

CF: Anyway, we had a very good time. I worked eight hours; we were on duty one night a week; you could live in New York and have a practice. During that period we had our two kids, one of them born on Staten Island in the Marine Hospital. Well, that's enough about Ellis Island. I published three papers on fugue-states, which have become....

AR: One in War Medicine. Where are the other two published?

CF: There's a long article in the *Quarterly*, two articles in the *Quarterly*, one with Ed Joseph in 1948; those have become kind of classics, those papers. I provided the first classification of fugue-states and very little was written on the subject. Analysts don't see them. During these years my analysis with Harold came to an end and I was still trying to make up my mind where I belonged professionally, whether I wanted to get into the New York Institute, and for a few years I didn't do anything. In 1946 I went into practice. I had a little office in my apartment. In 1949 I got an office outside the apartment. It was in the Croydon Hotel and it was an office that I shared with Clara Thompson.

AR: Really?

CF: Yeah, and so I was there from 1946 to 1949. Now during that period I still had contact with the Washington people. There was formed the Eastern Branch of the so-called Washington School of Psychiatry, which later became the William Alanson White. You know the history. Thompson, Eric Fromm, a bunch of people defected from the New York Institute--Horney and Silverberg in 1946. Horney was a dictatorial woman and the Institute remained medical, you know, and so there was a problem about Eric Fromm. Finally, later Eric Fromm, Clara Thompson, and Silverberg and others, Janet Ribah, split off. Silverberg, by the way, went to Flower Fifth Medical School and formed his own group, which is still around, and that was really the first medical school that analysts got connected with, I think.

That was a time I had a chance to read a lot of Freud and analytic literature, and the Washington School of Psychiatry was for a little while giving courses and sometime around there--was I still at Ellis Island?—I took a course with Eric Fromm and I had a very bad impression of him. I noticed that he would always quote Freud incorrectly and I always had a pathological bias toward truth. I would be finished with someone as soon as I noticed--like I was with Fromm-Reichmann--as soon as I noticed that they didn't tell the truth because I thought that we were in a field where we dealt with so much fantasy and had so much trouble knowing what the factual basis of our work was, that if people were telling lies, we were sunk, so it isn't accidental that I preferred to deal with experimental data, where it's much more clear what a datum is than what we ordinarily think of. Anyway, then I had some dealings with Sullivan. Sullivan was connected with that so-called Eastern Branch of the Washington.

AR: Sullivan was in New York?

CF: He had come back to New York. He went to Washington; he was with the Washington Society for a while. This was long after Shepherd Pratt. This was long after his famous experiment with schizophrenics. You know about his experiment with schizophrenics?

AR: No.

CF: He and Silverberg, way back around 1924, supported by the authorities at Shepherd Pratt (Sullivan had already acquired a reputation for being able to treat schizophrenics) had a special ward to treat young schizophrenics' first attack at 18 years old or so. He wanted to choose the personnel, that is, the attendants, who took care of them, so he chose male nurses, I guess homosexual males, and what he actually did--it became kind of a scandal, although it's never really been written up--he permitted or encouraged the male nurses to have homosexual relations with these newborn schizophrenics. The idea was to gratify their unconscious

homosexual wishes which were making them psychotic. How that was supposed to cure them never became clear. This was at Shepherd Pratt. He did this with Silverberg. I had heard about it—now Silverberg once—and my memory is so faulty—I don't know whether this was after I got to New York or how I got in contact with him. He once gave me some of the notes, ward notes, nursing notes that were kept about these cases. No one had ever seen this stuff. Neither Silverberg nor anybody else had ever.. published them. Shepherd Pratt might have had some government money to do this. Anyway, the funds were withdrawn and they hushed it up and it never became public; it never got published.

AR: Nobody got sued?

CF: Nobody got sued. Sullivan had some kind of idea--one of his things was that if a boy doesn't have a chum, you have to have a chum period, early adolescent or preadolescent, if you're incapable of having a chum, that is a dire predictive factor for schizophrenia.

AR: He had a chum, a Dr. Sullivan, who was very important in his adolescent period, as I remember from the book.

CF: Well, he thought you needed a chum when you're a kid, it takes place when you're seven or eight years old, that's when you're supposed to have a chum. He didn't have a chum. He was raised somewhere in isolation.

