INTERVIEW WITH DR. SAMUEL J. SPERLING

by

Dr. Arthur J. Ourieff

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Committee for the History of the Society

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O: Now the way we have been running these is that they have been very informal and unstructured interviews, in which you try to reminisce without any particular order, and to tell us as much as you can remember about the early days, particularly your own personal reminiscences, your own personal reactions to various events. We have recorded a number of these, and the idea of this oral history is that some day someone will come along and will put these together and get a multi-sided picture. In terms of such things as confidentialities you will be given an opportunity to edit this, and we'll try to get you to be as free and as open -- and in terms of people who will have access to it, even after you've edited, you will have control of that. I mean, if you want to restrict its use, or not restrict its use. In other words, this is a tape that is primarily for the future; if there are certain things that you might perhaps want to say, particular things of particular people, for historical purposes, but not necessarily want made public, then you will have the opportunity to keep that out of the editing, or restrict in any way that you wish. We have not yet decided -- this will be transcribed and typed, and you'll get a copy of it, first the rough draft for you to edit, and then when you finish editing it a final copy will be made. And we now have five finished interviews, and I think about five that are in process, being edited. So you can begin any place you'd like -- how you got to come to California. I might say that you are being interviewed because you were among the first candidates' class, isn't that right? And that may be some place you want to start, or before then.

S: Well, my memory isn't too good.

O: It will get better as the tape goes on.

S: It was 1937, or probably 1938, maybe early in that year. I don't know how, that I went to an analytic study group meeting, I don't know even whether it was called a study group then, but there was a meeting, and I can't recall how I got invited there, but I did. It was held in Simmel's home when he lived on South Hudson Street. I think it was the S.W. corner of Eight and Hudson, or something like that. After that, I attended two or three of the meetings. It was held in the living room and seemed crowded. I don't recall whether the living room was small or whether there were a lot of people, but I believe it was a large room, in keeping with houses down in that section.

O: Do you remember some of the people?

S: I recall Mrs. Libbin but what impressed me at the time was a very friendly welcome by Dave Brunswick. He's the only one I definitely recall except that I know Simmel was there. I don't

remember who I heard speak there, but I know in one of those meetings there was a talk by Louis Montgomery, who later was found out to be a faker. He was a lay analyst, a young fellow of rather small built. He was evidently sponsored by Simmel and he spoke rather glibly about some kind of a case presentation. It was very interesting, so it seemed to me at the time. There was an intimacy and friendliness in the gathering that was unusual in my medical background.

I came to Los Angeles in the summer of 1935, and was quite uncertain whether I'd go back to Philadelphia, where I had many good connections. I had been doing neurology.

O: You came in 1935?

S: Yes.

O: As a neurologist, or a resident, or what?

S: My family was here, and I came on a visit, and it was a big issue whether to continue back East, in which case I would seem to be separated forever from the family, or start up anew out here. I finally made that decision, and got my reciprocity. It wasn't until the winter, the beginning of 1936, that I finally obtained all the requirements and license to practice medicine, and then I started to do neurology.

O: You had been here from 1935 on, or did you go back?

S: No, I didn't go back. My own interest had always been neurology, even though I had been warned that most neurologists were doing psychiatry. I was finding this out in actual practice, so I figured I might as well learn more about psychoanalysis. Most of the neurologists that I had contact with, back East particularly, were anti-analytic.

O: What hospital was that back East, Philadelphia Hospital?

It was 6 hospitals. I was connected with the Philadelphia General Hospital, where I actually ran the service in neurology; it was the post-graduate school of the University of Pennsylvania that I was connected with, and had the residentship in an affiliate hospital at that time, called the Philadelphia Orthopedic Hospital and Infirmary for Nervous Diseases where the department men were on the staff but they also had other connections. To repeat, the orientation was mostly neurologic and anti-analytic, although there were numerous psychiatric patients. I had heard A.A. Brill talk about 1929 or 1930 in Philadelphia at the Philadelphia Neurologic Society, in which he provoked and received a very hostile reception.

However, I was impressed a year or two later, and was friendly with Doctor Biddle, who was the first analyst to open up practice in Philadelphia. Before I left, Paul Sloan had come and a group at the Pennsylvania Hospital had started being trained by Nunenberg, who had come to New York.

