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Interviews with Dr. Charles Fisher, New York Psychoanalytic Institute and Society Oral History Project

CF: Some 20 years ago, Margaret Brenman sent out an announcement through the American that she was specifically interested in what analysts had learned from their own analyses about their motivations for becoming analysts. Not things like a way to make a living, interest in helping people, etc., but the more irrational aspects of their motivation. I'll try to deal with my own case later. First, in this respect I have to tell you a little bit about myself in chronological order. I'm the younger of two brothers. I'm the months younger than my brother. He is now dead. This means that I was a premie, a precipitate baby, born on a staircase. My father tells my birth story sort of like that of Moses in the bullrushes. My mother plays no role in it. My mother is always absent. Very early in my postnatal life, my father came to the hospital in Los Angeles where I was born, and there I was in a basket. This was before the days of incubators, and he said I was lying in the basket and I was dying, so he said, so he took me home and he nursed me, not my mother. My mother had a 101/2-month-old baby, my brother, to take care of.

AR: How did he know you were dying?

CF: I don't know. That was my father's diagnosis. He picked me up and he took me home and he prepared the bottle and he nursed me. And there was never anything said about my mother. So I grew up in Los Angeles.

AR: Had your family been in Los Angeles a long time?

CF: Not a long time. My parents were both immigrants. My father was born and raised in Roumania. He had a very hard childhood. His mother died when he was nine and he had a wicked stepmother. All I know about him was when he was nine he was apprenticed to a tinsmith, and in those days you were sent to a distant village to be apprenticed. He was sent away and he never saw his parents. He came to the United States. I don't know much about his life before that. It was about 1900.

AR: They went on foot across . . . ?

CF: Irving Howe told me how in Roumania large masses of Jews marched to the Hamburg ports to attempt to come to the United States, where they all thought they would become wealthy. It must have been about 1880-1900. He came around the turn of the century, maybe earlier, because he called before my brother and I were born. My mother came from Odessa and I never knew until much later just when.

AR: A mixed marriage.

CF: A mixed marriage. My mother came at 13. They met in night school and lived on the Lower East Side like everybody else. They obviously had a very unhappy marriage.

There was a set of fraternal twins born before my brother and I were born. My brother was born in 1907, I was born in 1908. The twins were born a few years before that and they both died early. Somewhere in that interval my father left my mother, whether he actually deserted her or whether it was to go to California to see if he could make a living and send for her, it was never clear. I know he was at the St. Louis Fair, the St. Louis Exposition, in 1904, and my father had a passion for expositions.

AR: He was certainly adventurous.

CF: Yes. He was a tinsmith. In those days they were like peddlers. He was not a traveling businessman, but he was a traveler. When he left Roumania, he stayed in Hamburg for a while. He was an uneducated man but he spoke about five or six languages. He spoke Roumanian, Yiddish, English, some Russian.

AR: Many Russians came originally from Germany..

CF: Yes. But he had a passion for world fairs because he could pick up prostitutes, at least that was my private opinion; I have some evidence for that. Anyway, he landed in San Francisco. He was in the San Francisco earthquake, which was in 1906. He woke up one morning and the city was shaking, plaster was falling off the walls, and everyone ran to the bank to get their money. That's the last I heard of the earthquake. The next thing I know he was in Los Angeles and he evidently sent for my mother. By this time the twins were dead. They died very young.

AR: Of what, do you know?

CF: I don't know what they died of. I have a picture of my mother holding one of these twins. It looked like it had marasmus. That's the only picture I have of my mother. So they settled in Los Angeles and my brother was born and less than 10 months later I was born. My father was a Jewish anti-Semite. His real name was Cohen. He had some Roumanian name. He got to Ellis Island so on his naturalization certificate he was Herman Cohen, but he had a lot of trouble with that name because wherever he went he would be the object of anti-Semitic comments, because of the name Cohen. My father did not look Jewish. A lot of Roumanians don't have Semitic names. He would get into fights and he once got into a fight with a man and hit him over the head with a wheel and almost killed him, so after that he decided to change his name. So he picked the name Fisher. Why did he pick the name Fisher? Because half the Fishers were Jews and the other half were not.