AR: Yes, upstate New York.

CF: Upstate somewhere. I must send you that thing that Farber wrote about him. It's a brilliant piece.

AR: So you took a course with Fromm, you were unimpressed with him.

CR: Yes, I was unimpressed with that. And then I was once invited by Clara to present my material on fugue-states to a group and Sullivan would be there, so I did, and Sullivan foamed at the mouth. He completely dismissed it because I talked about the unconscious and all that stuff and my formulations were all Freudian, and then I got some impressions of him which finished him off for me. He boasted he never read anything Freud wrote after 1921, as if that was something that deserved some accolades. And Sullivan was a bitchy fag and full of contempt, so after that I wrote Sullivan off, too. I had read a lot of his works and never thought much of them. I thought they were pretty empty, he didn't have much to say. The more I saw these people, the more admiration I had for Freud. Also, I was analyzing the best I could in those days and my own experience was pro-Freud. This was from my own personal analysis because Harold was a Freudian and Edith Weigert was still a classical Freudian and conducted classical analyses. So of all these people of considerable talents, even originality, what I was most impressed with was their defective characters, and yet a man like Sullivan could gather a following, he had a very charismatic quality, was a cruel, cruel person, vicious. So in 1947—when I first came to New York, Edith Weigert had given me three names, all Berlin analysts. one was Edith Jacobson, one was Carl Harold, so I went to see Edith first, but I kind of had the idea that I wanted to go to a man, so I went to see Harold and I liked him and began analysis with him in 1943.

AR: The last time when we left off we had gotten you to New York and hooked up with the New York Psychoanalytic Institute and even gotten you to become a member.

CF: Had I become a member? I guess I told you how I was inducted into the Society by Edith Jacobson and Annie Reich. I had no idea of becoming a member at the time. As a matter of fact, I didn't even think about it. I was absorbed in my own analysis and struggling toward mental health, that distant galaxy which I never hoped to reach. I never thought of becoming a

member. I never asked to apply. Edith once said to me, "Don't you think it's about time you became a member?" So I thought I would have to take some more courses.

AR: What year was that?

CF: 1947. I stopped my analysis with Carl Harold I guess in 1944, so from 1944 to 1947 I was adrift. I purposefully wanted to be adrift because I was breaking away from those Washington people and seeing a few patients on my own and trying to make up my own mind where I belonged and where I increasingly thought I belonged was with Freud. During those years I did a great deal of reading and the more I read, the more Freudian I became. Carl Harold was Freudian and that influenced me and, of course, Jacobson. But I was not much interested in those things at the time. I was just mostly looking for someone who would analyze me, because I was in very dire need, so about 1947 I was already at Mount Sinai, the war had ended, and I volunteered to work at Mount Sinai six hours a week and that was a great experience. I came there in 1946 and in a few months it will be 40 years since I've been there, and there was a remarkable bunch of very talented people.

AR: Moe Kaufman was the chairman?

CF: Moe Kaufman was the chairman and he set up a very gemütlich atmosphere, everyone called one another by their first name. He was a war veteran and half the others were war veterans, and that kind of produced a feeling of camaraderie. It also was that glorious time when Kaufman had established an inpatient ward at Mount Sinai, full of psychosomatic cases, and the first great wave of optimism was that psychoanalysis was going to solve the psychosomatic problem. There had already been a lot of writing then, like Flanders Dunbar and Sidney Margolin, and a lot of other people were writing, and it was believed that psychoanalysis had something to offer and also that we would provide a therapy.

So I established my private practice. In those days it wasn't difficult to get patients, because beginning about 1946, after the war, there were floods of patients and I think I was making \$10 to \$15 an hour, that was the going rate, so I stayed in private practice for eight years and just saw patients. I became progressively dissatisfied with that because, frankly, I didn't like the loneliness of it, I didn't like the idea of not having anyone to talk to. I did have one association which was an influence on me. I had my office in the Croydon Hotel, I shared it with Clara Thompson. Sidney Tarachow had his office there at the Croydon Hotel. By the time we had both moved in, we had become friends and every day we'd have lunch together and we'd go for a walk. Sidney was in analysis with Heinz Hartmann, so Sidney would tell me about the wonders of being in analysis with Heinz Hartmann. What I had had absolutely no training in or understanding much of was the analysis of the transference, so Sidney used to tell me all about the subtle things that got analyzed in the transference, so I considered that one of my first and greatest teachers was Sidney Tarachow, he really had a great influence on me.