O: So they started at that time. At that time you were a neurologist who looked askance at these analysts? Or had little to do with them?

S: Didn't have much contact. There were so few of them, but Biddle was readily accepted on the staff of the Philadelphia Infirmary for Nervous Diseases, a historically important hospital. He was greatly respected because of his social background -- the Biddles are one of the most highly respected families in the town, and this spread over to his specialty.

O: The Biddle family that helped get Freud out of Austria?

S: Yes.

O: Biddle and Maria Bonaparte.

S: Yes. One Biddle was Assistant Attorney General. They were top society - "main-liners" - but he personally was very nice, friendly, and socially likable, even though a very reserved kind of person. He lent therefore a certain atmosphere of prestige to psychoanalysis, probably due more to his social background. I mean, my Chief, Doctor Weisenberg, who was the big professor at the Graduate School, Editor of the Archives of Neurology and Psychiatry, and held prominent positions kind of played up to him and you know he wouldn't have done that except for Biddle's status, more than any acceptance of psychoanalysis. Anyhow, there was some exposure to an analyst. I started to read, e.g., Fenichel's "Outline of Clinical Psychoanalysis", went through it rather cursorily, and it seemed to me very comprehensive, which I really couldn't understand, but I had other exposures to psychoanalysis. I had some of Freud back in college, but all this with a lot of scepticism and no real background. Anyhow, there was that latent element, say, that I should learn more about that field, and maybe give up the neurology, because neurology was very fascinating, but not a very lucrative field for the so-called medical neurologist; the neurosurgeons were taking over more and more, and that seemed more so here than it was back East.

O: That trend has continued, hasn't it?

S: Yes. And the neurologists, the medical neurologists, were doing more and more psychiatry. It's an old thing. Anyhow, after roughly about a year or two of struggling to build up a neurologic

practice I started to read more psychiatry and went to these few meetings, and then I decided I'd be analyzed myself. So in the summer of 1938 I made an appointment with Simmel. I mentioned my desire and he quickly said, "Fine, there's a new man that's come here,", that he himself didn't have time, but suggested I go over to make arrangements with Otto Fenichel. So I made an appointment, and after a brief interview during the first day I came in, he said, "Lie down on the couch and start." The fee was set, five dollars an hour, which at the time seemed a lot to me, but of course not in relation to his reputation.

O: From his book?

S: From his "Outline", which had appeared in the Psychoanalytic Quarterly. I didn't see it in book form. I've never seen it in book form, though I know it has been published, but very hard to get. Anyhow that later developed into "The Psychoanalytic Study of Neurosis." I was launched on the course of analysis.

O: Still you weren't a candidate, I mean, this was as a private patient?

S: No, no, this was as a candidate, and I don't recall just when, but it wasn't long before we were having seminars. We, the people in Fenichel's seminar, shortly became Greenson, myself, Dick Evans, who was then practicing internal medicine mostly, and Mrs. Libbin. Reider came shortly thereafter, and Jeannette Lyle who later became Mrs. Carl Menninger.

O: She was an M.D., or was she a lay person?

S: A lay person. There was also a fellow Hendrickson, who went into the Navy early and later began practice as a psychiatrist in Seattle. Evidently a Study Group under the auspices of the Topeka Psychoanalytic Institute was in process of formation at the time, and I was registered as a candidate with Topeka, with Fenichel, Simmel and Deri as Training Analysts.

We met in Fenichel's home. There was also a kind of a literature seminar, held monthly or bi-monthly, in which somebody would present usually a case and there would be discussions about it, but this included the entire group, analysts and candidates. It wasn't the study group formal monthly meeting, where they would have lay people invited, but a seminar group, and even though we weren't analysts we were in this group. So there were only two seminars going on. With time these became more regular and organized. In one of those that I recall, around 1940, Fenichel took up the Glover book, which dealt with the responses to different questions. Glover had asked the British analysts, e.g.,

"How do you handle this problem; how do you handle that problem; do you make an interpretation at the beginning or at the end of the hour, how often?" -- And that kind of thing. And the answers were resolved, of course, by Fenichel, that you do this under such and such conditions, and may do the opposite under other conditions. The book provided the nucleus for a discussion of technique that we went through in one semester.