AR: There was a proper degree of ambiguity.

CF: Yes, that's right. On my birth certificate I was named Charles Cohen. Many years later I had it changed, but from the time I was born he was already Fisher. My father would not live in the Jewish section of Los Angeles, an area called Boyle Heights. We lived in a section of Los Angeles, lower-middle-class. Now it is close to the center of town but then it was kind of far west, and it was a Gentile neighborhood. We had an Irish family on one side and a family called Best on the other. My father was 40 when I was born, my mother 32. My father kind of retired early. He had social inhibitions. He was very shy and self-conscious and somewhat withdrawn. He bought a house and he was a skilled workman, and he built a shop in the back of the house. He made a fairly good living. In 1916 he bought a Model-T Ford, a Victrola "His Master's Voice." He bought us toys. You

notice I don't say much about my mother. As far as I know, she was always depressed, she may have had a postpartum depression, and I have very few memories of her. My mother committed suicide when I was eight by taking poison. In those days there weren't any barbiturates around but there were poisons, a stuff called bluing, bichloride of mercury.

They would bleach clothes with it and it came in little bottles with a skull and bones. It wasn't a liquid, it was pills which you dissolved. Now, she got up early one morning, locked herself in the bathroom--about four or five in the morning--and I think my brother heard her in there and heard

her vomiting and must have awakened my father, and by this time, my memory is that I was standing outside the door (describes the layout of the house) .

AR: You were in the bathroom and the kitchen?

CF: Here's the kitchen, the bathroom was here, and I was over here somewhere, standing against the wall very coolly watching the following scene. My father broke down the door and I got a glimpse of a glass half full with the blue liquid. Then I recovered the following memory in analysis, and I'm not sure if it was a memory or a fantasy. I see him carrying my mother out over his shoulder, and in the kitchen she starts to vomit and he drops her. She's lying on the floor in a pool of reddish vomit, and that scene ends.

I remember now my father calling the doctor, who told him to give her a raw egg, and the next thing I know an ambulance came and took her away. She lived for four days.

She died on April 1, 1916. A few things are relevant to my future history. I asked my father, whom I was very fond of, I admired him. I was eight. What made my mother do that.

He gave me a very strange answer. He said, "She did it because she had a bad dream." What a thing to tell a child!

AR: When did you ask him this?

CF: When I was eight. Within a day or two after that.

AR: Dreams are very powerful.

CF: I had no difficulty understanding that dreams were the royal road to suicide, the royal road to the unconscious. I had no trouble with that, but it was very puzzling. She had a bad dream. You understand, I spent my life with dreams, I dealt extensively with nightmares, and that little statement may have determined my career. That's associated with another scene. My brother and I and my father are sitting together and my father has a bottle of pills and he is calmly counting them, and he announced that my mother took enough to kill an army. 16 pills. So I learned to have a cool and detached attitude.

AR: An experimental attitude.

CF: Yes, that's right. But my father was not a scientist, he was a very emotional, passionate man. He was very unhappy with my mother, partly, I had reason to believe, because they had a very poor sex life. I have some memories--this was a little house with two bedrooms, and there was a wall separating my parents' room from my brother's and my' room. I don't know how many experiences you telescope into a single memory, as Anna Freud suggested about memory, but at least on one occasion I knew that my father made sexual advances to my mother and she was pushing him away. That may have happened many nights because the amount of amnesia I have of my mother is extensive.

AR: Was this retrospective amnesia, after the suicide?

CF: This is what Greenacre talks about in her paper on girls, latency girls, who had some traumatic experience which served as screen memories, repressing everything that went before, but I have as much memory of the first years of my life as most people. It's just that I have lots of memories of my father and only a few of my mother. I thought my mother didn't speak English and I don't remember her speaking. She was silent, she had a white face, sort of blank, depressed, and I didn't think she could speak English because I don't remember her speaking it. I found out later, when I was in my forties, that she came over when she was 13. That's still on a borderline where it's possible to learn to speak English well without too much of an accent, so

my mother may have spoken it very well. Furthermore, she may have been more educated than my father. My mother could write Yiddish. I think that was an unusual accomplishment for a woman at that time, wouldn't you say?