AR: He was a great teacher for a great many other people as well.

CF: Yes, he was a good teacher. Did you know him?

AR: A little bit, because I was at Downstate.

CF: He was a great teacher and a great wit, a great storyteller, a very lovely human being. He was very helpful to me. Those years are kind of dim. We had our two children then, Carla was born in 1943 and Barbsie was born in 1946. I'm talking about after 1946 when I was in practice. I didn't have any affiliation at the time. I started my analysis with Jacobson in 1947 and I was going to Sinai and became a liaison psychiatrist, but interestingly enough I was always looking for something to research, that is, I had a yen for doing experimental work, I had a yen for experimenting with anything psychoanalytic that I could lay my hands on. Not that I was

interested in confirming psychoanalytic propositions; I was interested in the experimental exploration of psychoanalytic propositions. They might or might not confirm Freud but they might produce some new knowledge. I was interested in a new discovery because the fun of research was always to feel that you're going to make a new discovery.

AR: Yes, that you're on the frontier.

CF: Yes, that's what I enjoyed. Anyway, one of the influences on me was Rapaport's book on the organization of pathology and thought, which came out in 1951. Did you ever read it?

AR: Yes, early in my career.

CF: I read that avidly because it had a lot of things in it that interested me.

AR: It's organized like the Talmud, like the Bible, with commentaries on the back—the same format.

CF: You know that Rapaport—I just flipped through the book lately—there's a little bio of text and then pages and pages of his commentary. Rapaport was a funny guy. He hardly ever wrote anything that was original. He systematized Freud and he systematized Erikson, and *The Organization of Pathology and Thought* was not original but the footnotes were original, full of original criticism and original ideas, but he was a man of tremendous ambition, who was inhibited in writing anything himself—there are people like that—but he could write these critical things about other people. If you gave him a paper to read, he was famous for this, if you gave him a six page paper to read, he would write a six-page critique of it, single-spaced, take endless time, but he obviously had some block about writing himself. He wrote a book called *Emotions. and Memory*, which was as close as he ever came to writing a book, and then one article which he thought of as his most original article, a short article on activity and passivity—do you know what he did with it? It was published in an obscure Spanish journal in Spanish and it was only accidentally discovered.

AR: Didn't George Klein pick up on it?

CF: George Klein picked up on it. I think I picked up on it. I found it somewhere. Anyway, it got back here and was translated. So he didn't have a big output, but his influence was on others and he could be a very hard taskmaster.

AR: He was an influence on his counterinfluence.

CF: That's right. So Klein and Holt and Irving Paul and Schafer and I think Fred Pine and Spence and Luborsky, that whole crowd, fell heavily under Rapaport's influence and he was a real slavedriver. There are others who came later. Anyway, in *The Organization of Pathology and Thought* there was frequent mention of Poetzl but he mentioned him in such a way that I never quite caught on to what Poetzl had done; he'd talk about he did something about apperception within perception, or apperception and perception. I didn't know what the hell that meant. Anyway, I'd done this one research with Leslie Farber on experimental dreams. However, I did do something quite independent on my own in 1952-53, I wrote two papers on the nature of suggestion. I did mention them to you.

AR: Yes, they're in the *Quarterly*.

CF: No, they're in the *Journal of the American, JAPA*. Did I tell you the story? They were picked to be the lead article in the first edition of the *Journal of the American Psychoanalytic Association*.

AR: There were a lot of very illustrious people in that edition.

CF: I thought it was great—the lead article is. always— I thought that was a great honor. I was terrified at even handing it in because I thought I'd be excommunicated for doing what I did. I would stop patients in the middle of their hour and say, "Tonight you're going to have a dream about your father and you'll remember it in the morning and write it down and bring it to me the next day." Now, that's interference. At any rate, I wrote those two papers, which were interesting and really said something about the nature of suggestion. It was accepted and then I called it back as not ready to publish, I had left something out. I had left out a certain control which both Merton Gill and Sidney Margolin had pointed out to me I should have done. Agreeing with them and being conscientious, I said, "I don't want you to print it now. I'll rewrite it and send it," so six months later they were published. I've never heard from them since. I only know one person who ever seems to have read them and that's Leon Chertok in France. The editorial board must have thought something of them and wanted to publish them.