I stopped my analysis in the Spring of 1942 when I went into the Armed Service. At that time Fenichel had started a seminar, which took up his future book "The Psychoanalysis of the Neuroses" chapter by chapter, testing it out on the group to get reactions and responses. In one seminar we spent about half a year, on the "Three Contributions", going through it in great detail.

O: This was when now, in 1942, or 1938 when you started?

S: When we started, 1938 or 1939, or something like that. And there were reports we had to furnish, I don't know, but between applications and that kind of thing there was definite formal, say, application and reports and so forth.

O: After you were in analysis, after you actually started seminars though.

S: Well, I don't recall, but it was all under Topeka at that time.

O: Who came to the seminars besides the four candidates? You mentioned Margrit Munk. Remember anybody else?

S: Well, I mentioned Jean Lyle, and what his name -- Stewart -- Byron Stewart, whom I knew because we had interned together at Philadelphia General Hospital. He subsequently went to Menninger's, and was a candidate in analysis there, and then came out here and was taking the course. I don't know, his status wasn't too clear at that time. He came a little later, and he had hypertension for which he had some vascular operation, but there were psychological manifestations subsequently, and he dropped out of training. He died about ten or fifteen years ago.

O: Was he a young man?

S: My age. But I'm not sure of the exact time. I know he was either late or he might even have been after the war, because I got back around 1946 I guess.

O: Now during these early years you began to practice as a psychiatrist, or still as a neurologist, or as both?

S: Well, every neurologist did so-called neuropsychiatry, so I was doing both, and I was in the Neuropsychiatry Department at the Cedars of Lebanon Hospital; they had both, you know, they didn't separate them. It was only toward the end of the war that they then set up, under the auspices of the Cedars Hospital, but a somewhat independent psychiatric unit there; Harry Nierenberg became the head of that clinic. This was in 1946 or 1947 and was supported by the Jewish Community Federation Service and merely used space provided by the Cedars Hospital. An independent psychiatric set up under the auspices of the Cedars Hospital was created, headed by Doctor Eugene Ziskind. The psychiatric service then was set up independently as the Los Angeles Psychiatric Service which was quartered in a converted home on South-West Lake Street near 6th Street.

O: Originally it was under the auspices of Cedars.

S: The only relevance to mentioning this was that there was some conflict about which group to go with. I spoke to Simmel about it, and he definitely advised me, which I subsequently think was wrong, to let Gene stew in his own juice, for he couldn't really build a good psychiatric department. I therefore stayed with the purely psychiatrically oriented service, the LAPS. It was a mistake for the analysts to withdraw from supporting the Cedars set up, thereby letting it be dominated by Gene with his anti-analytic orientation. But that, you know, was really a side issue to the analytic movement.

O: In the years before the war, when you were at Cedars, were the other candidates also at Cedars in the Department of Neuropsychiatry?

S: No, only Reider was at the clinic there. Of course, he went into the Service, too. Greenson had had a residentship there, but he didn't do much subsequently at the hospital. The service was wide open, e.g., there was a fellow doing only hypnosis there, Dr. Jacobson, who is the father of the Jacobson who later became head of LAPS; he taught me how to do hypnosis, but that's not very pertinent.

O: What sort of reception did your neurological colleagues give you who were not interested in analysis, that is, were you in some way segregated by the other people because you had gone into analysis?

S: No. Of course, I didn't stay that close, so it didn't make much difference to me.

O: Another question. In those early years -- do you recall where your referrals were from; that is, did you begin to get referrals, let's say, because you were interested in analysis? Did that influence the type of practice that you had? As far as you can recall?

S: No, it became more --

Interruption

O: We were talking about the practice, what the practice was like before the war, as a young analyst.

S: I wasn't an analyst.

O: But I suppose in those days as soon as you became a candidate as far as everybody else was concerned you were an analyst, because there were so few.

