

Charles Brenner, M. D.

Interviewed by Edward Nersessian, M.D.

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Q: Dr. Brenner, the bibliography in Volume 53 of the *Quarterly* shows the wide range of your contributions to psychoanalytic theory and practice. But in recent years three of your discoveries seem to have the most impact on theory and practice. These are the ideas about the depressive affect, the role of compromise formation in normal and pathological mental life, and ideas about the superego. I wonder if you could summarize these ideas and tell us how they evolved.

A: Well I'll do my best, Ed. They evolved back in 1972 when I was asked whether I would participate in a day-long panel discussion or symposium that was going to be held in Boston. It was organized by Dr. Berezin, who as you know is an analyst who is very much interested in geriatric psychiatry. It was about the subject of depression. And I welcomed the idea because I had something that I felt I wanted to say about depression. But I wasn't quite sure exactly what it was at that time. And I did go up to Boston -- spent the day -- and I gave a paper which was eventually published, I think, in the *Journal of Geriatric Psychiatry* a few years later. It started me on organizing the ideas that I had about the role of depressive affect in psychic conflict, really. And . . . I don't know how to say it exactly.

See, I had been naturally following the ideas that Freud had developed in *Inhibition, Symptoms and Anxiety*, that psychic conflict is initiated by anxiety, and tried to see that in the material that the patients brought. So if a patient was depressed and if it seemed as though depression went back to childhood -- that is current depressive affect went back to or was related to experiences in earlier childhood - then I tried to say, well it's really anxiety that initiated the conflict and the depression is somehow a substitute for it. Or maybe feeling depressed is to avoid being aware how anxious you are. Well I never felt easy with that because the evidence from the patients wasn't quite that way. I remember . . . .

Q: The depression, orality and depression, that was never something you found very useful?

A: Well, see after 1959, when I published that paper on the masochistic character, it seemed -- and I had reviewed the literature, particularly the contributions of Fenichel and Greg Rochlin on depression -- it seemed to me that they were just shutting their eyes to what their material showed. I mean Fenichel had this case of a woman who was very depressed in later life and her depression clearly went back to the time when she was, I don't know-two and three-quarters, or something like that. And a sibling was born and she felt that her father didn't love her anymore because she was a girl and not a boy. And he [Fenichel] said he recognized the importance of the penis envy and the depression over it that the girl had. But he added, "But of course it must have been oral." And Rochlin similarly had these cases where it was very clear to him that when his patients (these were patients who were cyclically hypomanic, or at least elated, and

depressed, and they were in analysis -□they weren't that sick, you know, they were analytic patients) and whenever there was a fantasy that they were castrated, then they became depressed. And whenever there was a fantasy that they were - whenever they felt that they were phallically potent, then they were elated. But, he said, of course, it must be oral. And I couldn't quite see that. And you know it was obvious to me that they were more influenced by what had been said by Abraham and Freud before than what they actually observed in the patients that they had treated.

So I was also influenced, I remember as I talk about it, I remember Erma, my wife, told me that years ago when she was working in Boston with some little children in an analytically oriented nursery school setting -- that was the James J. Putnam Nursery School, which was the first psychoanalytic nursery school, as far as I know, in this country at any rate -- and they discussed the case of a little girl who had a sibling, or maybe a couple of siblings, and was upset with each one. This was a four- or five-year-old girl. And the mother had promised her she wouldn't get pregnant again, and she didn't have to worry about any more babies coming into the family. Then the mother got pregnant and either had the baby or the child knew that she was going to have a baby. And this child got very depressed. Not anxious, but very depressed, very unhappy, miserable. That, of course, is just anecdotal material. But the analytic material that was available, both in the literature -- because Rochlin and Fenichel had the best cases on this subject in the literature -□and what I was able to observe in my own patients persuaded me that depressive affect was something that was related to psychic conflict from the beginning, from very early in life. And that the suggestion that I had made back in 1950 when I first presented that paper that was published in 1953 on anxiety had more merit to it than I was somewhat persuaded it had at the time. That is I wasn't at all sure that it had so much merit at the time.

What happened was I gave the paper to the Society in 1950 and it created quite a stir because for a reason that I hadn't expected at all. I included in the paper the idea that the "Aktualneurose" -- the actual neurosis -- wasn't a valid diagnostic entity. And I thought that was accepted by everybody. Nobody said anything about "Aktualneurose", hadn't for years, and I remember talking with Kris about it before I gave the paper, while I was writing it.

And he said, "Well of course there's no such thing anymore. We don't believe there's any such thing anymore. " So that was just in the paper as part of the discussion. I didn't even think that anybody would question that.

Well they certainly did. Of the people who got up in the discussion I remember Abe Blau particularly. And in the course of the next couple of years there were two or three papers published to try to prove that there really was such a thing as actual neurosis. What they'd all seen and what they wrote their papers about weren't "actual neuroses" at all, though. They were traumatic neuroses; mostly cases they'd seen in the service [WW 2]. But somewhat they'd got them confused with what Freud called "Aktualneurosen."

Anyhow, I gave the paper then and there was, as I say, quite a lot of criticism of that idea in it. And Hartmann discussed the paper and he said, "Well, you could say if you

wanted that the affect or emotion that the little child felt in the early traumatic situation was unpleasure. And that the term 'anxiety' should be reserved for a later state of development when in fact danger and ideas of danger, the future, memories of the past and so on, were available to the child. In other words after a certain amount of ego development." But he thought it was better to use the term "anxiety" even for this very early time because it emphasized the genetic relationship between that and the later states of anxiety. And Hartmann was pretty influential and he and I - and Kris, for that matter -- were quite good friends you know and I looked up to him and respected his opinion. So I didn't submit the paper for publication right away, though I did submit it finally after the papers of Blau and others that were published had cited it.

But I dropped the ideas about anxiety and unpleasure that were in it and never went any further with them until 1972. Then, after that meeting in Boston, going back over it and on the basis of my own experience with patients and trying to say to a given patient that well, you think you were depressed but you were really anxious; you think you were unhappy but you were really anxious, not just unhappy, I plucked up my courage and said to myself, "Go ahead with your ideas about depression and develop them." And doing that led also to the formulation of a theory of affects, you know, and that I was really very pleased with. I hadn't expected that to come out of the discussion of depression, but it did. And I think that's valuable too. But, leaving that aside, as far as clinical work is concerned, I think that there's no question that you are doing better justice to the facts if you pay attention to what form of unpleasure precipitated conflict in a patient's early childhood instead of thinking that everything is anxiety. I mean I see it over and over, every day with patients. And of course particularly with patients in whom depressive affect is a prominent symptom and a troublesome one in adult life. To find out what it was that was responsible for so much unhappiness as contrasted with anxiety or fear. There's always both as I tried to emphasize in what I wrote. But to try to find out about the unhappiness, specifically, and understand it, I think leads to a better understanding of psychic conflict altogether. And in particular to an understanding of the psychosexual development of girls that was quite amazing to me. I mean when I developed it and wrote it out, it came out so simply and so straightforwardly. I'm sure it's right, I really think so.

Q: Of course in practice, once you think about it, you see it just as much, or you see it quite frequently, in men. It's not something that you would . . .

A: . . . limit to girls. No, not at all. But I don't think you can understand penis envy, what we call penis envy, or a girl's oedipus conflict, without taking into account the role of depressive affect. I don't think you can understand it correctly in either men or women without taking into account the role of depressive affect in conflict. But it is so certainly that a severe degree of depressive affect as a part of mental illness in adult life is much more common in women than it is in men. You know, severe depression, psychotic depression, the usual ratio that's given is two to one. So that makes some sense when you think of it that way, when you understand why, or you have some idea as to why that should be so. But those are all problems to work on, to get the material about it, and so on. This is after all based on my own experience, it is never going to be enough to even begin to answer many of the questions that come up. But if it raises the

questions, and gets other people working on it, pooling their findings and so on, then I think some of the questions can be answered.

Then you asked me about the superego, right? And about...

Q: Compromise formation.

A: . . . compromise formation. Well, compromise formation, that's not as original an idea, I suppose. Since analysts, or maybe all analysts to some extent, had recognized pretty well that normal psychic functioning sometimes involves conflict just as pathological psychic functioning always does. Nobody ever did it as systemically or as -- what would be the right word? -- rigorously, or maybe, conscientiously. So I said to myself, "If that's what you think, then see where it takes you and try to decide whether you think that it's really so." And, of course, I think the answer is, unquestionably, "It's really so." And when you pay attention to the fact that it's really so, instead of just sort of realizing it and accepting it without paying too much attention to it, then I think you come to the conclusion, not only that what's pathological in mental functioning shades into what's normal -- which was an idea that I brought out back in 1955, you know. That's what I wrote in the Elementary Textbook, which was published then.

And, while again, it wasn't exactly original, it wasn't exactly accepted at that time, either. After all, most people who are in practice today have at least read the Elementary Textbook and very many of them, in this country at any rate, and elsewhere as well, had it as their introduction to psychoanalysis. So to them it seems well, that's the way it always was. But it isn't exactly the way it always was. And so as I started to say, it's not only that you realize that there's a gradation, but that you realize that conflict and compromise formation are an inevitable part, or a ubiquitous part, of mental life, from very early in childhood, certainly, to the very end of life. And that the difference between what's normal and what's pathological is, as I explained in the book, is how much difficulty is caused by the conflict, not whether there is a conflict or not. And really, you know the early idea was that if a person was normal there was no conflict. There might be repression, you see, but no conflict. The conflict came only when repression failed. And so conflict was pathological in mental life. Well, that idea gradually eroded. But it was never actually given up, explicitly, by anybody as far as I know, until . . . do you know of any?.

Q: No. I'm trying to think that even when I had my training at the Institute, that reading Freud that was a very central idea.

A: Sure. Right. And nobody contradicted it. But if you talked about clinical cases or so, they'd say well in this case, you know, it's a normal outcome of conflict. See, instead of a pathological one. Or they say conflicts are sometimes valuable, lead to good results -- you know, that sort of thing -- as well as bad ones, so you can cope better with a conflict. Very vague and unsystematic and unsatisfactory formulations of that sort. So as I say it wasn't actually new. Anyhow, now the superego

Q: May I just ask you a question?

A: Sure, of course.

Q: How do you put that together with Hartmann's ideas about the autonomous functioning of the ego?

A: Well, you know I was part of a study group that ran for many years -- Jack Arlow, Dave Beres, Martin Wangh, and myself. Well, we spent a lot of time, of course, discussing what Hartmann, and Hartmann and Kris, and Hartmann and Loewenstein wrote. And in particular about autonomy. And it is clear what Hartmann had to say about primary autonomy was very important and without any -- I mean you couldn't question it, in my opinion at any rate. That is that mental functioning isn't to be understood as something that just develops from. frustration. That there are sensory and motor apparatuses that are genetically determined and so on, and that are part of mental development, part of the equipment. So that's the primary autonomy. But the secondary autonomy always was troublesome to me, you know, and the more we discussed it the more we agreed that we wanted to find out from him what would he say about it. And we arranged a meeting with him, and Rudy came along, because Hartmann was by that time beginning to fail as far as his health was concerned. And Rudy was always very protective of him as well as extremely admiring and so we invited the two of them for dinner and the six of us had a very nice dinner. And then we went up to, I guess it was either Dave's or Martin's apartment in the city and sat down in the living room and started to talk. But it was just a little bit too late. Hartmann wasn't up to really discussing these ideas in any rigorous way as he would have been able to do five or ten years before that. Because he was a person who intellectually was very able, a very brilliant man, and he would have -- if we'd got to it a little earlier, I think we would have had a very interesting discussion. As it was, the whole thing just really fell flat.

But I think that emphasizing secondary autonomy and explaining it by neutralization is, in the first place, wrong because neutralization is an ad hoc explanation, it doesn't have anything to support it, that is except the speculative idea itself. It's just a speculation. And for the other thing, the more you think about it, the more you realize that what psychoanalytic data have to say about normal mental functioning is how much of normal mental functioning in later life -- later childhood, later life, and so on -- is influenced by and determined by, childhood instinctual conflicts. Not the other way around. I mean, everybody who isn't an analyst -- and a lot who are - is ready to talk about how different adults are from children. And how when you're normal these childhood conflicts no longer influence you. And only if you're sick they do. And so on. That, you don't need psychoanalysis to say. What psychoanalysis has to say -- when you have somebody on the couch -- is that you see how pervasive, ubiquitous, and inextinguishable the results of those conflicts are on mental functioning. And to emphasize the neutralization, or the secondary autonomy, whatever word you want to use for it, is really going at the whole thing wrong end to, it seems to me. What you want to emphasize is just the reverse.