AR: Well, there were women who learned to write it.

CF: If she could write Yiddish at 13, she certainly could speak English. One of the reasons my mother was depressed was that they had no friends; she had family in New York, two brothers and a sister. She used to write to them and she used to write in Yiddish, but she had no Jewish connections in Los Angeles, and they were friendless because my father had reclusive tendencies and maybe my mother did, too. But all my memories of her are silence, she was a silent person. She hardly spoke, so I thought she didn't know how to speak English. That was a big distortion. She was so depressed she didn't speak.

AR: You don't remember her speaking to you in English?

CF: I have no memory of her speaking to me at all. The few memories I have of her were her washing my hair. My most pleasant memory I have of her has a sort of glow around it. She must have given me a bath. I remember she was carrying me out of the bathroom to bed and had put a towel around me, and that's the most intimate memory I have of her. Now when they'd go places--my father had a lust for living, he could never get satisfied. He would take us to vaudeville shows, take us to the beach in the summer. My mother never seemed to get involved in these things. My mother would take me shopping. Whenever the family-divided-up, I would go with my mother because I was the youngest; my brother would go with my father. My mother used to spit in her handkerchief and wipe my face, which I hated. We were very well taken care of as children. We were very well dressed when we were kids. My mother's suicide occurred when she took the poison. It was a terrible death. She died of uremic poisoning.

AR: Yes, it attacks the kidneys.

CF: My eighth birthday was on the 26th of March and she took the poison on the 28th or 29th, and she had made a birth-day party, she had made a cake, and one thing I could never understand or be able to forgive her for is how could she have done this so close to my birthday?

AR: It was her bad dream.

CF: Her depression was connected with childbirth. And I learned many years later, I was told by her sister that she hated my father, that my father had killed her and was responsible for her death. She was always pregnant, he was always knocking her up.

AR: She only had three pregnancies.

CF: That's true, but I remember she may have had a miscarriage of some kind when I was little, but they said she was pregnant all the time. My brother never enters into these considerations at all. I was so smart, and I had just skipped a grade, in February, I skipped a half year, then another half year. Then I caught up with my brother and so I was in the same grade as my brother, and my brother didn't like that. Teachers in those days didn't know anything about psychology and they were always making comparisons between him and me. In those days I was always the smartest one in the class. Anyway, to come back to what all this has to do with becoming a psychoanalyst--my father was my first patient. He would confide in me and I learned to listen to him. I never told my father my troubles. He began to tell me things

when I was five, six, seven years old about my mother and he said he was going to get me a new stepmother, even though at other times he'd tell me that stepmothers are terrible people. He would also tell me some sexual things.

AR: When you were six and seven?

CF: Yes. All this is between four and eight, so I learned how to listen. I was obviously his favorite. He wasn't a bad father. He only hit me once that I can remember. He slapped me when I once said the word fuck to him. But I never figured my father really meant it. I must have been about 13 when I told him that I masturbated and he pretended to show some disapproval of that. My father was a very sensual man, very frustrated. I lost my mother when I was eight and my poor father was dying to marry someone else.

He was by this time 48, and he was a very attractive, very handsome man, so he hired a housekeeper.

AR: Your mother was 40 when she died?

CF: Yes, 40. I was eight and my brother was nine. So my father tried to keep us and he hired a housekeeper, a very masochistic little woman who had a little six-year-old - girl. She only lasted a couple of months or so. Somehow the child got drunk on beer. There were some big scenes and she disappeared, and shortly after that my father found a foster home for us with a Mrs. Goldberg, and we stayed with her it must have been a year, because my mother died in April, 1916, and in July, 1917, my brother and I were put in an orphanage where I remained until I was almost 18. We lived in a Jewish orphan home in Southern California. All I've been telling you I've told at least three analysts and a lot of other people, an every time I tell it I get upset.

AR: What do you make of that?

CF: There are certain trauma you just never get over. I've had a number of abreactions, of an odd kind.

AR: It must have been an incredibly puzzling experience for you.