S: No. Many people didn't know I was in analysis then, until a year or two passed, and of course the war started in 1939; there was a lot of political-economic turmoil, with the very rapid changes going on in this town, and many doctors being called into the Service, and that kind of thing. I became an examining psychiatrist for the Los Angeles Induction Board. That would take up most of the mornings until we finished up the line of inductees. So there was a lot of things going on then, and of course there was concern about what would happen in the war, and about all the anti-Semitism, and when we would be drawn into the war. Of course, after Pearl Harbor it became only a question of time until we'd have to go into the Service. Everybody was quite upset, especially after those early reverses shortly after we got in the war, so it seemed that your own personal practice and analysis receded in importance.

O: You were away for four years, from 1942 to 1946. You did psychiatry or neurology?

S: Well, I went into the Air Force, because there was the claim that younger officers would make more rapid advance in that service; secondly, there was a great need for psychiatrists. Actually what happened was that the Air Force did not have general hospitals, or even regional hospitals, so they tried to hold on to psychiatrists, but had little psychiatric duties for them. So after a short stay at Santa Ana, where we got some training, I went up to Lemoore Air Field, and was in charge of one of the medical wards, practically devoted to coccidioidomycosis, i.e., valley fever.

O: Where was Lemoore?

S: About thirty or forty miles southwest of Fresno. However, it had the advantage for me, when I was at Santa Ana or up there, of being able to get back to Los Angeles occasionally, and at one period I came back to Santa Ana, but then was sent on duty at the Induction Board in Los Angeles for a couple of months. That was a very good set up for me at the time, getting per diem and living at home with the family. At Santa Ana, Simmel told me to look up Milton Miller, who was the psychiatrist at Santa Ana at that time, but he was rather a cold fish.

O: Was he an analyst then?

S: Yes, he had been an analyst from Chicago, but I guess there wasn't too much you could do. Dick Evans was also at Santa Ana, working there, and I think he sooner or later got to do something psychiatrically. There was always this threat of being moved out, and I was getting kind of restless, and the uncertainty, for you never knew when the orders would come. Finally I got shipped over to Luke Air Field in Arizona, where presumably I was sent to do psychiatry. When I got there, I found that there was a psychiatrist there who had one ward and didn't need anyone else. The medical chief there, scratching his head, couldn't use me, so they sent me down to Ajo Gunnery Base, in charge of a little twenty bed hospital there. We sent anybody who was really sick back to Luke Field. This was about ten miles above the border in the heart of the Arizona Desert, so I thought it would be a horrible place. But during the winter and spring of 1943, it turned out to be one of the most delightful periods that I spent in the Army. I enjoyed it, was my own boss, and had very little to do, so that I was able to study for my Boards, and took them.

O: Boards in neurology and psychiatry?

S: Neurology and psychiatry.

O: Were they separate then too?

S: No, you could take them both, or you could take one separately from the other. I think the difference was you had to get an 80 if you went for psychiatry or neurology both, whereas if you went for neurology you could get a 60 on psychiatry and pass, or vice versa. Anyhow, I knew that it couldn't last long, and I decided then -- I'd been back East at the Board meetings, and at one of the American Psychiatric, often held successively -- to talk to Bill Menninger who had become Chief of Neuro-psychiatry in the Army. He transferred me from the Air Force to the Army who needed psychiatrists desparately. Getting somebody out

of one service command into another was not easy to accomplish by one's self. I was sent to Winter General Hospital at Topeka, where my hunger for psychiatry, or more particularly for some communication with analysts was appeased. There I could attend some of the seminars and meetings at the Menninger Clinic. I stayed in Topeka for about ten months, and then was sent into the consultation service, winding up at Indiantown Gap, in Pennsylvania. This service was similar to an out-patient or psychiatric clinic. Later I was involved in a so-called rehabilitation, experimental program to reclaim neurotics. With the rapid changes in the war, there were frequent changes in the use of the psychiatrically disabled and their psychiatrists.

In 1945, with the whole war picture rapidly changing, I wanted to get back to analysis and Los Angeles. Germany was practically defeated then. However, I was sent to a Camp close to San Francisco. After a few months I was transferred to Camp Hahn near Riverside, where they were redistributing soldiers returning from the Pacific theater of operations. Several months later I was transferred to the Pasadena Regional Hospital where I was Chief of Psychiatry. By the time, 1946, when I had gotten into a position to resume analysis with Fenichel, he took an internship at Cedars, and so that was out for the time being and, as it turned out, forever.