The superego ideas, the relationship between masochism and normal superego functioning comes out of a 1959 paper on the masochistic character. I remember that the objection to that idea that Loewenstein raised was one that he expressed -- I think he wrote about it somewhere or other, though never at great length -- but anyhow his idea was that superego functioning, that is, bowing to the dictates of the superego, being, if you will, puritanical and so on, but in any case, moral, wasn't to be thought of

as related to masochism. Only if there was moral masochism, or masochistic perversion. But I think that's not so. Again, the analytic data, at least in the patients that I've seen or supervised, or whatever, speaks in favor of the conclusions that I wrote about.

Q: Could you summarize those conclusions about the superego?

A: Yes, I think for one thing it's a question of defining what are you going to call superego. Starting with Annie Reich people tried to differentiate ambitions, or ego ideals, and so on. Well, I don't see how you can do that because I think you have to say that whatever you're going to lump under the heading of superego has to have something to do with morality. Otherwise, when you begin to talk about ego ideals and ambitions and so on, you get into instinctual gratification right away. I mean a little girl, her ideal is to be her mother or her father, or her older brother or sister, or something like that. Well that hasn't exclusively to do with superego. It has something to do with it, but directly it doesn't. It's an ambitious fantasy. And when you read the literature that's written by people who use the word ego ideal, they're always on the edge of talking about ambitious, libidinal, aggressive fantasies and mixing them up with something that has to do with morality.

I think Hartmann was right that you conveniently include under the heading of superego what has to do with morality. There's no other simple definition or classification that's a very satisfactory one. Granted, that it's not a very precise one, because these things shade into one another always. But then that's true of the agencies of the mind -- they do shade into one another. Ego, after all, is allied with the id. It's the executant of drive gratifications, drive wishes for gratification. And it isn't sharply separable except in situations of conflict and the same thing is true of superego functioning. So what is morality is the question, and where does it come from? And I think where it comes from is pretty much as Freud said, from the intense emotional conflicts of the oedipal phase, by and large. Although obviously things start before and continue on after. You know it's not just - superego formation isn't just limited to that period. But by and large that's where it gets its intensity from, and its importance from. And morality is how to do what will please one's parents and not provoke them or antagonize them into punishing. That's what a little child's view of morality is. How to be a good little girl, or a good little boy. That's what it means. You do what your parents want you to do, or think what you think they want you to think and not otherwise. And when you look at it that way, then it's compromise formation like many other compromise formations that have their roots in that time of life, in the conflicts of the oedipal period.

The role of libidinal wishes in superego formation and functioning was without . . . Well, I had written a paper on that, read a paper on that some ten years before. But I didn't have it very clear in my mind about the compromise formation. The relation to masochism I was pretty clear about. The relation to compromise formation and the idea that after all the superego, these various aspects of one's personal moral code, are in fact compromise formations was not clear to me. And I believe - I think it's so as I said - that that's the reason why the superego plays such a puzzlingly ambiguous role in mental life. And that it can function on the side of defense at one time and be defended against and function almost as a drive derivative at another time. I think that's why it's

so. But again, that's something to be worked out more in detail.

Q: You mentioned Loewenstein's idea. Your ideas about masochism and his are different.

A: Oh yes. And especially in relation to the superego.

Q: Right. And the definition you gave in that 1959 paper for masochism is the definition of masochism that you held on to.

A: I don't see any other sensible one myself. After we talked about it a long time in the Kris Study Group, I wasn't persuaded that there was any other better one. I don't know, I tried to be as open-minded about it as I could.

Q: In terms of the earlier comments you were making about the depressive affect, and so on. I remember when I read the affect paper, you make a statement there in passing that every affect has a thought. And every thought must have an affect. That there isn't that you can have one without the other. The one may be unconscious at that time. And you are then, in describing anxiety and depressive affect, actually given examples of the thought content involved. Especially with the depressive affect which I think is very useful. Because if you listen to the material then, with those in mind, then the mood, or the depression of the patient seems to make more sense.

A: I think so. Yes. I would go so far as to say what we call affect is pleasure or unpleasure, plus an idea. Not that every affect has an idea, but that the definition of affect includes it.

Q: Yes, you can't really-And you really talk in general now more in terms of unpleasure and pleasure. And one of the unpleasures is the depressive affect and the other is anxiety.

A: Right. That's the idea. I would say also, I don't know whether you would agree, Ed, but it seems to me, at any rate, that the ideas of compromise formation that I developed in the book *The Mind in Conflict* go a long way to helping clear up a lot of problems about transference and countertransference. I think that that is a clinical application, there, that's important, too. It always interests me. You know, I've gone around to a number of places now, psychoanalytic societies and institutes, groups, and talked with them about the ideas in *The Mind in Conflict*. They've invited me to come. They say they'll read the book, and we'll sit down and we'll talk about it. Spend a morning or an afternoon talking about it. Well, I'm delighted to go, because I want to learn what they think about it and hear what they have to say, and tell them as much as I can about it. And I've had some very interesting and instructive sessions. Nobody has yet talked about transference and countertransference. They'll talk about affect theory, they'll talk about superego functioning. They'll talk about compromise formation in general. All of those things, you know. But never about transference and countertransference.

Q: But in the book, the other thing that I think is, to me it seemed very original and new, are your ideas about drives. And the way you see drives as distinct from the way Freud described them. But you, of course . . .

A: But that didn't begin with the book, of course.

Q: The transference-countertransference, you already had written and made certain specific comments about transference some years ago. In terms of technical, the paper on Technical Precepts in Psychoanalysis

A: Oh, whether to interpret transference just as resistance, you mean. Yes. Well I don't think that was so specially novel or original. Maybe it was. I don't think so.

Q: Well it seems to me there are still people who go along . . .

A: . . . with the idea.

Q: Who borrow the other . . .

A: Yes, but it was kind of old-fashioned even then, I think. I think, I don't know. Yes, that's right, that paper on technical precepts did include that.

Q: What would you like to say about the transference-counter' : transference ideas in *The Mind in Conflict*?

A: Well, that there's always transference. That it's there in every aspect of the relationship between patient and analyst. And that the technical question always is, how important it is at the moment. And is that what should be interpreted, or is best interpreted, or not? But from a theoretical point of view, transference and countertransference are just subheadings under object relations. They are compromise formations like every other object relationship. And they're an inevitable part of what goes between analyst and patient. Always.

Q: But once you connect it to object relations, you mean, they're an inevitable part of any relationship.

A: Yes, exactly. And the thing that's unique about analysis isn't that there's transference, and for that matter countertransference, it's that the transference is analyzed, instead of reacted to in some other way. I mean that was a stroke of genius on Freud's part, to understand that that was so. It wasn't because he was irresistible to women that they all fell in love with him, one after the other. But that he really understood the dynamics and the function as resistance, and so on. That was really an extraordinary thing.

Q: So you wouldn't think that transference in the analytic situation is something that develops gradually and becomes more and more complicated, but that it is there from the outset, it just becomes more and more accessible to the patient and obvious to the analyst, %to then interprets it.

A: Yes, don't misunderstand me, though. To this extent, obviously, in every object relationship the nature of the relationship between the two people is important. So it makes a difference that you see a patient five times a week, that a patient comes to you for help of one sort or another and that it's a relationship that goes on for a long period

of time. Those are all significant and important. But what's there in the patient's fantasies, to be analyzed and understood, is essentially there from the beginning. And sometimes it's very dramatic, of course. The patient can be involved with you emotionally in his mental life very intensely before he ever sees you, because of the nature of his psychodynamics and his suffering, and his reason for coming to see you. His expectations.

Q: What would you like to say about your ideas in regard to the drives?

A: Oh, well, I think that drives aren't a phenomenon on the border or the boundary between psychic and somatic. I don't think they're different from other aspects of mental functioning in that way. I agree that they have to do with the functioning of the body, particularly of the brain, and they certainly have to do with the various parts of the body, sensations from the various parts of the body that Freud described. The genitals, obviously, and the mouth and the anus primarily. But other things too. You know, vision, touch, everything like that. But he didn't go far enough I think. In other words I think that yes, that's true of the drives, but it's true of psychic functioning in general, too. And that the concept, drive, is essentially a concept that's supported by psychoanalytic evidence as well as the other evidence that Freud deduced in the three essays. And I don't think that you have to think about drives as being more related to other aspects of body chemistry and physiology than other psychic phenomena. And that's particularly true of the aggressive drive, of course, where Freud's ideas are clearly wrong and yet the importance of what he identified as the death drive -- or we would say aggression -- seems to me very clear from the analytic evidence.

Q: When you said Freud's ideas about aggression are really clearly wrong, you were referring to the death drive concept?

A: The death drive, yes.

Q: Which isn't something that you agree with at all.

A: No, of course not.

Q: And what about the repetition compulsion?

A: Well, Freud introduced the idea of the repetition compulsion for this reason. His idea was that the death drive was unaccompanied by, or unrelated to, pleasure. That's why it's "beyond the pleasure principle." That's what he meant by the title. According to Freud, libidinal discharge gives pleasure, aggressive discharge doesn't. So then it's easy to understand the constant repetition of behavior striving for libidinal pleasure. That's the way the mind works, that's the pleasure principle. But how to explain a striving after something that gives no pleasure? What could that be, you see. And so he said well, maybe there's a need to repeat, and he went into this idea of a repetition compulsion. But that was to explain what today we would call the phenomena of aggression in mental life, with the understanding that these phenomena are unaccompanied by any pleasure premium. Once you assume, as nearly everybody has since the forties, when Hartmann and Kris and Lowenstein wrote their paper, "Notes on the Theory of Aggression" - once you assume that there is a pleasure premium to

aggressive discharge and that there are conflicts about aggressive wishes or the aggressive aspect of wishes, as well as about the libidinal ones -- once you put aggression on a par in those respects with libido, you have no more need for a special repetition compulsion with relation to aggression than you do for one with relation to libido. There was no need for a repetition compulsion with relation to libidinal wishes. Ideas of repetition, I mean the phenomena of searching or looking for the childhood gratification, the importance of the early gratifications in determining the aim and object of the partial drives -- all that was understood, but it didn't logically require a special repetition compulsion of the sort that Freud introduced in *Beyond the Pleasure Principle*. It was simply that it's in the nature of drives to drive the mind to repeat the experiences of gratification that were present, current, experienced by the individual early in life. That's the way things are, clearly enough. You know, childhood instinctual wishes are immortal. Or let's say they last till you die, anyway. That's a certain kind of psychic immortality, if you will! But that's not what he meant by repetition compulsion. So I never could understand why Schur wanted to write a whole book about repetition compulsion. It wasn't necessary.

Q: So the earlier ideas about repetition and symptom formation . . .

A: Sure. That goes back to 1900, you know. To the seventh chapter of *The Interpretation of Dreams*. Hallucinatory wishful fulfillment. But that wasn't repetition compulsion. That wasn't what he meant by that.

Q: And the repetitive quality of the symptoms in an obsessive thought or a compulsive act?

A: Powered by the wish with all the conflict and defense and so on that accompanies it. Sure. That's not repetition compulsion.

Q: So essentially the continuous pressure of the wish and the conflict that this gives rise to explains the . . .

A: . . . Phenomena of repetition in mental life. Yes, right. Exactly.

Q: So having touched on that, I was going to ask you whether you maintained your views about the fact that the structural theory really replaces completely the topographic theory?

A: Well, yes, it grew out of it. I mean certain parts of the original theory, the so-called topographic theory were discarded and replaced by other ideas and so the consequence is a new theory which is the same as the old in certain respects, different from the old in other respects. And we give it a different name. That's about what it amounts to. I mean there wasn't a topographic theory that was thrown out the window and a structural theory that came in. Certain parts of the first theory, the original theory, which has the name topographic theory seemed to Freud, I think correctly, to be demonstrably untrue and so he substituted for them other ideas and then a different name was given to it.

Q: But there are people who still think of . . .

A: Who want to keep the demonstrably untrue ideas Yes, that's right!

Q: Which area do you think that hit the hardest? Is it in the area of dreams that this has been the

A: Well, that was Lewin's idea, of course, that you analyze dreams with the topographic theory. And that the dreams could be best analyzed with the help of, or understood in terms of the topographic theory and symptoms best in terms of the structural theory. But that was a silly idea, if you want to know the truth. Smart as Bert was, and clever.

Q: What about the triad? The oral triad.

A: Oh, the oral triad. That has an interesting history, you know. It created an enormous stir when he presented it. So, the triad of oral wishes: to eat, to be eaten, and to sleep. And this really was as influential, was as much the vogue for several years, as, let's say, self psychology is today. You go back over the literature, you'll see, it was taken up with great enthusiasm.