CF: I never thought of it in those terms. It was certainly baffling. I'll tell you about an abreaction, a particularly bad one, of some significance. My mother was in the hospital for four days before she died and we were never taken to see her. A uremic death is not a pleasant one. I recently saw one. I had become very close to Margaret Mahler and she died rather quickly over a week's time, although she seemed to be doing very well before that. They operated on her because they thought she had an obstruction, but she didn't.

She was an 88-year-old woman with cardiac and respiratory trouble. First she had cardiac failure, then respiratory failure, and then kidney failure, so what carried her away was an uremia, and I saw her and she already in an uremic coma, and they had put a white sheet over her and she was all blown up. You can get urinary retention with uremia.

And it was a very shocking experience because when my mother died, we were taken to the funeral home and somewhere, I was in the car with my brother, and the car stops and a man comes to us and says, "Would you like to see your mother?" and my brother said, "No," and I, being brave, said, "Yes." So I go in and all I see is my mother in an open coffin with a white face and a white gown, all blown up, and I was very calm and detached, so when I saw Mahler, this was the only other person I saw like that, it was a very shocking, very upsetting experience.

AR: Did your father cry? Do you remember him crying?

CF: I didn't see him cry. We used to wait for him on the front lawn of our house. We knew he had gone to the hospital. We'd wait for him to come home and give us a report. Finally, the fourth day came and we knew that she had died. So we went into the living room and he told us that she was dead and then he began to cry, and at that moment I threw myself on the floor and I started to cry and I checked myself. I said to myself, "What are you crying for? You didn't love her and she didn't love you," or something like that. The scene ends then. I remember my brother crying, not my father. Then years later, I was in California and my brother and I went to the cemetery to both my mother's and father's graves. We talked. We talked about the day when our mother committed suicide. He remembered things that I didn't and I remembered things that he didn't. One of the things that he remembered that I didn't was that our father cried, he wailed a long, long time. You could hear him a block away. I repressed that. It was a massive repression.

AR: So you stopped your own crying.

CF: I stopped my own crying . Years later, after I went into analysis with Edith Jacobson, not too long after I started with her, there was something about it and a lot of time was spent around the few days around my mother's suicide, and suddenly I began to wail. This wail was coming out of me. I was detached and listening to it, and it went on for the whole hour, I couldn't stop. It went on and on and on, and it was my father's wail. I did that in two sessions. And that was a very important therapeutic experience. I always ask patients where they feel their depression and some people say in the head, some say in the belly, but I had the distinct impression that mine was in my belly. I used to outline it.

It was over on this side, it was an introjected lump, and after that experience, the lump disappeared, it palpably shrank. My father made a number of attempts to get remarried, but after he put us in the orphan home, he told us that in a short time he was going to get a new mother for us, but he never did. From then on he was a broken man and I guess guilt-ridden, and he just went downhill. He continued to work at his trade. He would come every Sunday to the orphanage and visit. We were not allowed to go out or to go home either. There were three modern cottage-type orphanages in the United States (Jewish)-- the first was in Pleasantville, New York, the second in San Francisco, and the third one in Los Angeles, built maybe around 1912. I went there in 1917. It was beautiful. There were big stucco buildings, there were cottages, a ten-acre paradise, full of fruit orchards, orange groves, and there were cows and chickens, and the plant was beautiful and we were not treated badly, we were well taken care of. It was a Jewish charity. By the way, this was before modern social work. There were no social workers, the superintendents tended to be rabbis. We had an old man rabbi, he was about 65 and he knew nothing about children. His wife, who was some kind of bitch, ran the place, and they had ignorant, untrained female matrons. There were about 100 kids. It was a small institution and we had everything there but love, unless you could eke some out from somebody in some way.

AR: Were there counselors?

CF: There was a housemother, either widows or old maids, who had had nothing to do with children. Some of them were witches, some were very nice, none of them were lovable, except one, and I was already grown up by then.

AR: And you went to school from there?