O: He gave up his practice?

S: Yes. As I understood it and as he had so stated to others, he wanted to influence the course of development of psychoanalysis in this country. As an unlicensed M.D. he felt he couldn't effectively exert much influence in the American Psychoanalytic Association; so he decided he would obtain his medical license in this State. I also know incidentally, that he started his fruitless discussion about psychoanalysis with Dr. Ziskind, the Chief of Neuro-psychiatry at Cedars, and offered to analyze Eugene Ziskind to demonstrate its validity. Undertaking an internship was quite a burden at his age, but he evidently felt it was important enough when he did it. Unfortunately he had the cerebral accident, a ruptured small aneurysm, probably congenital, and passed away in early 1946. I was discharged from the Service officially in June of 1946.

O: He had already passed away then?

S: Yes. I think it was about January of 1946. It was quite a loss to all here, a particular loss to me, for I had been looking forward to resuming my analytic training and analysis.

Great changes were occurring in our psychoanalytic situation, leading to the formation of the San Francisco Psychoanalytic Society, and by the time the war was ended there were enough analysts in Los Angeles to set up an independent analytic Institute and Society.

O: Do you remember anything about that? How do you remember as someone just coming back from the Service and not let's say being on the inside of that -- I don't know if you were --

S: No, I was not.

O: How did it appear to you, as somebody who was still in the process of training -- looking for another analyst, or waiting for an analyst, or whatever it was. How was that period, how was it experienced by you?

S: Well, there were many new faces, new analysts and others becoming training analysts. There was Miller and Levy, Charnov, Rangell, at al. I had started some control with Slutsky before the war. He died of heart disease. Grotjahn was here, too. There was a tremendous widening expansion of analysis, and the difference I remember was this feeling of new people, younger people. The analytic atmosphere lost some of the intimacy and flavor of the early days. I had attended the early West Coast Psychoanalytic meetings before the war, one at Ojai and another at the Hollywood Roosevelt Hotel, and became acquainted with the San Francisco group and a few others scattered on the West Coast. I think I gave some of these programs to Al Kandelin; the earliest one I see is in 1946, which was held in San Francisco -- no, that was the San Francisco Society - Psychoanalytic Society. At this time we were still a part of San Francisco; the President was Winholtz, Vice-President Otto Fenichel, Secretary MacFarland; Simmel was the Chairman one day, Erikson another, Barrett on Sunday. Those meetings were intimate; you got to know and meet everyone, and it seemed to me, maybe I'm misjudging, that the papers, though fewer in number, were much more fundamental and comprehensive, and the discussion much more interesting than they became later on. Volume increased, but not depth. Of course, getting away to places like Ojai and Santa Barbara was a very happy and intimate occasion. After that Santa Barbara meeting at the Biltmore they never had us again.

O: Why, what happened?

S: I think it was partly anti-Semitic, but partly also the group stayed up late, told jokes, and were noisy and rowdy, I guess

they felt they were rowdy although it wasn't anything too untoward, but that was such a staid place that it was only last year that they had another psychiatric group meet there, possibly because it's now under new management. Is there anything you would want to ask?

O: Well, perhaps we could go back for a bit more before the war, to see if you could recall some of the flavor of those seminars with Simmel and Fenichel. I gather they were the one that did most of the teaching from 1938 to 1941 and 1942; not so much in detail, but in terms of the flavor, what sort of -- what was your feeling about their teaching ability, what was your feeling about their personalities; what sort of people were they as far as you experienced them as a young candidate at the time. Some personal experiences you may have had with them?

S: As I say, I recall that teaching by and large was done by Doctor Fenichel. Simmel had a little, but not so much courses, case work, something like that, Fenichel was a great organizer. In a meeting he would bring out order in what seemed unrelated and confusing to me in a presentation, and highlight and epitomize the essential issue, which he could do extremely well; I had had so little background myself that it was very impressive, and in the seminars there was a lot of free discussion, as it was a small and more intimate group in the seminar. In the monthly case seminars, where a case would be presented and discussed, they'd go around the whole room to give each one an opportunity to discuss. This included practically everyone, Training and other analysts and candidates, as Deri, Hanna, Fenichel, Simmel, Dave Brunswick, Margrit Munk and Estelle Levy.