Well, Bert himself began to have sort of second thoughts about it, because the people who liked it the best, of course, were the Kleinians. [Laughter.] And he wasn't charmed by that! But in any case, it was several years later, he was giving a paper -- I think he had been given an award of some sort -- and gave a paper, I think it was to the -- I know where the room was, in the Academy of Medicine, that he gave this paper. And it may have been a meeting of the Psychoanalytic Association of New York, or it may have been a special meeting that was called by the foundation that gave him this prize. I'm not sure. I don't remember that exactly. But he said something about this oral triad that made very good sense, and clinically was applicable, and it was that when a little child sees a sibling, a baby, nursing -- let's say a three-year-old or a four-year-old -- sees the baby nursing, that he imagines that the baby wants to eat, to be eaten, to merge with the mother, and of course he sees the baby go to sleep. So that he imagines in his jealousy of the baby, and his wish to be the baby, these wishes to eat, to melt into the mother, to be held in the mother's arms and become part of the mother and to sleep, are what he would want. Now that is a very different matter from saying that's what's going on in the baby's mind at three months or two months, or zero months, for that matter. And I remember being very struck by that because that makes very good sense and it is a brilliant hypothesis it seems to me, a brilliant speculation about what a child's envy of a new-born sibling could be like, could have as its ideational content. I think that that's so. Although I doubt if it's limited to just those three things. But nevertheless it would include them. And could explain why some patients have such wishes in, you know, such an imperious, imperative way. That it's an expression of their longing to be the baby whom they envy so. That makes very good sense to me.

But, Bert being Bert, he didn't stick with that. Everybody else wanted to think of it as something that's really in the baby's mind before the age of a year and he sort of wish-washed about it, back and forth. He never said yes and he never said no. But I think that's what it really is, what the oral triad is.

Q: So are you saying that you do feel it has an explanatory role in adult analysis?

A: Yes. That it could appear as a set of related fantasies and if it does, when it does, that I would think then this must be related to sibling rivalry of just the sort that I was specifying.

Q: You have also written about dreams, and certainly there doesn't seem to be much new that has been written about dreams in the last twenty years, I guess.

As opposed to earlier on, where dreams, studying dreams, and various things about dreams and the psychology and so on, seemed to take a much more important place. What can you say about that? The decrease in the general interest; your views on dreams

A: Well, as you say, dreams were written about a very, very great deal in the early years of analysis. You couldn't pick up an issue of the *Internationale Zeitschrift* or the early issues of the *International Journal* without there being a section on dreams. The idea that a dream is somehow different from other mental functioning and that a dream is a hallucinated wish fulfillment -- I mean adherence to those ideas, I think, has inhibited, made difficult, very much interest in dreams because it doesn't, at the present time, fit with the way we think of mental functioning. It doesn't fit with the structural theory at all. The only way to understand dreams is as compromise formation. And nobody wants to do that because Freud said dreams are hallucinatory wish fulfillment. Well, yes, they are. So's everything else a wish fulfillment. Dreams as a hallucination. Well, many dreams are, not all, but many dream are. But it's a compromise formation. And until people realize that, they're going to wander in just an endless morass of, "Is a dream a communication? Does this dream have some superego or punishment importance?" Well it always has everything; it's a compromise formation like any other fantasy and it's one that's usually characterized by some visual elements and a belief in the reality of the experience.

Q: But it is something that takes place in a different state of mind, mental state, than other things, so

A: Yes. Takes place in sleep, you mean.

Q: Yes.

A: Right.

Q: And, as such, do you think it gives, at least it is an area that its study could lead to a better understanding of the functioning of the mind and the brain in that altered state? And even give some ideas about, let's say, thinking.

A: Well, you know, anything could be. If you ask a question that way I can't say no. Because if somebody has an idea of how to use dreams and the psychoanalytic method to explore those topics, then I'd be the first to say, well that's great. But I don't know how it could be done. And I do think that analysts have been very hampered in whatever interest they have in the psychology of dreams, -- they've been very hampered by not recognizing clearly that a dream is a compromise formation and that many of the questions that they ask about dreams and dreaming are nonsense questions because

they don't start with the right idea, the right understanding about what a dream is. A dream is a compromise formation; it has the aspect of wish fulfillment in it; it has the aspect of conflict in it, that is anxiety and/or depressive affect; it has defense in it; it has superego functioning in it; all of those things. The idea, "this is only a dream," for instance. There's nothing special about that, that's a form of defense, when the anxiety, when the unpleasure gets too strong, whichever it is. And that's one way of defending – by thinking, "This is only a dream." Just as people who are awake, patients, can defend themselves against the unpleasure associated with certain thoughts or wishes by saying, oh well, it's only a thought, it's only an idea:

Q: So the difference, or distinction, between the manifest dream and the latent dream loses its importance in the way that you say. It becomes no different than any of the latent thoughts, if you will, in a slip of the tongue, or even in associations.

A: Absolutely. Or in what we're doing now. [end of tape, change of tape]

Q: So I was going to ask you, as I was saying, this eliminates the distinction between the -- decreases the importance of the distinction between the latent and the manifest, especially as it relates to the fact that the manifest content of the dream comes out during waking and when the person is awake. And that the latent content is what is taking place during sleep.

A: The manifest content?

Q: In other words, the manifest content of a dream is essentially, or has a great deal of, secondary process thinking.

A: Oh you mean the narrative account of the dream?

Q: Right.

A: Yes, well that isn't what Freud meant by manifest content.

Q: No?

A: No, what he meant by manifest content was the experience of the dream. Which of course is accessible to you only as a report. But if it's your own dream then you have a memory of what you experienced during your sleep.

Q: Hmm.

A: That's what he meant by manifest content.

Q: I don't think I've ever thought of it that way. That the narrative, what the patient reports, is considered the manifest content.

A: Well, that's the nearest you can get to the manifest content. That's why I said it's important, it's useful to remember that what the patient tells you is, in fact, his or her first associations to the dream.

Q: You mean the actual report of the dream, is the first association to the latent

meaning of the dream?

A: That's right.

Q: That's a different way to look at it though, no?

A: Yes. It is, that's right. But there are analysts who say, "Well, you should try to imagine as you listen to a patient telling you a dream, you should try to imagine what the patient experienced in the dream. So try to conjure up in your mind, the images, visual and other, that the patient is telling you about." I look at it a different way entirely. I say, what a patient is telling you is his or her associations to the conscious experience during sleep that he or she remembers during the hour. And that's valuable for that reason. "I had the funniest dream," is already an association to it.

Q: Yes, but there is something I'm trying to ask you about this and I'm not putting it well. I guess it has to do with the distinction between primary and secondary process thinking. That the activity that takes place during dreaming would be considered primary process thinking. And that it is put in the language of secondary process upon awakening. Is that

A: Secondary elaboration, or secondary revision.

Q: Well, I wasn't thinking only of the secondary revision. No, I was thinking in terms of just

A: It's verbalized.

Q: A more logical, cohesive narrative than what is actually dreamt.

A: Well, then that's already revision, if it's made logical or cohesive. What Freud thought of as editing. Well that, you know, leads into a lot of areas. What do you mean by secondary process thinking as opposed to primary process thinking? More or less infantile is the way I think of it. Not that there's two different kinds of thinking, the primary process thinking and the secondary process thinking. I don't believe ...

Q: You don't think that thinking . . .

A: That was his idea that led him to the first theory of mental functioning that he had. Namely that the infantile way of thinking, the primary process way of thinking, which has characteristics that are determined by a need for discharge or gratification, -- immediate, prompt (better word) -- that that is all that's present for a certain number of years in early childhood. He thought at first maybe as late as seven or eight years. But at any rate for a certain number of years in early childhood and that it is gradually overlaid and eventually disappears from conscious mental life and is replaced by this new, verbal, logical secondary way of thinking. The first is historically primary, the second is historically secondary. One overlays, obscures and obfuscates the other. Well, "that ain't the way it is," and he soon realized it, but still especially because, as you say, dreaming seemed to fit in with that idea, that was the whole, sort of fundamental basis, of his way of thinking about mental function that we call the topographic theory.

That as soon as language comes in, and things are verbalized, that there's a different way of thinking than beforehand. And while I think he was absolutely right that, as he put it, the acquisition of language leads to an enormous facilitation of thought, I don't think that it leads to something as qualitatively different as he thought, at all.

Q: So you don't think that an adult has in fact two types of thinking, secondary process and primary process.

A: No. More or less infantile, yes. But not two different types of thinking, no. You can emphasize the differences if you want, but even ordinary, everyday thinking in adults and verbalization is characterized by the use of opposites. "Oh yeah", meaning "Oh, no." "Boy he's a big fellow," meaning he's the opposite. All of these things are perfectly characteristic of adult, verbal thinking, as Freud saw when he started to discuss jokes. The same with condensation and displacement. He said, "Well, it's a return, of the repressed from repression." But it's a funny way to say it. It's just a part of normal thinking and expression. And, of course, when you get to these very high level, highly valued, verbal productions like poetry, then you're in a real pickle, you know, if you try to say, "Oh, well, a poem is primary process thinking, it's not adult thinking, it's just infantile, it's regression in the service of the ego," and so on. It gets to be a very untenable set of hypotheses, I think. Sure, the things that he described – you know, the phenomena that he described, are certainly so. But I don't think that they're explicable or well explained, at any rate, in terms of two different kinds of thinking. I really don't think so, Ed. All right?

Q: Okay. Let's take a break.

.....

Q: From what you were saying, you don't really think there are two modes or types of thinking.

A: No, I don't.

Q: There's just one type and it contains elements that we usually attribute to primary process thinking.

A: More infantile.

Q: More infantile, and others that are more logical.

A: Mmmm. More grown up, yes. And impress us as more logical. They aren't always, but . . .

Q: So in a dream, in dreaming, therefore you don't think that thinking is in any way different.

A: No I don't.

Q: But it is true that some of the mechanisms that take place in a dream, use of symbols, or what you said before, opposites represented, those things that we usually

associate with primary process seem to be more frequent in a dream.

A: Are they? I'm not convinced that that's so. No, I really am not, Ed. We pay more attention to them. But I don't think that it's really so that they're more common.

Q: I'm trying to think of an example of something in a dream that is . . . Certainly there are . . . Well, for example, Freud says when in a dream somebody goes from one room into another, it indicates a change in time, is one of the things he says. How would you see that?

A: Well, I'm not sure that it always does. You know early on, when all of these findings were very new, Freud and the others had the idea that there was a primal language -- an "Ursprache." And that if they could learn the primal language they would have done something like archaeological investigation. You know, I don't think that it's been demonstrated that when somebody goes from one room to the next in a dream, that it necessarily represents a change in time. I don't know, maybe it does. But I don't think today we think of all this -- what would you say -- that symbols have such a definite and universal meaning as they hoped to find that they did. I may be wrong. I haven't subjected it to, you know, investigated it in any systematic way. But I think that there are a lot of such statements that would have to be tested before we tried to draw sane conclusions from them.

Q: You know, something like, in a dream, frequently, the analyst may be represented as someone sitting behind.

A: Sure.

Q: Now, if just the element of something behind totally represents the analyst in a dream, that is not something that would happen in normal, let's say, adult language. That is you wouldn't represent the person just by the position they sit at.

A: Well, maybe you wouldn't, but in the French Assembly, they talked about the right and left [Laughter] and that came to have very definite meaning. They talk about the White House, when they mean the Executive Branch of the Government. Or the Quai D'Orsay, when they mean the French Foreign Office. If that's the right term for it. Or the Porte, the Sultan was called P-o-r-t-e. I don't know exactly what it meant but it had something to do with the harbor at Constantinople, I always thought, maybe incorrectly. But anyway, all sorts of references like that.

Q: Okay . . .

A: I see you're not convinced by that, but . . .

Q: Well, you know, I don't think there's any question one could say that certain, that primary process is not limited to sleeping, that it's present all the time. And so you can see certain connections that are essentially primary process, or certain expressions that essentially come from primary process. Like in jokes, or let's say, somebody puts a sign on the car, number four [in sad??]. But you were saying that you wouldn't feel that this kind of a representation necessarily implies a different mix of thinking.