Anyway, I mention those papers because they have something to do with Poetzl. I bumped into Lou Linn one day and he was carrying a book. He said, "Have you ever read Poetzl?" I said, "No, but I've always wanted to." He said, "Well, it's very interesting. You want to take it?" So I said, "Yes." It was in German. I took it home and sat down with a dictionary and ploughed through this terrible German, which I have described as "the most turgid German that ever flowed from the pen of man." I was able to figure out what he had done and what the Poetzl experiment amounted to, and I got very excited. You understand, these articles were written in 1917. This was 35 years later, and no one had ever bothered to repeat them, although Freud devoted a long paragraph to them in *The Interpretation of Dreams*. He said this was the most interesting dream experiment that anyone has done; they stand apart and they have implications not only for the psychology of dreams, but they go way beyond what concerns analysis. Now Poetzl had been a member of the Vienna Psychoanalytic Society for three or four years, and he was a neurologist who belonged to the Vienna Neurological Society. He became interested in analysis and wrote a long monograph on what came to be known as the Poetzl phenomenon, which shows the influence of Freud. He dropped out of the Vienna Society. Certain people remained interested in his experiments. Schilder mentioned them from time to time. He influenced Schilder, and a couple of Americans named Malamud repeated the work. They did an interesting experiment. They used patients in a mental institution. They exposed pictures, not tachistoscopically but for 30 seconds, and were able to show the same phenomenon, that is, even looking at a picture for 30 seconds, part of it may be repressed, denied, or not looked at, and that part that is not apperceived at the moment, that constitutes a fleeting day residue and gets into subsequent dreams. The Malamuds were really the first ones to repeat the experiment, not as it was done. The real experiment, which was the most interesting, involved the pictures being exposed for 1/100 of a second, so you couldn't see anything. At that time I was 45 years old, a boring period of my creative life, so I got very excited about the Poetzl experiment and I decided to repeat it, all on my own.

So I got a slide projector and a lens off of a camera, and I had a primitive tachistoscope about like the one Poetzl had used. It doesn't take complex apparatus and I began to repeat the experiment.

AR: You were doing this at Sinai?

CF: Yes. I had a little room off Ward A where I could work. I did a number of experiments and they came out pretty classy and I put them together and published them, and the *Journal of the American* was excited about them and they gave it the lead article of that issue and that article caused a big stir. This was called "Dreams and Perceptions," and I got hundreds of requests for reprints, had little notes from everybody, from Karl Menninger, complimenting me for doing this marvelous experiment. of course, it all had the prestige of—I introduced the article by quoting Freud, you can't go wrong if you quote Freud. I don't know if I quoted Rapaport, yes, I quoted him about other things, and I wrote a 50 page article and I included drawings of dreams and pictures, and when I presented it in 1954. I brought my tachistoscope and flashed some pictures on the

screen, which impressed everybody. I did my experiment right in front of them. This was at the New York Society. By that time I was sufficiently over my phobia for speaking that I was able to perform. That was the first time I had given a paper in the Society aside from the one back in 1944 which I gave in my uniform. This one I gave without my uniform. It always helps if you know you have a good thing and I knew that I had written an important paper, which would excite some interest, which it did.

AR: Who were the discussants, do you know?

CF: I think Kris was a discussant. He was very friendly. What did he say? I can't remember the discussion. He wanted to do something together with me on memory. Kris was interested in memory. I don't know what he had in mind. Anyway that was a great success and then in 1956 I published a second paper called "Dreams, Images, and Perception." By that time I had heard of the Foundation's Fund for Research in Psychiatry at Yale, they were giving out money, they had just started, so it was suggested to me that I apply. At that time it had a marvelous board. It had Sybil Escalona, the guy who became Dean of the medical school at Yale, Fritz Redlich, and many others in research who had influence. This was the beginning of a revolution in psychiatric research.