O: Did the candidates get a word in edgewise?

S: It wasn't a question of a word in; they'd go around in a circle so everyone certainly had a chance to say something. That was of course the big difference subsequently, that there was this much more intimate group in the seminar, with tea, cookies, fruit, or candy served.

O: But Simmel would not usually come?

S: Yes, Simmel would be --

O: But not the leader. He would be one of the group.

S: No, he was one of the leaders, but one had the feeling that Fenichel could express himself much more clearly. Simmel had

some kind of difficulty, I think with the language as well as clarity of expression. I don't know, he was kind of complicated, and thus, didn't make as effective an impression. Fenichel loved teaching, and could be very clear and systematic in what he expressed, which of course was reflected in his books. He had an encyclopedic memory and knowledge, and could readily cite pertinent literature. One had the feeling at that time that Fenichel, a relative newcomer coming into the local picture -- with a tendency to take over more, might arouse jealousy or rivalry in Simmel. Simmel was not very competitive, and seemed to accept his activity and help. Of course he'd state his own opinions. One of their big differences was that Simmel believed strongly in the death instinct.

I had the impression that people like Margrit Libbin, Dave Brunswick, Estelle Levy, had brought Simmel and Deri here in order to build an analytic training group, that they too were students who wanted the kind of leadership that Fenichel fulfilled very ideally. I have a brief memory of Doctor Libbin, I think he was a doctor, who Margrit Munk had been earlier married to, in those very early meetings in 1938. I don't recall hearing him talk or discussed much. He was presumably the earliest pioneer here, and I think he died shortly after 1938, and didn't have too much to do with the group. I think Margrit was already divorced or separated from him when I first saw him and he was not friendly with the group, with rumors of some paranoid trends.

O: There were then five, let's say official candidates at the beginning. The four that you mentioned, four men, and one woman.

S: Well, Lyle wasn't a candidate, I don't think, but she came there. There were one or two other people that came to those seminars, and I think Byron Stewart came in a little later, and even Evans I don't think was a candidate at that time; he came later. And I can't quite recall, but they were in the group. Evidently it wasn't too officially designated.

O: Who were the Fenichel boys, so called? Weren't there a number of you in analysis with him?

S: Well, there was Greenson, myself, and Reider, I think Stewart later, and Dick Evans -- I'm not sure about Evans.

O: Many more with him than with Simmel?

S: Yes, partly because when he came, Simmel had already been here a couple of years, and had a big general practice.

O: Now let's go to the period of the split. Were you still a candidate at the time?

S: Yes.

O: And we're interested, for this history, in various people's interpretation and reaction and experiences of the split, not only on the inside, of the people actually involved, but the people who were involved from the outside because they were candidates in analysis, and so, as it was going on what did you know about it? What was the feeling, what was your memory of what the fellows talked about, about it, and so on?

S: I can't really recall. I remember Reider after the war did not come back here, he went on to San Francisco. Romy became a member just about the end of the war, and so the original group had been considerably broken up. Newhouse was in there too, not in the original, but not too much later. I don't know exactly when he started, but he was there before the war; he had been at Compton, and stayed on. The analysts didn't talk to me much about it, because I was still a candidate; this was supposed to be going on in secret in the Educational Committee. We knew generally of these differences at the time.

O: How did they become apparent to you as a candidate, in discussions at the meetings, or scuttlebutt among the guys --?

S: A little scuttlebutt with -- some of the analysts would make innuendos. The remarks later would be about the relative incompetence of maybe somebody like May Romm, or that Norman Levy didn't believe in the libido theory; but at first it was kind of vague. There may have been a certain caution about informing the candidates of these differences. Also, I had finished my seminars and was not taught by any of the dissident group. We knew there were differences, and threats of a split. I heard criticism of Simmel that he was making concessions, trying to smooth things over, to hold the group together. I heard reports to the effect that had Fenichel lived, the high regard that he was held by all could probably have held the group together. That may be somewhat questionable, though the tendency would have been, I think, much stronger in that direction. There were all sort of inferences, but nothing specific except the question of incompetence, or they didn't really understand analysis.