A: That's right. Why are people crazy about baseball? Don't you think it has something to do with unconscious symbolism? I do. I really do. It's not in any way conscious to the people who are involved. And the same way with so many activities in life. The more you think about it the more you realize that the type of thinking that Freud described so convincingly and I think correctly in connection with dreaming is not by any means exclusively related to that particular activity. Now obviously there are certain things that are different when you're asleep from what they are when you're awake. But I think that they would have to be investigated starting with the understanding that we have, or should have, about the psychology of dreams at the present time, rather than starting with the idea that everything Freud wrote in 1900 is correct. It wasn't. It was a marvelous discovery, but it doesn't mean that nothing has changed in our understanding of it. And what analysts particularly do, and this is more true with dreams than almost anything else is, is that whenever they're interested in dreams and dreaming *The Interpretation of Dreams* is like the holy scripture. Every word that Freud wrote has got to be correct. Well, you can't do scientific work of any sort in that way. It's like what the French -- Lacan and all the rest of them do -- the so-called textual analysis. It's obvious that what they do is that they take everything that Freud wrote and they debate like the Scholastics or the Talmudists -- and I'm sure there are Muhammadans who do the same thing -- about every single word and, almost, letter. As though it was a divine revelation and if you understand the words that are in the book, that are in the papers that Freud wrote, then you'll know more than if you don't. I mean it's crazy.

Q: Well there is recently more of an interest in and questioning of the accuracy of the Standard Edition and the feeling that you should really go back and understand Freud in a very, [I don't know what this is supposed to mean.] the Any that he wrote in German. And though the Lacan thing is somewhat different, but it seems to be . . .

A: Well I think it's largely misguided. I read Freud in German before I read it in English, because it wasn't available in the Strachey translation until somewhere in the 50's, beginning in the 50's. And I would say that the best edition of Freud's works is the English edition, the Standard Edition. It's the most accurate, it's the most scholarly, it's the best in every way. It certainly is better than the German edition of the *Gesammelte Werke* has been up until now. Because it's more scholarly, it tells you more about the subject that you're interested in. At least about it in connection with what Freud wrote. And the criticism that people like Bettelheim level against the Strachey translation that Strachey, English-speaking analysts, use words like ego, words derived from Latin and Greek, whereas Freud used words derived from ordinary, everyday German, is based on a complete misunderstanding of scientific German. I don't know how much scientific German you know.

Q: None.

A: All right, it's true in general for the German language and particularly so for scientific German, that the convention is different from what it is in English. In English we use, for scientific discourse, Latin and Greek derivatives. In German they don't. So in German, instead of speaking of oxygen, which is "acid former", they speak of Sauerstoff, which is the same thing, "acid former," only from German roots instead of Greek ones. Instead of speaking of hydrogen, which is water maker, they speak of Wasserstoff. Now you

can't tell me that a German chemist thinks any differently about Wasserstoff than we do about hydrogen. It doesn't have any different connotation for him. And that's what Freud did when he wanted to invent words, by and large. Not always, sometimes he used words of Latin and Greek origin. But by and large he followed the usual tradition in German. That's the way the German language is constructed. It's not only true for scientific German, it's true in general. If you see a compound word in German and you want to translate it into English, you take each part of it, translate it into Latin and it comes out the English word. I mean the theory of dreams or let's say the theory of mathematical functions –  $y = f(x)$ . So the theory of functions in German is Funktionlehre. Now, "lehre" is derived from the ordinary word for teach, "lehren". But it means theory. Now when a mathematician talks about Funktionlehre, it doesn't make it any nearer to his heart than if we speak of the theory of functions. That's the way it is. While German is Bettelheim's mother tongue, and of course he knows it far better than I ever will, he doesn't know, obviously, scientific German. What he says is just nonsense, it doesn't make any sense at all.

Q: That's the Bettelheim. But you, yourself, I think, for example, translate "where id was ego shall be" differently.

A: Yes.

Q: So you don't have a hundred percent agreement.

A: No. I don't think any translation is perfect. Neither is the original, you know, the German has mistakes in it too from time to time. And Freud himself made slips occasionally too. But as a translation I think Strachey's translation of Freud is really one of the very best. And it's so scholarly and tells you so much about the history of Freud's thought and so on, that I think it would be a shame to change it, really. It can be corrected here and there. There are errors. I picked one up just the other day. I was reading . . . this Etchegoyen's paper which is in the *International Journal* in preparation for the Hamburg meeting. And he cited the fact that Strachey translated "perhaps even before" into "somewhat later." So he made a mistake. Freud obviously meant "around about the same time." But what he said was "perhaps even earlier." Well, so that's a mistake in the translation that obviously should be corrected. But as a whole, you know, as a translation, I think it's just ridiculous to niggle at it. Make the change, whatever you want. "Wo Es war, soll Ich werden." It means, Freud meant that instead of being under water, it should be up above, it should become ego. Not ego should be, should move in. [Freud was referring to the Dutch system of polders.] But that that part of the mind should become integrated with the rest of the ego. That was what he thought should become ego. So that's a little difference, but . . .

Q: To me it sounds . . .

A: In that case I would say it could be improved on. But almost nobody ever says that's wrong!

Q: Okay. Changing a little bit the subject, I was going to ask you that when one reads through your work now, looking back and moving forward, there is a consistency there

that speaks of a high degree of independence in your thinking. I am wondering if you could nevertheless pinpoint certain early influences. Teachers, colleagues that you think played a role in the development of your thought.

A: It would be awfully hard for me to pinpoint. I always said that the most important part of every analyst's training is obviously the personal analysis. The most important part aside from personal analysis -- the most important part of my psychoanalytic education was that study group, the four of us. You know, we hammered away at each other after we got to know each other pretty well, and we got along very well. We used to meet weekly for several years and we really learned a lot from each other and made each other think very hard about what we thought we knew. So I would say that's a very important part of my education. The attitude of testing and not just accepting on faith and trying to understand. And trying to relate generalizations to the data on which they're based and to understand what it is that made Freud, for example, or anybody else, believe what he did, or conclude what he did. I couldn't tell you where that comes from but that is part of me. That's not just in psychoanalysis of course, obviously. And I had some very fine teachers in Boston, Eddy Bibring was one of the best. He was very, very good. But then there was Helene Deutsch, who gave a continuous case seminar. She was awfully good, I learned an awful lot from listening to her. Then there was Hanns Sachs who was very interesting. And certainly he started in analysis in 1906, or 1908, something like that. And Bibring's wife, Grete Bibring, and Jenny Waelder and a lot of the others. There were some very good ones who came over and settled in Boston. And then when I came to New York -- because by that time I'd finished all the courses and so on -- I certainly was influenced by and learned a lot from those people who were senior colleagues rather than formally teachers. Kris and Hartmann, primarily I would say -- those two the most. But I used to feel that I never went to a meeting or heard a paper but what I got something out of it. Whatever it was, it was at least something. A new idea, or a question about old ideas. Or sometimes an opinion about the person giving the paper that was different from what I had, whether for better or for worse. You know, different from what you could get from reading what he or she had written. And for a long time I went to a lot of meetings, and you know I was very conscientious about that.

Q: Bibring wrote a paper about depression.

A: Yes.

Q: Where he I think, if I'm not mistaken, he feels that the state of helplessness, of the ego

A: Yes, right.

Q: What do you think of that? I mean he didn't say anything about the depressive affect, but he certainly separated, or he distinguished that from the classical idea, you know, orality and depression.

A: Right. In that respect, certainly he did. You know Bibring developed Parkinson's it was very severe. And I think that's where that came from.

Q: Yes, the helplessness.

A: I would think, you know, being helpless is depressive affect. It's unpleasure plus ideas that something terrible is happening, has happened. So I would agree there. But to say that that is the whole story seems to me ...

Q: No, I was only thinking in terms of influences, that ...

A: Well, that came later, you know, that was in the 50's sometime.

Q: Because you mentioned Fenichel and Rochlin. . .

A: Well, see, the book that we went over in our Study Group, and we spent pretty near a year, once a week, discussing was Fenichel's *Psychoanalytic Theory of Neuroses*, which had just been published at that time. It was published in 1945 or 1946, whichever it was. 1945 I think. And so we went over that and that was pretty encyclopedic for those days. I think he had something like 1200 references in it. And by and large it is very fine piece of work and his ideas in that book and in the various papers that he wrote that were accessible were very influential, certainly, in the way that we thought and tried to understand the clinical material that we were confronted with.

I also read a book of Hartmann's – well, two books, that were only available in German at that time. The one was *The Ego and the Problem of Adaptation* which has since been translated and which you're familiar with. But in the early 30's, I think, he wrote a sort of outline or exposition of the basic ideas of psychoanalysis, that was intended for professional psychologists. And that was quite an interesting thing to read. Nobody's ever translated that. It's been outdated since, but, you know, when I read it, it wasn't outdated so much. And that was very interesting. But I read everything I could get my hands on and that was quite a lot. I started reading psychoanalysis when I was rather young, because I had made up my mind that's what I wanted to be. And so when I started, you know, was in the beginning of the academic year 1932-33. And I started with Freud's works and read them sequentially as best I could. Although there was a lot in them that I just couldn't understand at all because I had never seen patients, I didn't know anything about it. But that was the way...

Q: How did you come to know about Freud and his works? From school?

A: Well, in a way. But I went into analysis for the same unconscious reason that I went into medicine in the first place and that was what you'd call a rescue fantasy. Obviously that's a simplification. But my mother was ill when I was about three years old and she went off to the hospital. So she was away for quite a while. And from that time on I decided I was going to be a doctor. I had no idea why I decided I was going to be a doctor, but obviously if I'd been a doctor I would have been with her in the hospital. That was clearly part of it, though by no means the only part. But by and large that 's what it was. And I was always going to be a doctor. There was no question in my mind. Well, before I went to medical school, that is, during the years I was in college, I didn't know anything about psychology and cared less. Chemistry was what I was interested in really. And as far as medicine was concerned, the only thing that I was much interested in besides chemistry was bacteriology -microbiology. From reading, you know, the life of

Pasteur, mostly, really. And then just before I entered medical school - my father died about six months before I went to medical school, and my mother had a very severe psychological reaction, an hysterical psychoneurotic reaction, and I decided I was going to be a psychoanalyst. I didn't know what a psychoanalyst was, or anything like that. She seemed to think that there was somebody by the name of Freud who was pretty good about this sort of thing, you know, not that she was ever analyzed. She wasn't, unfortunately. But she tried to read some of the things that he had written. Well, that's how I decided I'd be a psychoanalyst. When I graduated, in early June, I was interested in physical chemistry. In September I was going to be a psychoanalyst.

Q: And that's when you picked up the...

A: Well, then I got some advice from a teacher with whom I worked rather closely in medical school. He said that if I wanted to be a psychoanalyst I should at least read some psychiatry first. So toward the latter part of my first year in medical school I read through Bleuler's Textbook of Psychiatry, in English. And it was like taking a train ride with your eyes shut, by and large, again because I'd never seen a patient and I didn't know what the hell he was talking about most of the time. But I read it word for word. And then in the fall of 1932 I started reading Freud. And I tell you, I had the library of the medical school all to myself, or the section of it that had Freud's work. There wasn't any competition whatsoever! I was the only one who was reading psychoanalysis, so I had a wonderful library. I could get whatever I wanted from the Harvard Medical School Library.

Q: And how did you set about at that time to read it? From the beginning, chronologically?

A: Yes, *Studies in Hysteria*, and then, from then on, the other things that he wrote. The shorter papers and the book on jokes and the book on psychopathology in everyday life and the dream book, obviously. And some of it I could understand, and some of it I couldn't. But I read all of it two or three times before I finally got through, you know. After a few years I began to have some knowledge of what was going on. Saw patients and heard more about it. But it was independent mostly for several years.

Q: That was before you were in analytic training, right?

A: Oh, I wasn't in analytic training until 1939. But you see how it was, Ed. The Psychoanalytic Institute in Boston was very small. Everybody who was there including some who had already graduated, took every course that was offered whenever it was offered, because you never knew when it would be offered again. There was no regular curriculum. There was no bulletin, there was no syllabus. And some of the courses were worth taking and some of them weren't. So the course that we had in reading Freud -- at that time there were in English five volumes, or I think still only four volumes, of collected papers. And there was a course in reading Freud, a literature course. That was so in every analytic institute at that time. And the fellow who gave it was a very nice man, but he really didn't know very much about it. So the way it turned out eventually, since I had read the papers myself before, and so on -- the way it turned was mostly that I gave the course. I mean he would ask me what did I think? And it was kind of

embarrassing at first, but then I thought what the hell. If that's the way it is, that's the way it is. There were other courses like, you know, Bibring gave a course, a scientific course where we just sat and listened to what they had to say, or discussed with them. It varied. Bibring was very down-to-earth and a very easy person. There was nothing pompous or self important about him. With Sachs it was very old fashioned. He was like the old university professor. You know, it wasn't that he was austere or unfriendly, but if he gave a course he lectured. That was it, you listened. You occasionally might ask a question at the end but . . . But Bibring was much more like a seminar, you know, you could talk back. So that's the way it was.