NIH had begun back in the 40s—it used to be when you were a principal investigator in a medical school on a project, you didn't get paid. The Foundation's Fund for Research in Psychiatry was willing to pay the senior researcher, who was also an analyst in private practice, a certain sum of money for giving up some of his time for research. They gave me \$7500 for 15 hours of work. It wasn't much money but a principle was established. Around the same time, the National Institute of Health was doing the same thing. I don't know who started it first.

AR: That's when they started the career investigator.

CF: It was a little later than that that they started the career investigator, but that changed the whole complexion of research. otherwise, if you were in private practice and wanted to do a project, they wouldn't pay you. If you had enough money and you got a big grant and you had some assistants, they'd pay the assistants. Anyway, that changed the climate of research in this country. They continued to give me grants from 1954 to 1959, so in 1956 I published a second paper on "Dreams, Images, and Perception." In 1957 I published another one on "Construction of Dreams and Images," and altogether from 1954 to 1960 I published about nine papers. They were always thick papers. I was always longwinded, and I had pictures and diagrams, about 50-page papers. Anyway, I need not go into the details of the work.

AR: Have you thought of having them collected?

CF: Yes, people have been asking me for years to collect them and publish a monograph or a book. The Foundation's Fund wanted to do it, the *Journal of the American* has a monograph series, Basic Books, I think. We had about three offers but I just never bothered to put them together.

AR: You got three offers?

CF: I just never did anything; I wasn't ready to do it yet. In a sense I was correct, I wasn't ready. I was always somehow dissatisfied with whatever I did, it had to be better. Not that I'm the most perfectionistic researcher in the world—I'm not. I do something even though I know it's defective or faulty. I'm perfectly willing to publish and I always feel if there are any mistakes in it, as long as the basic stuff is okay, someone else will correct them or I'll correct them. I didn't have to have perfection. I wasn't that way. Anyway, at that time I got dozens of requests for reprints and invitations to talk all over, and I understand many institutes were having seminars going over these papers of mine. For a period of 13 years, in the early 60s to the mid-70s, my secretary saved all the invitations I would get. I have a big folder, it has 80 invitations, from practically every

institute in the country and lots of other places, to lecture, write papers and monograph. It all created a big stir. It also got into my next phase when I got into the REM stuff.

AR: I don't know of any other psychoanalyst who created such a stir in quite that way.

CF: No, I don't think so. It lasted a long time, for a period of 12 or 13 years. I was getting six invitations a year that I had rejected. I'd just write that I didn't have time, and I'd select some. For a while I gave papers to half the institutes in the country and then I couldn't do any more; all these honors detract from your work. I was offered at least three professorships, which I turned down.

AR: Really? Where?

CF: Some good ones, I've forgotten. You know, once you make some kind of reputation there's a concerted effort to keep you from working any more, so what you have to do is to refuse honors. I was asked to run for President of the American three times. Like Caesar I rejected them.

AR: Is that a corollary of the Peter Principle? The principle that people get to a certain level and then they deteriorate. There's some administrative principle which says that people are pushed higher and higher to a point where their talents—it's a reflection of how inefficiently organizations are run in that regard.

CF: If that's the Peter Principle, I resisted the Peter Principle. The reason I've been able to work as long as I have and still be somewhat productive is that I refused these honors. First, I didn't want to be President of the American, as nice as that would have been. My narcissism is great enough, but it's like being president of a large labor union and you have to devote a lot of time, and I was not sufficiently interested in politics. I had no great ambitions to change the course of psychoanalysis in this world, nor did I notice that anybody else was doing it, and I just simply refused. I was offered to be the President of the Institute. I finally did accept to be the President of the Society. I did that because it didn't take any work. You'd go to meetings, you'd wear a tux, you didn't have to do any preparation, and you had a secretary who did what little work there was to do.

AR: You were the President when I became a member.

CF: You remember my being the President?

AR: You must have signed the certificate.