O: That was all that you, that the candidates generally knew?

S: I wouldn't say the candidates as a whole, but that was the impression I had at that time. I had the feeling at the time that one couldn't ask the training analysts as they were committed to secrecy. We didn't ask what goes on in the Educational Committee.

O: Did it seem of tremendous significance?

S: Well, remember the actual split happened after I became a member. Certainly it was significant; you could even sense it at the West Coast Psychoanalytic meeting with the group up in San Francisco, where there was also that kind of leaning. Some of it was in terms of the lay analysts' situation, and the position that the American had taken not to train any more lay analysts, and there was some feeling against the lay analysts as compared to the medical ones. This attitude was one held with great conviction, I think, by Levy, Miller, May Romm, and that kind of group in contrast to those who favored them, by people that especially had come from European backgrounds. I think it was Simmel's effort and purpose to bridge this gap by using people like Doctor Tidd, who got along with both groups, and therefore could be agreed upon. It led to his election for Presidentship in those early years, as well as the fact that he carried a certain aura of respectability or acceptability from the medical profession.

O: A number of people have talked about this split being one, let's say, of German versus, or European versus non-European, and yet there was Grotjahn, and there was Alexander, who wasn't out here, but was behind the scenes, or was he not even behind the scenes, as far as --

S: But Alexander's influence was represented indirectly by people like Levy, and Miller, who came from Chicago.

O: They were Americans, as opposed to the only European in their group then, who was Grotjahn.

S: Yes. There was talk after the war about Grotjahn doing unanalytic and unregulated things, acting independently, as it were. He'd see cases quickly, and begin a training analysis, you know, of once or twice a week, that kind of thing. So you heard those kind of reports. What I'm not certain about is whether some of these things are more retrospective. You heard them more definitely sometime later on but there were rumors, or back talk, so that you didn't really know. Subsequently with the split the objections came out much more

definitely. An official statement was then put out by the Educational Committee.

I heard criticism of Simmel, because he had tried to hold the group together, and that he had tolerated too much from these people. In his attempt to build up the Institute he had favored May Romm, who later became practically the predominating influence in organizing the other Institute.

O: What about some of the -- let's begin in terms of how it was from your point of view. What was the role of somebody, say, like Doctor Lewy, in terms of the candidates or young members, or let's say the non-Education Committee members? What was the understanding of, let's say, his role in the split, or Charlie Tidd in the split, or was that not a thing that ever --

S: I didn't know. I met Ernst first in Topeka, when I was there in 1943, and subsequently he was cautious in making a major move to come to Los Angeles. I think he was very reluctant to express himself definitely; in another sense, of course there was no question that he was aligned with the old group, the European, or the so-called orthodox group. I had the impression that he may have been quite reluctant, as a relative newcomer, to take overtly a very active role. What went on in the Educational Committee I don't know.

Incidentally, during the war, when I was at Topeka, we rented a home for a couple of months which belonged to the son of the president of the bank there. It was a California ranch style home, out on 26th Street, about four blocks or so west of what was then the edge of the city, and with the wide open spaces there, that was unique for that area. We gave a few parties there, and one was for Simmel when he came through. It was a big affair, and I invited, naively, the whole Menninger crowd for this particular evening. Karl had wanted to see that home, to which he had no access as the Menningers were then not too well regarded by the local social community. There were too many delinquents, and stories about the terrible treatment, and what these terrible people often did in the town, which was in general an ultra-conservative and rigidly moral kind of town. With the appointment of Bill Menninger to the Surgeon General's office, as Head of the Neuropsychiatric Division, a great deal of prestige however accrued to them. Anyhow, we had the group out for the big shindig for Ernst Lewy, and subsequently --

O: You meant for Simmel.