Q: You mentioned reading psychiatry. You know there was a time when psychoanalysis had a great influence on psychiatric training, it seems to be somewhat lessened now. How do you think your ideas, for example about depressive affect, what role would they play in psychiatric thinking? And their studies of depression?

A: Well, that's an interesting question, Ed. I think that Kraepelin's ideas about depression or depressive illness, are so generally accepted and are the basis of so many grant proposals and studies that have been financed by various foundations and by the government that I don't expect my ideas are going to have very much influence. There's too much inertia, you know. But sooner or later, they're going to have to wake up to the fact that Kraepelin's ideas were all wrong and that just because some patients have mental illnesses in which depressive affect is a very prominent symptom, doesn't mean that it makes any more sense to talk about depressive illness than it does to talk about anxiety illness. Now of course there are psychiatrists who want to talk about anxiety illness. And they base it on drug studies and so on. But you and I know that anxiety is a part of every psychic conflict. And I think depressive affect is a part of every psychic conflict, whether the outcome is normal or pathological. And it doesn't make any sense to try to make the presence of depressive affect in a syndrome of mental illness the touchstone or the diagnostic criterion, for that reason. It doesn't make any sense. And they're going to recognize it sooner or later, but I doubt very much it will be sooner. I think it will be much later. Because they're too committed to it.

Q: So essentially they use psychopathology in a totally different way, now.

A: Well, it would make perfectly good sense to say I have a group of patients who respond to this drug and I want to study them and see what they have in common and what their biochemistry is and so on and so forth. That's all right. And if you make your diagnostic classification on that basis, it's at least a start in the right direction. But if you make your diagnostic classification on the basis of whether or not depressive affect is a prominent feature of their symptomatology, then you're starting behind the eight ball. You've got no chance of working out anything that's going to make real sense in the long run. Just the same as if you did that with anxiety. I don't think that you're going to get anywhere. And sooner or later they'll find it out. I think so.

Q: You sometimes say diagnostically there are really three groups: sick, sicker, sickest.

A: Yes, well that's as close as you can come. I mean it's better to say I don't know any better diagnostic classification than it is to pretend that you do. You know, if you go back

in the history of psychiatry, to the time before the Wasserman reaction was discovered one of the big problems was to make an early diagnosis of general paresis, and to differentiate it from the so-called endogenous psychoses as early as possible. Because the prognosis was so very different. And they had no bacteriological, immunological way of making the diagnosis, so they tried to do it psychologically. And I don't know how many Ph.D. or M.D. doctorate theses were written in Kraepelin's clinic trying to establish a psychological test, or series of tests, or group of tests that would make the distinction. They're all gathering dust somewhere, if they haven't burned up since, because along came the Wasserman reaction and they were all outdated immediately. But they really tried to do that and that's the same sort of difficulty that you're in if you have no really better way of grouping your mentally ill patients than on the basis of symptomatology. We know that it isn't the symptomatology. And especially, it will change from year to year in the patient and it'll seem to be different if one person with an interest in one aspect of psychopathology examines the patient or takes a history on the patient from what it does if somebody else with a different interest does it. You can't separate these patients symptomatically in the way that even Freud thought maybe you could do. Certainly Kraepelin thought you could do it. And Bleuler thought you could do it. I don't think it can be done that way. What psychoanalysis has to say comes much closer to Anna Freud's diagnostic profile. You can say something about the types of conflicts and the severity of conflicts and the sorts of compromise formations that this or that patient has. But that's not a diagnostic classification. That's like what would be the sort of analogue of a complete profile of blood chemistry, of physiological studies, pathophysiological studies, something like that. But not diagnostic.

Q: Sane analysts say you have a patient in analysis and the patient becomes manic, or perhaps severely depressed. It's perfectly fine to start them, if not yourself, have someone else give them the medication that they need, while you continue the analysis.

A: I don't know, I don't have any experience with that.

Q: But your way of looking at psychopathology in a way would exclude that anyhow.

A: That's right. Either a patient is analyzable or not. And certainly I wouldn't say you shouldn't do psychotherapy as much as you can with any patient, and maybe the hospitalization or institution of drug therapy would be necessary, but I wouldn't think of it so much as analyzing. Because when such a patient is analyzable, no matter what their reaction, it should be if the patient is analyzable you're going to be able to deal with it without the medication. And I think you can. Even Abraham was able to analyze some patients with some good results who had had pretty severe depressions in the interim between. At least that's what he wrote, that's what he said he could do. But I'm sure that I've seen patients who without analytic work would have been hospitalized patients, and with it you could deal with it.

Q: Okay. The next question I was going to ask you has to do with, again, the relationship of psychoanalysis with other fields. In particular, the neurosciences, neurology, and so on, mainly the relationship of the mind to the brain. I know you've made some comments about that in the book *The Mind in Conflict*. Could you say some more about that and how you see it?

A: Well, most of what I would have to say, Ed, is repetition of what I wrote in the book, at least I think it is. Mainly that one aspect of the functioning of the central nervous system is what we call psychology, that is the mind, thoughts, mental phenomena in general. Now unless you're going to subscribe to the view which seems to me a wholly unscientific one, that there's a body and a soul and that the soul is something separate from the body, then I don't see how you can come to any other conclusion. If you extirpate the cerebral cortex of a man, everything that we call mental disappears. It's not there anymore. And all the findings of comparative neurology indicate the same thing. That is, the more cerebral cortex there is the more nearly there is what you might call, or should call, mental or psychic functioning. Certainly the case of the great apes that are closest in size of cerebral cortex to man, it's very clear that their psychology comes the closest to being what we're used to seeing in human, although obviously it's very much more limited. I should say it seems to be very much more limited because I've never studied the great apes. I've never made the acquaintance of one of them! So that's what I would say. I can't see it any differently from that. What we're studying as psychoanalysts, when we use the psychoanalytic method, is an aspect of cerebral functioning. And this particular aspect of cerebral functioning is, it seems to me without question, best studied by the psychoanalytic method. There isn't any other method that comes close to giving results that are either as extensive or as reliable. Introspection is about the only other way of studying important -□subjectively felt important -- aspects of the mind and introspection is completely unreliable because we all deceive ourselves and insist on deceiving ourselves about many of our most important motives. Well that's all in the book, I don't really have anything to add to it. I don't know what I could say beyond that.

Q: Do you think psychoanalysis can make a contribution to the question of the relationship between the mind and the brain. Or do you see future scientific development coming primarily from the neurophysiology, neurobiology, and so on, direction?

A: Well, look, Ed, it's sort of this way. Can physics, methods of physics up to the present time, make a contribution to the relationship between radiation and electronic orbit? Well, in a certain sense, yes. I mean equations as to the relationship between frequency and energy and so on. But, you know, it justifies you in saying when an electron moves from an orbit of higher energy to an orbit of lower energy, radiation is emitted. But it doesn't tell you what exactly that radiation is. Now we're in a similar situation as far as mind and brain are concerned. Whether psychoanalysis will ever, whether there will ever be any explanation in the sense of knowing why it is that, when cerebral cortex is sufficiently large and organized in a particular way -- that is, the synapses and the end connections and so on are of a certain sort, that what results is what we call mental functioning -□whether that will ever be explained why that's so, I don't know. Any answer to that question seems to me to be very far in the distant future. But of course analysis has a lot to say about that aspect of brain functioning that we call the mind. It certainly does. Just as physics has a lot to say about radiation, even though it can't answer the question why should there be radiation in the first place. I don't think any physicist has any idea why there should be, but there is, and you can say a lot about it. And the same thing with mental phenomena.

A: This is an aspect of cerebral functioning. Now how much the techniques of neurophysiology, electroencephalography, biophysics, all the chemical analyses and so on, how much they have to contribute, that is of interest as far as mental functioning is concerned, remains to be seen. I don't see how anyone can predict it. There are enthusiastic people who think they can see and predict, but they can't really.

Q: what if a neuroscientist, or a neuropsychologist, let's say, was to study thinking, or perception, or memory. Wouldn't you say that not taking psychoanalytic discoveries about that, in terms of distortions and so on, in term of defensive alteration, and so on -- not taking those into account would be an impediment in such research?

A: It'd be a fatal flaw, in my opinion.

Q: So that psychoanalysis could have a place, should have a place, in that kind of

A: Oh in that respect yes. Anybody who is doing any research, with whatever method, on that aspect of brain functioning that we call the mind, should know the findings of psychoanalysis. If he doesn't then he's ignoring the most of what is known about that. And that isn't a very sound way to embark on research.

Q: Well there is a lot of research that tries to there seems to me there is a lot of, at the moment, movement in neuropsychology and even in direct brain research, you know, right brain, left brain research, and so on. That they are beginning to focus their attention on the functioning of the mind, ignoring the psychoanalytic findings. I have at least seen a couple of books on the subject that come up with their own theories trying to explain certain findings. one, for example, tries to explain why you suddenly feel a certain mood and how a certain system in the brain gets clued on something and you feel a certain mood, and so on. So they're trying to explain what psychoanalysts have been involved in for all these years, without knowledge of psychoanalysis. And essentially ignoring psychoanalytic findings.

A: I don't doubt it for a moment. I mean you can go to (who are those two people up in Boston who recently wrote about dreams and dreaming Hobson, Alan Hobson and what was his co-author's name?

Q: [Bernay?]

A: I mean their idea was, "Oh well we know why dreams take place, there is an activation of a certain system in this part of the brain stem," and that may be absolutely true. I wouldn't say anything against it because certainly as an analyst I don't know one way or the other. But they go on to say what happens then in the way of conscious experience during sleep, that is "the dream" as a conscious experience during sleep is just random. It's like a cat walking on the piano keys, it has no meaning, and so on. Well there they're just talking nonsense. Because they ignore the findings of psychoanalysis. So that is an example that I happen to have heard about and know about. But anything similar could happen too. There are lots of people who do lousy research. And to try to do any research on those aspects of mental life that psychoanalysis has illuminated without paying attention to the findings of psychoanalysis is poor research. I don't care who does it. It's uninformed.

Q: Do you think there's any chance that one day there will be a move towards using psychoanalytic ideas in that kind of research?

A: Oh sure, I think that there are people who try to, yes. Who try to be guided by that. For instance, when I was visiting my daughter Lucy in Ann Arbor, I met and talked with a fellow who gave a talk at our Institute. Although I don't think you may have been there. It was one of the nights when the weather was worst a year or so ago; it poured cats and dogs. Howie Shevrin. And he's doing electro-encephalographic research that is analytically informed.

psychoanalytic society out there. **[end of tape; new tape for interview portion taped on November 15, 1986]**

He's trained as an analyst and functions that way. And he has a colleague who's an engineer, an electro-encephalographer, and so on. And the two of them think they've doped out a way of correlating certain psychological phenomena, especially perception of stimuli, with electrical activity of certain parts of the brain. So that he's working with, what they call it is "evoked response potentials." And his work is all analytically informed and analytically oriented. I don't know enough, haven't read enough about it to be able to judge it as research, but it certainly seems promising. And I think there is something to it. It seems to me that there has got to be. And it seems to me the right direction in which to go, the sort of direction that is, you know, sensible.

Q: So you think there's a place for analyst researchers to go in that way'.) That there is a place for us to do research in that direction.

A: Absolutely. I don't know how profitable it will turn out to be but never does anybody, you know, when you do research.

Q: Talking about research, what directions do you think psychoanalytic research will take? What areas do you think are fruitful to look into?

A: Well, I think any areas are fruitful to look into that a person is interested in looking into. Now I think that there are still plenty of discoveries to be made, plenty of things to be found out, by application of the psychoanalytic method in the clinical situation in the psychoanalytic situation. And I don't think there are all that many colleagues who are or have been really -- who are now or ones who have been in the past -- all that good investigators. You know we talked about that and we have a chance to see it when we sit down and discuss with colleagues in seminars like the Kris Study Group and so on. And we can see how uneven people's abilities are because your ability to do good work, psychoanalytically, is so much dependent on being straight in your mind about what it is you're trying to do. That is, unless you "understand your patient" it's very hard to know what's happening. Well, that's something that would take an awful long time to talk or write about. But you know what I mean. It's really so. And just take as an example the role of depressive affect in conflict. It took one heck of a long time to really work that out. And yet once you realize that it's so, then there's no trouble seeing it. It's not one of those things that has to be inferred from very tenuous data. It's right there staring you in the face. So I think there are a lot of other things that are there staring us in the face

when we're able to look at them, you know? It's just that way, it really is. You see that in research. Not just in psychoanalysis, but all the time and in various fields of science. Certainly in the biological ones that I know the best. And I think there's still a lot to be done in the way of developing people who are really skillful and reliable in the use of the analytic method, and interested in what it has to tell us about mental functioning. So that I think there's still a lot to be mined from that shaft.