CF: I must have. Anyway, I enjoyed being the President. I was there from 1965 to 1967. There wasn't much to do. I always enjoyed going to the meetings. Also, you notice, particularly on the part of your German and European colleagues, who have a quite different attitude toward authority than Americans, suddenly you were treated as if—not even Heinz Hartmann, Kris, all of them—suddenly you're the President. It doesn't make any difference to the Americans. The Europeans with their authoritarian souls really treat you differently and you notice that. Anyway, I enjoyed being President for a couple of years. You didn't have to do anything. I put in my dues at the Institute for a number of years, although I did manage to preserve my practice in the afternoon and I did my research in the morning, and I managed to preserve my time so I could continue to do some work and also make a living, but I committed myself to be on a good many committees. There were periods when I was at meetings three days a week. I was on the Education

Committee for six years, at a nice time, in the 60s. Things were rather peaceful. What authority there was reigned supreme. It was not a time of friction. Hartmann was on, Loewenstein was on, Bak was Chairman, and he was a very good Chairman. He was a real politician, a manipulator. He was the one I said never took a majority vote lying down. But he was very good in those days,

he was liberal. When issues came up, he was on the liberal side, and everyone was friendly.

AR: That was before the Victor Rosen-Robie Bak confrontation.

CF: That was at the end of my time. Do you know that whole story?

AR: No. I would be interested in it.

CF: I won't take up this time. I know the whole story, I was inside it, and I'll tell it to you. But during those years Margaret Mahler was on the Education Committee, Edith Jacobson, Annie Reich, Rudy Loewenstein, and a few minor characters like me. I was intimidated. Nothing much happened, just normal business went on, and there were no crises at the time; it was a rather peaceful period. The Institute was at its peak of prestige and power in the 60s. Patients were still available. There was money coming in and it was a nice time. That was the Golden Age. Then I got honors. I got the first Menninger Award for the best research of the year. I gave the Brill Lecture in 1965 and the Freud Lecture in 1969. I got the Hartmann Award.

AR: Then you must have been the second Hartmann Award person?

CF: Actually, the Hartmann Award was supposed to be for young researchers. When I got it in 1957, or 1967, I was nearly 60 years old. It was supposed to be for young investigators. What happened is that all the older analysts got it. Greenacre got it when she was about 90, Bert Lewin got it, even Anna Freud.

AR: There were two awards, the junior and the senior.

CF: There wasn't at that time. That was a wise move to do that—they gave one to Anna Freud, to Greenacre, and Bert Lewin got one, and then about that time I represented the youth, about 1960 I got it. So in 1956 I was made a training analyst. Again, it was conferred on me. I made no application. I was just told one day. Annie Reich called me up and said, "You've been made a training analyst." I said, "That's fine," and the next thing I was told they wanted me to teach a course.

AR: By the way, you were probably one of the first Americans to be made a training analyst at the Institute. How many of them were there before you, who weren't from Europe? I think just Jack Arlow and Charlie Brenner.

CF: We were made training analysts around the same time. Arlow said I was made one before him. I think there may have been some others before me, I'm not sure. I'm quite sure there were. It's worth looking up. *The Archives of Psychiatry*, is it called? *The Journal of the American Medical Association*--is it the *Archives of Neurology and Psychiatry* or just the *Archives of Psychiatry*? They took a poll once of the most popular analysts or something. It started out with Hartmann and Erikson and went down a list, I wasn't quite at the bottom of it but I was the only one who was a full American, the only American on the list,, the rest were European.

Anyway, I'm getting off the track. I wanted to tell you something. In all this I never felt that I was an adequately trained analyst because I had not had the advantages of good courses or supervision. I was kind of self-made. I had my own analysts to imitate, as one tends to do. I didn't imitate in any kind of slavish way. I didn't take on my own analyst's research interests, which I readily could, since my primary preoccupation was with depression, and you understand what led me into my own interests were deeply involved with depression as I knew it, and actually altered states of consciousness in which people do crazy things, dreams, fugue states, where people commit murder, sleepwalking, primitive ideas of a little boy, not so primitive, but I understood somehow the power of the forces inside that could not be controlled and they could do crazy things like commit suicide. This formed the core of my basic passion for exploration, I think, and I

was always interested in depression and always thought that Freud's paper on "Mourning and Melancholia" was—one of his great, great masterpieces as well as a great literary masterpiece, and I read a lot of the literature on depression. It was not anything I wanted to explore except in myself. I haven't gotten through with my own career. I have had several careers.

I learned a lot from George Klein, Bob Holt, and Irving Paul, Wolitsky, and that whole group. I learned how to do an adequate experiment.