S: Yes, Simmel -- and Lewy was there, and Margaret Brenman and Will Gibson, her husband -- in fact, for a few months Bea took piano lessons from Bill Gibson who subsequently received great recognition as a writer and playwright. Knight, Rapaport, Gill and others were there. I learned subsequently that this was one of the rare occasions that the whole Menninger crowd came together in a social gathering. There were some very strong social disagreements, so that some analysts, and especially their wives, wouldn't mingle with others. I in my naiveté had invited --

O: Sometimes that's the best way. As you observed the split then, the protagonists, so to speak, were on the one hand May Romm, and on the other hand Simmel -- these were the two, as you experienced it, or --

S: No, I wouldn't say that. There was definitely May Romm, with a coterie of people around her, as Milton Miller, and Grotjahn and, I think, Norman Levy who were then training analysts. I think it was part of the deal in keeping things together that they made Frumkes and Norman Levy training analysts. Once they had four training analysts, they could split off, to become an independent Institute. I did not have the impression that it was primarily people like Simmel that led the fight. Rather he gave me more the impression, I don't know how valid, of being a front man, of making strong efforts to hold the group together. There were strong forces behind him, possibly represented by Brunswick, Hanna Fenichel, Deri, and Greenson; I think Greenson became a member around 1942, just before he went into the Service, so he nominally had his five years membership when he got back. There was a great need for training analysts then and he also was made a training analyst. This helped lay the groundwork for the subsequent split, for the number of members was sufficient for an Institute and a Society, and in a few years the break finally came. There was the sense then of one faction trying to hold things together -- I don't know then what might have developed. It was predicated on the belief that it was better to stay together with the hope that the influence of good analysis would seep through and affect the dissident group. Another faction felt that it was foolish to try for this goal, that it would be confusing, with splits, arguments, differences running right down through the organization, so that things would be much clearer and simpler were there a complete divorce. My impression was that Simmel was trying to avoid that separation, and there was some critical feeling towards him as leaning too far back with concessions, in his efforts to avoid an open break.

O: Are there any other memories, anecdotes, personal experiences of your early training or during this period that you think should be on here? Any impressions?

S: After I got back from the Service, I started practice. I tried to get the style of different analysts, so I thought I would control a case with May Romm. The patient was not too good an analytic case -- one who was pushed into treatment by her husband, a well known song-writer. May Romm was not very helpful. She began by making certain inquiries about this person, and how I got the referral, -- that seemed not quite in order, but I ignored it at the time. We could see that this patient was fighting against the treatment, that her illness was a hostile way of getting even with her husband, and she was struggling against keeping in analytic treatment with psychosomatic symptoms and she ran away in about three months. As I looked back at this control experience I could see that somehow May didn't even warn me of her impending escape, or help me to prevent it. I was trying to point out this, but maybe nothing could be done with such a patient. Subsequently there was a divorce for which I didn't blame the husband. She was just fighting the treatment because her husband wanted her to recover and preserve the marriage, because there was a child in the family, but he was pretty exasperated by her failure. It was very disappointing that May couldn't help and I don't think really understood the psycho-dynamics of the case. I did have a certain feeling that May Romm has her own unique style. There were two things I could give her credit for. One is that very few analysts can get a point across by telling a patient a joke; she could do it effectively -- I think nobody else --

O: Freud could.

S: Well, I don't know.

O: At least he was supposed to have.

S: The second thing is, she is said to have made the comment that if she couldn't get anywhere with a patient in two years she was through with the case and called it quits. I think this was just the recognition of her experience for she didn't analyze a lot of the transference neurosis, and so the analyses came sooner or later to a dead stop. She cultivated the positive transference and not the negative, and they either ran away or she called it quits, so you've got to give her credit when she called it quits, because she knew that was as far as she could go with them.

I also controlled one case later with Milton Miller, and we had great difficulty, because I kept pointing out certain resistances while he would keep asking me about and focusing upon the dreams. He understood dreams pretty well, and could point out certain psychodynamic changes going on, but I felt it was ineffective to keep interpreting dreams with such a patient, who used her intellect and dreams as a resistance. Miller was very persistent in primarily wanting the dreams, while my feeling was I could talk and interpret dreams and it wouldn't mean anything, although I did do that. But he was very rigid and rather intellectual in this insistence on minute dream interpretation. I think he was influenced by French, who did this kind of minute dream analysis.

That was my experiences with those representatives of the other group.

O: O.K. For the History Committee let me thank you.

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