Now what about all the computer stuff and what about the recorded interviews and what about all of those things? Well they're worth trying and doing, you know, if people are ready to do them. And what they'll show, I don't know. It's very hard to tell. Since we talked last I was asked if I would be the principal speaker at the occasion of the 75th anniversary of the American Psychoanalytic Association. Friday will be a sort of celebratory day, next May down in Washington. And there will be, as I understand it, some kind of formal greeting from somebody in the government or similarly placed, as high up as we can get who's worth listening to! And then, after a very short business meeting, I will talk in the morning, and in the afternoon there'll be two panels. One is a panel of three historians: Peter Gay, who's an analytically trained historian in New Haven, of whom you heard, I'm sure; and another man, Professor Emeritus of the history of science, I.B. Cohen, from Harvard, whose work I know a little bit because he was one of Elsa's teachers. He has been Professor of the History of Science for some time at Harvard (or was, for some time, because as I say, he's emeritus now). And another historian whose name I don't know. They'll talk about psychoanalysis and its impact on the intellectual scene, perhaps particularly in the United States. And then there's a panel of three analysts -- Bob Michels will talk about psychoanalytic education, some sort of historical and prospective talk on that. And Leo Stone, I think about technique. And Wallerstein about theory, or research maybe particularly.

So having been asked to do that I've been thinking along sort of these lines, you know, what I am going to say and talking with various people about it. And so, for instance, in 1950 you would have been virtually certain that the direction of psychoanalytic research for the twenty or thirty years to come would have been in the direction of psychosomatic illness. That was where the interest really was. That was where the action was. That was what people were talking about and thinking about. It turned out to be absolutely wrong. Absolutely wrong! And a couple of other things. Twenty years later the big news was ethology, comparative behavior of animals and man. And so we had Lorenz and his gray-lag geese, and Tinbergen and a fellow right here in New York who was a very fine speaker and I think a very good scientist, an animal psychologist, by the name of Birch. And Harlow with his monkeys, and so on. The interest in that all of a sudden disappeared. There is nothing more, you don't hear anything about it. So it's very hard to predict. I don't know where the action will be twenty years from now. If I had to say I would think, as I said, that the analytic situation would still be one of the places where people who are really interested in how the mind works will spend a lot of time. Because I think that's a place where you can find out an awful lot. But that's been my own experience in my professional life. That was what I devoted my life to, was becoming skillful in the use of the technique, questioning it always, trying to improve my understanding of it, and applying it. And it worked for me, you know. So I think it can work for other people too. But I may be all wrong, you never know. But that has been

my experience in science in general. At least in the branches of science that I have had anything to do with. And they pretty much have all been chemistry and the preclinical and clinical sciences. I've found that learning the method, that learning how to do it, whatever it is, is a big part of getting good results, worthwhile results, dependable results. It doesn't seem like it, but it really is so. And of course everybody is influenced by his own experience. So that's the experience I was influenced by. How to do EBG's and electrocortograms and things like that and what the sources of error are and how to deal with them and long before that the same with chemical analyses too. Which is where I started in laboratory work.

Q: So you had an interest in research all along?

A: All my life, yes.

Q: Because you could be very adept at the psychoanalytic method and continue to, let's say, look at depression the way you were taught to look at it, and keep doing it the same way. You at some point felt dissatisfied with a way of looking at it and then you had a different way of looking at it.

A: You could put it that way. It wasn't that I was dissatisfied. It was that I was always interested in understanding why do it this way, why do it that way. Is that the only way, is that the best way. You know, more like that than to say I was dissatisfied. I didn't know enough to be dissatisfied or satisfied. But as you learn a method you have to be skeptical about what it has to tell you and what sort of questions you can use it to answer. And is it really doing for you what you want. These are generalizations, I know, I'm not really satisfied with them but I can't be that much more specific right at the start. What are you hearing when you hear a patient talk? How do you put those ideas together, the impressions together that you gather? That's not so easy to do, you know. It really isn't. And how you think about the data that you get in any method of investigation, in any field of investigation, how you think about those data, how you connect them, the one with the other, has a lot to do with learning from the data, with drawing conclusions from the data, either about the individual patient, let's say in analysis, or about any sort of nonpersonal, you know, biological or medical study. How do you understand it is a big part of making observations.

Q: So in terms of understanding how you came about with your findings in depressive affect, your ideas about transference, superego and so on, you were saying it wasn't so much that you felt dissatisfied with the accepted theory of depression and started questioning it, but that you would have a general questioning attitude towards every aspect of psychoanalysis and those were the areas that you . . .

A: Yes. You know, you could see it wasn't exactly what you read in the book it was supposed to be. So how do you figure that out? Was there something either wrong with your understanding of what you read or is there something wrong with what those people said about the data of their observation?

Q: So unlike some kinds of scientific research where somebody happens to at one crucial point look at something differently, or fall onto something, in your case it was

really a constant questioning and trying to understand and trying to correlate clinical material with theory that brought you to these insights.

A: Well, I think so.

Q: It wasn't one day you woke up and said I've been looking at depression the wrong way all this time

A: Oh no, it took a long time to figure out. I knew there was something wrong but what was it? Why didn't the data fit better? What were the reasons for that? I would say it was a slow, slow process.

Q: Uneasiness with what the correlation

A: No, obviously with certain observations, you know, and even what I read in the literature. I can't remember exactly how it went, but I know the certain points where I read - you know I remember certain things about it. For instance, it seemed to me that some of the patients I had who were clearly very depressed and unhappy didn't have primarily oral problems, they had primarily phallic problems. This was what they were very unhappy about, was that they felt themselves to be castrated, whether they were men or women. Well it didn't seem to be in the literature anywhere, although if you go back to Abraham and his first papers on depression and you really read it carefully, you'll see that he said they have very intense conflicts over their oedipal wishes. So he did understand it that way. But he never followed it up, so he gave what seemed to be a very unbalanced picture. And then you read Fenichel, who quotes a case where clearly his patient was very disappointed because a little brother was born and she was neglected when she was two and a half, or something like that. And then Rochlin who reported these cases where the question was whether there was a phallic triumph or not. If there was they felt very happy and enthusiastic and if there was a phallic defeat or a feeling that they had been castrated there was depression. And yet he said, but of course it's all oral. Well that's the sort of thing that I mean. There's no doubt in my mind that there were oral fantasies, wishes, ideas, and so on, expressions of conflict in the patients that were reported on. But they were focused on because the observer was skewed and biased in his approach. So that I remember. But I don't remember the day to day experiences.

Q: But when one reads your papers or your books they are easy to read. But the arguments in them are very-everything is very tightly argued. There are no shortcuts and loopholes in them

A: Yes.

Q: How do you go about that? This is the way that you are thinking? Or as you keep thinking you keep asking yourself questions to try and solve the problems that come up until you feel you have a more complete solution?

A: Well I try to ask myself a question. If I propose an explanation, I always ask myself the question: "Can I find an example that doesn't fit?" This explanation sounds pretty good but maybe it doesn't fit other data. So you've got to change your explanation if the

data are reliable, valid. And so always any explanation that I try to give I am always testing and thinking about and trying to see evidence in what I hear from patients, for example, that it's wrong or that it's right, that it's, let's say, compatible with what I did. So if it's compatible with enough and if I don't find any situation that contradicts it, then I'm better satisfied. But to be skeptical, you know, it's much easier to be skeptical of somebody else than of yourself, for most people. I notice that, and I'm sure most other people would agree that it's so! So that's one of the things I try to cultivate, is skepticism about my own ideas just as much as about anybody else's. So when I say something usually I try not to say it unless I've tested it and argued against it in my own mind. But I don't always remember what the arguments are or what the tests are and so on. It's impossible for me to take notes of all those things. I can't write that fast or anything like that. I can't record it that fast. And it would interfere with the way I think. So I have to hope that I'm not too prejudiced in my own favor.

Q: When does this thinking take place? Anytime, or do you set aside some time to do some thinking?

A: Well there are certain situations that I have found very helpful. As I said, one was our small study group. So that's like a teaching situation, explaining it to colleagues. It's always a lot more comfortable to talk to people that you know, and so on and so forth, and who are able to understand what you're talking about without a lot of long explanation. But teaching situations in general - I love to teach, always have. And it's been said by many people that the way to learn a subject is to teach it. That means you've got to ask yourself a lot of questions as you're trying to teach something to somebody, and be more searching than you might be if you didn't have to explain it to other people so explicitly. So teaching situations are certainly helpful, of those kinds. And what's also for me something very similar, that is, wanting to write up results. And sometimes when you're sitting and writing and thinking about how to present something you realize, well, your ideas about it could bear some changes or at least there are more things you have to know, or more tests you have to apply. Because I don't know how it is for other people, but for me, when I write I always have an audience in mind. I don't mean particular people as an audience, but I'm talking or writing for somebody - for a reader and/or a listener, if it's a paper. And I expect them to be listening with a very critical ear, or reading with a very critical eye. So those situations are ones that I can identify, at any rate.

Q: Do you think that a particular way of thinking about your thinking, as if you were talking to an audience, helps in the lucidity of your papers.

A: Well it helps me. You know, I never thought about it very much, but as I was talking to you I realized, well, obviously then when I give a paper or when I write something for publication, some exhibitionist fantasy is active. Because I'm always in contact through writing or talking with somebody who's listening. Now, as I say, as a grown man, at any rate, I try to have that person listen with a critical ear -- a friendly one, but critical. And so I identify with that person, I suppose, to that extent. But for other people, it may not be nearly as much involved with an unconscious exhibitionistic fantasy, as, at the moment at any rate, it seems to me it is with me. I never thought about it before, Ed. So this may not turn out to be the case, I don't know. But, anyhow, I think everybody does it his own

way.

Q: But I was saying earlier that your papers are very easy to read, meaning they're lucid. So I was wondering if that technique of making yourself feel as if you were presenting it to somebody, helps in the way ...

A: For me it helps. Whether it helps for everybody, I don't know. Because it seems as though there are people who write and sometimes have very useful or even important things to say, people who write to dazzle an audience or to befuddle an audience even, unfortunately. Or impress as being profound and hard to understand, impress the audience with those qualities. One thing -- I deliberately chose to write in a style that's not very literary, that's purely expository. Mr. Bernard Shaw, though he was a liar when he said it, said clarity is the essence of style, or that's all there is to style. Obviously that isn't true of his writing and he was a brilliant writer. But still it's the motto that I try to follow. I've tried to be clear in my own mind what it is I want to say and I try to say it or write it as clearly as possible and as explicitly as possible. I try not to hide things either from myself or from the people that I'm addressing. It's true I have long before, since I was little, I heard from people off and on that I was very logical, or very clear. So it's got some roots somewhere in my childhood that I don't know anything about, I'm sure. Whatever its derivation is, I don't know.

Q: As opposed to it being innate?

A: Well, sure, it could be innate. As I say, roots in my childhood, I don't know what they are -- how much is constitutional and how much is some other, I don't know. But that characteristic of my expression, even when I was just talking as a little boy, being logical or clear, and so on, that goes back as far as I can think, as far as I can remember. And I try, I think because of the subject matter that I write about, not to be literary or flowery except when it's really absolutely appropriate. Let's put it that way. See now Freud wasn't that way. Freud was a very literary writer. He was a man who had an awful lot to say and who said it often very clearly and well. He was a great teacher and all the rest of that but he was--you know, the flourish, the turn of phrase, all of those things obviously came naturally, in the sense that it was a lifelong interest of his. But that I avoid as much as possible. Plain, simple, straightforward, direct -- because I don't want to impress just by the language or the mode of presentation. So it often sounds too simple I suppose. To many people. Well, to some people, I should say. That's a complaint. "It's so simple, it can't be that simple." Well it isn't that simple. It's only that I try to say directly what it is I had in mind and people then think that means what I'm saying is simple, because they can understand what I'm saying. It's not impressive enough, or something.

Q: When you read it more than once you appreciate more and more the complexities.

A: Yes, they're very complicated things that we all try to write about. And you can say what you think about them, but that doesn't make them simple. It doesn't even make your thoughts about them simple.

Q: You mentioned the study group with Drs. Beres, Arlow, and Wanhg. When did that

study group cease to exist?

A: When did it cease to exist?

Q: Well, I was wondering, you know you said you used a study group as a place where you could present your ideas. But what about the ideas in the 1970's, was the group still active?

