went to Yale and then to Emory and finally to Duke. I went to Harvard, then to Cincinnati and then Rochester. During the early part of our careers we were very rivalous, but as the years went by, as we each achieved success in our own areas, we came closer and closer together. We used to meet every year at the Young Turks in Atlantic City and review where we were and so on. That was our main contact with each other and our work overlapped on a number of occasions. Shortly before his death we talked about collaborating for the first time since internship and actually had some things lined up.

Dr. M: And there was the blow, he died in 1963?

Dr. E: He died in July of 1963, very suddenly.

Dr. M: Very sudden, I remember it was quite a blow. You got yourself going and doing things. You got sick about a year later.

Dr. E: I had a coronary a year later and there were many intimate connections. I have written about the anniversary phenomena which involved me and pursued me in relationship to his death. I think this has been another aspect of my approach to work. It has also been self-analytic. I have been as much interested in trying to understand my own behavior and venture to make it public at times with some hesitation.

Dr. M: Recently you have become more and more thoughtful about what you have been doing all these years. Medical education, you're beginning to look at this systematically. Is it a new direction? I don't know, I think its

Dr. E: No, it's not really a new direction, it's really a continuation. Right from the beginning, and it goes back to the Brigham days, my interest in teaching medical students has always been paramount. My first interest and my first love has been teaching medical students. That has been true all along. I have always enjoyed teaching in many different kinds of settings. It has been a challenging experience to try to introduce another perspective than that which has been traditional. Its been a long effort and struggle at times. We've met all kinds of opposition but I do think that now after 27 or 28 years at Rochester there is a certain flavor to the Rochester ambience and curriculum which I think comes from the

Dr. M: Yes. People talk about a certain quality the Rochester graduates have which I think is fundamentally the clinical orientation and ability to relate to patients, to people.

Dr. E: Yes, now I think it has been important too that our research has always been clinical. It has always involved patients. Not only has it always involved patients but to a very large degree we have been able to involve and integrate research into our teaching. The studies of the child with the gastric fistula, Monica, for example,

Dr. M: The famous Monica study, how long have you followed Monica?

Dr. E: 20, 21 years now. The child that we saw originally at 11 months, left on our doorstep literally, with a gastric fistula and depression, we now have followed into adulthood and marriage and she has a child of her own.

Dr. M: You're making observations about her relating to her own child now, her mothering.

Dr. E: Right, but we've involved the students in this first at a research level. We had nine or ten students in the '50's who were involved with us in the study in various ways. Franz Reichsman has been involved with me in this study all of these years, along with Vivian Harway, a psychologist, and Bill Hess, a psychologist; also Dane Prugh for a shorter time. This study has played a very important part in the development of some of our psychosomatic perspectives, as with respect to separation and loss. The whole group as you know has been involved in that area. Bill Greene, who in 1948 was a Fellow, began to work with patients who developed leukemia in a setting of loss. That was truely revolutionary work in that period when no one thought of considering psychological factors in diseases like leukemia and lymphoma.

Dr. M: It took great courage.

Dr. E: It took great courage and he was criticized for it.

Dr. M: Well you know I started off at the beginning saying it seems like you have been into so many things and it has been so diverse. But you know I think as we talked today I can begin to see the common threads. They are quite clear.

Dr. E: Yes, there is a common theme that runs all the way through and it has to do with I think of understanding human behavior in relationship to illness.

Dr. M: Yeh, I think as I have heard you talk today it becomes much clearer. I hope maybe we could make it clearer for others too that it's an important theme and a common theme that has characterized your whole career.