A: Yes, we met then. Dave Beres wrote the little preface or introduction to *Psychoanalytic Technique and Psychic Conflict* you know. And certainly they heard a lot about *The Mind in Conflict* during those years when the ideas were developing. And we met regularly until Dave retired and then the three of us met until Martin really became interested in other things, you know social problems and so on, and a few years ago left the city, also. So now Jack and I try to get together a least once a month and talk about different ideas. But that isn't really enough, you know, to take the place of more frequent meetings that the four of us used to have. So, I struggle along the best way I can.

Q. So the members of the group essentially knew about your ideas way before they were published.

A: I think so. I think they would agree to that. That's my recollection.

Q. We were talking about your research and research in general, and there has been other research in psychoanalysis in the last twenty years. You mentioned how there was great interest in psychosomatics and so on. Two things that there has been great interest in during the time I've been involved in analysis, has been Kohut's work on narcissism and separation and Margaret Mahler's work on separation-individuation. And in both of these situations, there has been a tendency to explain a great deal through these two ways of looking at things. I wonder what your feelings are about these areas.

A: Who's going to listen to this statement? [Laughter] I think, you know, when Kohut came out with his first book, my impression of it was something like this: That if the message of the book was that he was saying to his colleagues, "Look there are some patient's who don't seem to respond to analysis. But if you're patient enough and if you keep trying to understand, you keep an analytic stance with them and you're willing to sit with them for years, you'd be surprised how much you can help some of them." Well, that's my experience, that there are patients who are very sick and who don't just get better with analytic work, intensive analytic work, over a period of several years. But you'd be surprised how much you can do for some of them if you're willing to spend the time and the effort and stay analytic. So that I would agree with. The rest of it, you know all the self-object and the mirror and this and that and so on, it didn't make any sense to me, it doesn't still make any real sense, and clearly, it seemed to me, any generalizations, theories, about narcissism, that completely leave out phallic narcissism, it's just cuckoo. I mean, what's the sense of it, to say narcissism has to do with pre-phallic, pre-oedipal experiences, and traumata and maybe conflict -- it doesn't make any sense at all. But the other, you know, if that was what a reader would get from it, be patient, be analytic, and so on, that's a worthwhile thing to say. I don't think you need a whole book to say it, but Heinz never was one to spare words, you know. He talked very

freely, and he was a very bright man. And in my experience up until then, a very good analyst. I always thought so. Then, of course, there was the progressive development in the direction of more and more grandiosity on his part, until eventually he came to the point where he was saying that the oedipus complex doesn't normally exist as any problem in anybody's life. And that Freud was all wrong, or basically wrong, and whichever it is, tragic man or guilty man, he's a tragic man, is the Kohutian man. So that the great problems in our civilization are those of tragic man and self psychology will supplant analytic psychology, metapsychology, whatever. Anyway, he obviously, unfortunately for everybody, what we saw ten years before his death in the first book that came out, turned out to be the beginning of a psychological deterioration, I would say. That's really what I think.

And there are various reasons, I am sure, why it has exerted such an attraction for so many people in the field. But one of the principal ones is that it says, in essence, not only this is the way to do it, so it's like a cookbook -- which everybody wants, of course - not only that but, more importantly, that it says you don't have to worry about childhood sexual conflicts. Sex and all that stuff you don't have to worry about because that's not what's really bothering patients. It's all good clean stuff, that's all you have to be concerned about. And everyone wants to avoid his or her own childhood instinctual conflicts. So self psychology, like so many of its predecessors, beginning with Adlerian psychology, is one of those deviant or schismatic movements that in my opinion depends for its appeal on just what I said.

Now what was the other one?

Q: Separation-individuation.

A: Oh, Mahlerian. I always thought Margaret Mahler did something that was really terrific. That was this. She showed by example and said to her younger colleagues -- she wasn't so old when she started you know -- she said to her colleagues, "Look, let's don't just talk about what we think early mental life is like. Let's get in there with the kids and spend some time with them and observe them and study them as little children." And that approach appeals to me immensely. Before her time with very few exceptions, psychoanalytic ideas about the dawn of mental life in early infancy were based on speculation -- or was speculation based on evidence from analysis of older children or of adults. And saying, I'm going to spend time with the children and as an analyst observe them and find out what I can find out, I thought was terrific. So she did it. And of course there are other people who have done it too. There was a center up in Boston which is still there. And Milton Senn established one in New Haven, the Child Study Center there. And there was the J.B.G. Center in child development and obviously the nursery at Hampstead and so on. What has come out of it as far as Mahler is concerned, I'm less enthusiastic about. It's good to observe and all that part of it, but I don't think she was a very good observer. She was much too un-self-critical and, as you can tell from what she writes, for instance in -- what was the first book?

Q: The first one is symbiosis

A: Symbiosis and separation, something like that. Anyway, the first book. So she'll never

contradict anybody, because she doesn't want anybody to contradict her, you see. And you read that introduction and every reference she makes, she agrees with everybody who's ever talked about the field of child development. And it's impossible to agree with everybody, because this one says one thing and the other one says exactly the opposite. You can't agree with both of them, but she does. She agreed with all of them. I guess because she wanted not to be criticized. She was, you'll be surprised to hear, a rather timid woman in many respects, in those respects. She didn't give the appearance of timidity in her ordinary personal relationships at all. And I don't think she was, except intellectually, in that way. She wanted to be praised, it was very important to her, and so she couldn't be very self-critical. And that's a serious flaw. And a lot of what goes for results of her research I think is rephrasing or new words, or ideas that didn't need new words for them. I don't know, that's the way it seems to me.

On the other hand, I think there's one thing that comes out of all of the research that stems from her example and teaching and that is this observation that Roidge and Galenson made about the beginnings of the castration complex, the beginnings of concern about presence or absence of the penis at such a very early age, before the age of two. Isn't that so? At least in the sample that they observed. But again, I tried to explain to them, this is a terrific thing, but now it should be studied. You should look at it in a larger population. How is it with children who aren't intellectually so gifted? What's the age spread? Check it... You know they don't think like investigators, they don't test results, they have some explanations that come out of thin air. "It's not really phallic, it's anal." What does that mean? I mean you're talking about a penis or no penis. It's not really phallic! I've yet to come into personal contact with any of the group of investigators and pupils and so on, who have been influenced by Mahler -- I have yet to come into contact with any of them who has what I would call a first rate mind, or an ability to approach problems in a scientific way. Some of them talk less nonsense than others. But you know, none of them really have any scientific background, they don't think scientifically. And it's a dreadful shame. It's too bad. I suppose sooner or later someone will come along who does.

Now then there are the other people who are studying -- what it amounts to is the neural capacity of new-borns. Now that's very interesting. And they've found out a lot of things with new techniques and superfast cameras and all the rest of that stuff. That's very interesting. But it's essentially animal psychology. I mean, those aren't human beings yet. And the capacity of the central nervous system of the new-born and post-neonate is a very interesting thing to study, but then to say that that has much relevance for the mind and especially the problems that we deal with clinically as analysts, seems to me to be ignoring obvious facts that contradict such an idea. They're deluding themselves in my opinion. But it's very interesting as such. Sooner or later again somebody will do something with it. Or you hope somebody will do something with it, you don't know. They do very ingenious experiments because they're mostly psychologists, and psychophysiological experiments -- who smells what, where, and with animals and so on, and little babies -- that's something that they can do. They can translate their experiments on animals and sensation experiments on adults to working with these little organisms, these new-borns. So it's very ingenious, it's very clever, it's very interesting, but it's not very relevant to what we're interested in -- to mental functioning. And when I say that,

which seems to me so obvious, you know, people, not just "they", but other people can get very angry with me. And I'm quoted as saying I don't believe child analysis is any good, and all sorts of things like that. What was it Sam Ritvo said? (Sam Ritvo, you know I've known him for thirty-five years.) "Well of course Charlie thinks that babies are born without cerebral cortexes." He was just so mad he couldn't restrain himself. That's why I asked you who's going to listen to this stuff.

Q: I have no control over that.

A: You've got to say what you think. [i.e. I have to say what I think, whoever may be listening.]

Q: Talking about child research. You knew Anna Freud pretty well. What . . .

A: Well, not pretty well . . . I knew her.

Q: What did you think of her work, and her?

A: Well for one thing I think she was one of the greatest teachers that there was. Not just a great teacher compared with other analysts but a great teacher compared with anybody. She was a devoted teacher, she was a very hard-working teacher, and she was marvelously clear and persuasive. And years ago she came to one of the meetings of the Kris Study Group when she was in this country on a visit. And I remember everybody was there. We were piled into that Board of Directors room - and I think there must have been fifty people in that room. And I think Ernst was the moderator and Heinz was there, and Rudy and everybody was there, who could crowd into the room. And at one point (I don't remember all the discussion), but at one point she said, "After all, you know, I'm primarily a clinician, I'm not a theoretician like Heinz, for instance." Well you've got to realize this was in the early '50s. So a dozen and a half years after she had written *The Ego and the Mechanisms of Defense*. Well, for me, listening to the person who had written *The Ego and the Mechanisms of Defense* saying well, she wasn't a theoretician, she was really just a clinician, sounded like false modesty. But as time went on I realized she really was right. She was not. She was a marvelous clinician, I think, and has made very substantial contributions not just clinically and as a teacher, but also to the generalizations to be drawn from clinical data. But she was not primarily a theoretician. And when it came to testing generalizations rather than accepting them on authority, when it came to thinking about whether what her father had written, and others after him had written stood the test of her own observations, that she couldn't really do. She couldn't really do that. She accepted a lot that she shouldn't have accepted, that she should have known was wrong as far as theory was concerned. So that was interesting.

Q: But she did disagree with, with Bowlby and Klein

A: With Bowlby she could disagree, yes. But with Sigmund she couldn't.

Q: Do you have any idea, or did you have a chance to discuss your ideas about defense with her.

A: Yes, I did, as a matter of fact. I did discuss them. I spent an hour or so discussing them. She was not especially impressed. And she said, "Oh well, of course we know that any unpleasant affect can be defended against." But that wasn't the point then, you see. And she wrote that in *The Ego and the Mechanisms of Defense*, but that wasn't the way she viewed psychic conflict, you see. She viewed psychic conflict exactly as her father had written about it in 1926. She didn't see that there was anything especially new in putting depressive affect on a par with anxiety and saying that each is a form of unpleasure. She didn't see that at all.

Q: You mean she didn't agree or she didn't really understand very much.

A: She disagreed because she didn't really understand it. She couldn't. Interesting. But she was an extraordinary speaker. I don't know if you've ever heard her talk.

Q: No, I just once

A: Notes were unimportant to her, she just stood up and as Heinz Hartmann said once, "Anna always has two or three papers in her head." And that was really so. You could have taken it down verbatim and not have had to edit it. It was just that clear, and she spoke that way. So marvelously. Now the reason at that time, long, long ago, the reason why she had them in her head and not on paper, I think, was partly the result of an inhibition. I think for a few years she suffered from some inhibition about writing them down. Not that I know her well enough to be able to say that absolutely, but it seems to me that it was so. Whatever the inhibition was, she eventually got over it and of course for the last twenty years of her life was certainly very productive as far as writings were concerned. But there was a time there where some things that she should have written down, she wasn't able to. But she had them in her head. So that when she got up to give a paper to talk, to give a lecture, it was a marvelous thing to witness, really extraordinary. She was an exceptional person, in many, many ways. And, as I say, a very great teacher, and a wonderful clinician. She wrote, I think, the best of all of them -- the best to my liking. Not as brilliantly as her father, not as impressively as her father.

Q: She has clarity of style.

A: Absolutely. And unpretentious, straightforward, concerned to get her ideas as clear as possible and as understandable as possible. What can you say, that is the way she was.

Q: I was going to ask you in terms of this question I had in mind about the development, a little about the history of psychoanalysis. Changes in technique, changes in theory, that you have observed.

A: Well, the big change was initiated really, in 1923, with the ego and the id -- that three-year period that ended with *Inhibitions, Symptoms and Anxiety*. And that, I think, marks the watershed between early psychoanalysis and what we understand as psychoanalysis today. But it took a long time before the message came clear and was really applied in clinical work. And it took people like Fenichel and Anna Freud, who were, of course, associated in Central Europe. Fenichel, to be sure, was in Berlin and Anna Freud was in Vienna. But Fenichel then went to Vienna because Hitler came to

Berlin, unfortunately. So Fenichel, Anna Freud, the people directly associated with them, and in this country, then, in addition to Fenichel who unfortunately died so young, Hartmann, Kris, and Loewenstein, and people like Eddie Bibring, who also wasn't active too long in this country. And others who've been associated with all these people, either in Berlin or Vienna...

Q: What about David Rappaport?

A: ...who, made possible the transition, the real understanding and application of the message that was in *Inhibition Symptoms and Anxiety*. You know, that psychic conflict is what's basically important and that's what you try to analyze. And I shouldn't leave out someone like Helene Deutsch who was certainly a very influential teacher and Robert Waelder as well.

David Rapaport. Well there are certain things you have to understand. Rappaport was a very influential teacher, certainly in the last twenty years of his life, at Topeka and then at Stockbridge. Rappaport was not a clinician. He never analyzed a patient. He was a psychologist who had written about psychological tests of various sorts, and was informed about analysis, but not himself experienced in the use of analysis, and not even a psychotherapist.

Q: Was he analyzed?

A: That I do not know. From the amount he knew about analysis I would think he had been in some personal analysis, but I really don't know.

Q: Well, he wasn't really a psychoanalyst, you say.

A: Well he became important in the field of psychoanalysis as a teacher. And what he taught was mostly what he got—well it was all what he got from reading of course. But it was mostly what he got and applied of what Hartmann had written. I mean Hartmann was one of these people who had great difficulty in expressing himself clearly. Most people found him very hard to listen to. So hard to listen to and so hard to read that they gave up. They said if I can't understand it, there couldn't be anything there. Well, that's often true, of course. But in Hartmann's case it wasn't true. There was a lot there. But you had to get on to his style. And as I've said many times, his style was just as difficult to understand in German as in English. It wasn't that he had any problem with writing English. He was just as fluent in English as he was in German. And he was equally fluent in French and Italian for that matter. He was Swiss, part of his family anyway, or grew up partly in Switzerland. His father was some, I don't know, ambassador to Switzerland from Vienna? I forget, you can read it in the obituaries that were written when he died. But anyhow, he was a linguist, you know, like a professional. So it had nothing to do with the fact that he was writing in English in the last twenty years of his life. And Rappaport, many of the ideas that he put forward, he put forward as a paraphrase of what Hartmann had written. But I didn't think he would ever have denied that. That he was very much influenced by Hartmann. A lot of people didn't understand that because they couldn't understand Hartmann and they could understand Rappaport. So they thought they were Rappaport's ideas. And Rappaport, as I say, was a very

influential teacher. And very highly regarded and respected by many, many people who both in Topeka and here and Stockbridge were associated with him. So Gill, Breman, Schlesinger, Holzman, George Klein, Bob Holt, Roy Schafer—a whole slew of very well known names in our field. And they had learned a lot from him and they wrote a book in his memory when he died, so tragically, so early in his life. Then they intellectually revolted against him and every one of them had one criticism or another about psychoanalytic theory, as he had taught it to them. It was very interesting to see, it's like a parody of Totem and Taboo, you know. Not that any of them would have done it so obviously - if you know the facts of that situation, even from the outside. So Rappaport performed, I think, a very considerable service in what he wrote, and so on. And he was very good. But he never claimed in any way to base his ideas on a personal experience with clinical data, and that's important to bear in mind. And he was very -- he had an encyclopedic knowledge, he was a very bright man, and he was devoted to the ideas of analysis certainly. They wanted to get me to go to Stockbridge. I guess he wanted me to go to Topeka-- I think. I think he and Gill came to me after dinner once, it's so long ago, and tried to recruit me. I think it was when Knight was moving to Stockbridge, or just before Stockbridge. I said no. I wasn't going to go back to an institutional setting like that.

Q: What about technique? You know it seems to me that that comes up in the papers all the time, but that some of the things that Freud recommended as technique, the recommendations, technical things to do or not to do, not only he didn't follow, but many others for a period of time did not really follow them. What was responsible for that and what brought about the change?

A: Oh, I couldn't answer that question in any general way, Ed. I'm sure there are others who could answer it much better than I can.

Q: Even when you went through analysis, for example, at that time analysis was what, one year, two years?

A: Well, certainly a couple of years was considered a pretty substantial period of time. Let's see, what courses did I take in technique? In a continuous case seminar, for instance, in Boston, I had Helene Deutsch for a year or two as the leader of such a seminar. I had Grete Bibring as the leader of a seminar on technique. And the people who participated included older people in the society. It wasn't just class by class because there weren't enough people. There weren't enough of us. And when a course was given in Boston at that time, you never knew when it would be given again. So often even some of the members of the Society would attend. But when it came to continuous case almost everybody who was interested would come so it was quite an experienced group of people although the presenter was always a candidate. So I had people like those two that I mentioned and one of my supervisors was Jenny Waelder, she was my first supervisor. Greta Bibring was another supervisor. Then when I came to New York I was supervised by Herman Nunberg and by Marianne Kris and by Larry Kubie. So those were the people whom I picked because I respected them as everybody else did, and wanted to learn from them. Well, I don't know. It was hard. I think I learned pretty well, you know, I listened to what people said, I learned pretty well. I got the idea of resistance pretty well. But I don't think I was very clear about any

difference between defense and resistance. And I got the idea of interpreting resistance. But the idea of a consistent interpretation of all the elements of the conflict including defense, but not limited to it, or the idea of what constituted superego analysis, I think those things were very shadowy in my mind. And what one tried to understand, dreams and the associations, but not in any logically consistent way. It was you learned about theory, but you didn't apply it exactly to what you were doing. It had to guide you of course, in what you understood about a patient, to be sure. But not in any such way as it seems to me it can be taught, as I tried to teach whenever I supervised - if the supervisee is willing to listen. And if they're not willing to listen I don't always try! And then there was the emphasis on you have to be human with a patient. You know the reaction against just the silent analyst -- all sorts of rules of thumb that there were. I remember for instance there was an analyst in Boston by the name of Hanns Sachs. Now he was, I guess, senior to any of them, because Sachs joined Freud's circle about 1910. It was pretty early. And he stayed with him as long as Freud lived. As long as he was in Vienna at least. And the reason Sachs came to Boston, was that Freud thought he should come to Boston and be the director of the newly formed Boston Psychoanalytic Institute. So Freud "sent" him. (He didn't send him, obviously, he suggested that he would be a very good man and Sachs accepted the invitation.) But he was one of the ones who wore the ring -- he was one of the group around Freud that included Ferenczi, Abraham, Rank, Ernest Jones...well, Sachs was another one. And there were only half a dozen or so of them. And Sachs was one of them. So you'd say nobody should understand analysis better than him. Right? And he was a glutton for work. Well there were reasons for that, I suppose. Because he spent several years of his life in a tuberculosis sanitarium in Switzerland on a couple of occasions. I guess he worked to make up for the time he felt he'd lost out of his life. Or whatever. But anyway he worked very hard. At the time I knew him in Boston he said he saw twelve patients a day in analysis. For instance I remember he said once, "If a patient is silent you react by being silent, if a patient talks you react by talking." You know, somehow like homeopathy. But you know it has some sense to it. If a patient is silent because he wants you to talk then of course you don't just gratify his wish. If he's talking a lot because he wants you to be silent and not say anything, you don't just gratify the wish, so it makes some sense for some patients. But it's the sort of rule of thumb that, you know, our candidates would know better than to talk that way today, I think. And that was the sort of teaching that was around. It wasn't the best, like that particular rule of thumb. But anyhow, as I say, I tried a lot of different things that were recommended by different people, like you should be human with a patient. Well what does it mean to be human, it means to be a nice guy, right? And so you smile a little, or you "make nice" with the patient. You're friendly with the patient, you know, without any understanding then. Of course you're friendly and cordial with anybody that comes to you for help, you know. But that you have to understand the patient well enough to know what it is that you should or can do that will be most helpful to the analysis. And that it all depends on an understanding of the patient and that it isn't ... these interpretations and reconstructions aren't just magic and guesswork that you have to see how they're supported or not supported by what you hear from the patient, what you can observe about the patient. These were all things that I had to work out for myself. Although obviously I could never have worked them out without whatever teaching I got. Both by

example and by instruction. It's very hard for me to say how much was one thing and how much was the other. I don't know. But I have the impression, maybe I delude myself, that not everybody -- there were some people who understood well enough that it's understanding the patient and knowing what the patient's conflicts are that guides you in your work with the patient. But I never heard anybody talk about a line of interpretation. That I'm sure is something that I discovered for myself.

I don't mean to say I never observed it. I must have observed it. It must have been apparent in the way that somebody like Helene Deutsch or Greta Bibring conducted a continuous case. That once they understood certain things about that patient's conflict they didn't just spend the next day saying well the hell with that, let's see if we can find out something different. You understand what I mean? So by example and probably also in the supervision, somehow by precept, I learned it. But I never heard anybody say it. Now that must seem surprising. But I really never did. And I've had supervisees who -- ones who came to me after they graduated and so on -- who really found my way of working and thinking about technique novel. The idea that you're supposed to understand a patient, and be guided by your understanding, rather than just sit and listen and the pot boils and it comes up to the surface and you say whatever you can think of to say.

Kris said to me one day (maybe he said to a group of us, I'm not sure) he said one day, "Well," he said, "there are those analysts who say that you mustn't follow a line of interpretation, you must be always ready to hear something new. You mustn't be influenced by what you know of the patient's life and past and symptoms and everything else. So Fenichel's ideas of constructing a hypothesis about the patient, that's nonsense, you know, or rather, detrimental to your intuition and inspiration and so on." He said, "Well, they seem to be able to do good work with patients. But it seems to me that in my experience..." (I forget how he said it, but more than "it seems to me.") He said in his experience it was an advantage at least to remember what happened the day before. [laughter] Not just to start each day as though there had never been a day before. Which was sort of the way Theodore /Reik pretended to preach. I don't think he ever really did it that way. He couldn't have, it's impossible. But he pretended that that was to be recommended. And sure there are analysts today who think that you should just sit and listen and when you're moved you say what you want to say. It's not the way we think about it here in New York. You know some of the Kleinians, they think that empathy (what we would think of as intuition they call empathy), is according to one of them that ego capacity which enables one to understand the unconscious of a fellow human being. Well, intuition, understanding, whatever you want. Empathy is if you've been thoroughly analyzed yourself whatever comes to your mind about a patient that you're listening to is right. Must be right. Because your unconscious is tuned in to the patient's unconscious. So whatever you want to say, whatever you feel moved to say, you can be sure is okay. Provided there's no countertransference problem and you've been adequately analyzed yourself. But that's what they really say. We both smile at it, but that's what they really say. And they get up in meetings and say this. It isn't just that they whisper it behind closed doors. You know, that's their idea.

Q: Well I just wanted to ask you one last question. You can talk about this or not, I don't

know. What do you see as the future of our Institute and Society?

A: Well I'm no expert on the future of organizations. I'd be very surprised if the Institute and Society don't continue. I expect that they will. And I think that they'll be useful parts of the analytic community. Whether they'll have as big a role to play in the future as they have played in the past it's hard to say. After all the New York Society is the oldest in the country, the oldest with a continuous existence in the history of the world. And there were more psychoanalysts, and I think it's fair to say on the whole a more illustrious group of psychoanalysts, connected with the New York Psychoanalytic Society and Institute than with any other in this country. Let's say in the modern history of psychoanalysis before. And in the ancient history of psychoanalysis, there weren't any others. They were all in New York, with very few exceptions. See when I started in analysis, in 1939, you could say there were a few psychoanalysts in Boston, and there were a few in Washington, and there were a couple in Chicago, but that was about it, there weren't any others in the United States -- except in New York. More than half of all the psychoanalysts in the United States, considerably more than half, were in and around New York. So that isn't going to be true, it's less and less true that the major concentration is in and around New York. And of the major concentration they're divided among several societies, so the New York Society and Institute can't be as singular as it used to be years ago. But as I say, I expect it to continue, I expect it to function. And more than that I wouldn't know. You know in what ways, and what changes there'll be, it's very hard to say.

I can remember the time when a very senior analyst would call up the office of the Institute and say enroll so and so as a student, he has just started analysis with me. Because that's the way it had always been done. In fact it was done even in my early days.... You know, the time was before there was an Institute there was nothing to enroll. I mean so and so started in analysis with a senior colleague and that was the equivalent of a candidate in analysis. And after a few months of analysis the analyst would say, all right, start seeing patients in analysis. "Well, but I don't know what to do." "You start, you'll learn, and go to so and so for supervision." If there was somebody for supervision. There might not be anybody for a couple of hundred miles around. "So you'll learn." Okay that was the way it was done. I think institutes are an improvement over that. They have their problems, they have their inadequacies, and it's always possible to bureaucratize them in the pejorative sense of the word. But it's considerably better than it was when it was completely, entirely, laissez-faire -- unorganized, I guess would be a better word. What it'll be in the future, I have no idea. I mean I wouldn't venture to predict.

Q: Okay. Thank you very much.

A: It's been a pleasure.