CF: Yes, we went to public school. The public school adjoined the ten-acre plot so all the kids at school knew who the orphan kids were. One of the worst things about being in an orphan home was that you knew you were inferior and there was a stigma attached to it. And you know that they closed up the orphan homes. There were Jewish, Catholic, and Protestant homes, and the Jewish charities were always far in advance ideologically of the Catholic and Protestant ones, but it was finally around 1940 concluded that orphan homes were not as good as foster homes, a dubious proposition, because a good orphan home is better than the majority of foster homes because it has a certain stability and you have friends, other kids, and you make close relationships. There were things that weren't too bad about it, being in an orphan home, but it was bad enough. You spent your whole life waiting. You were supposed to leave when you were 16. There were no social workers. They kept me there until 1926, a year longer than most of the kids. I was the first kid there who went to college. When I came there, everyone agreed that I would be. ...

AR: The group decided that you were somehow special, from the very beginning?

CF: The authorities did. Everyone said I would be the first one to go to college. It was true, I was.

AR: You knew when you were nine.

CF: Yes. I would bring home report cards, all A's. So how I would go to college or what would become of me, that wasn't clear.

Charles Fisher Interview Part 2

AR: But then you were still in elementary school, not high school.

CF: I was in the fifth grade and I stayed there through high school. When the new institution was built, this beautiful place was not good enough for the rich Jews of Los Angeles. They wanted more of a showplace, they wanted something that didn't look or sound like an institution. It was a beautiful place out near Culver City. Do you know where that is? It's hills. You could see the ocean. It was called Vista Del Mar, which means "view of the sea." It was 20 miles away but on a clear day you could see the ocean. It had been laid out like a city street and it would curve around so you wouldn't know you were in an institution. There were beautiful stucco houses. Each house housed 20 kids, ten girls and ten boys, and they had house numbers. It was on a paved street so people wouldn't think they were in an institution. I was only there a year. They kept me until I entered college. I was still 16, so they kept me there a year and a few months until I was almost 18, by which time I was a sophomore.

AR: So you were in an institution, then, from nine to eighteen. And your brother-- how did he react to the idea that you would go to college and he wouldn't?

CF: That was traumatic to my brother. When my mother was alive, my brother and I did everything together, we played together, we got along nicely. But I had contempt for him because he was a bed wetter. When we got to the orphanage, I began to dislike him. I was ashamed of him. He wet the bed, and he didn't have much dignity about him, and then he was not one of my best friends. Harry Stack Sullivan said you had to have a chum not to be schizophrenic.

AR: I didn't know that. He had a close friend, this other psychiatrist.

CF: Sullivan was homosexual.

AR: But he had a male friend who was a mentor and friend.

CF: He was a strange man. There was an article by Leslie Farber in the London Times Supplement about Sullivan and how he didn't tell the truth about having had a schizophrenic episode, he was a psychopath, a liar.

AR: He was expelled from school for something terrible, from college.

CF: He may have been. But I knew him and I disliked him. He was interesting to listen to but there was more heat than light. He had a mordant humor and a peculiar, interesting, garbled way of talking. He gave you the illusion that he was saying something important. But he was a tremendous influence. He was a very disordered man, he kept most of his life secret. Farber said he was uneducated, no one knows where he went to medical school, and he was an ignorant person, but he had some curious charisma; not for me, though, I didn't care for him. Anyway, I'm losing the continuity. One of the things that led me into psychiatry was that I was a neurotic child and I knew that I was, because I knew that I had things wrong with me and I had no way of rationalizing and I knew that I had something that could be called pathology, whatever I called it. It was mostly what could be called social anxiety. I didn't have any habit disturbances. I had a poor appetite because I didn't like the food there. There were fruit orchards, melon patches, fruit trees, a real paradise, and I used to steal fruit, but I had social anxieties that were increased by the fact that I was in an orphan home. Even before I got to the orphan home, when I was six I discovered that I had public speaking anxieties of severe proportions. I'd go into a panic if I had to perform. It started when I was in first grade, the first time I remember having such a panic reaction. When you had to go to the toilet, you had to raise your hand and go up to the teacher and say, "Can I go to the toilet?" I remember I had to pee, so I observed that I was afraid on certain occasions; I sat in my seat trying to hold my urine. We had rows of benches in those days, not the individual desks and chairs they have now. You'd see a pool of urine on the floor and know that some kid had peed in his pants. So I sat there and tried to make up my mind, should I say, "May I go to the toilet?" or "Can I go to the toilet?" and I knew that whichever one I chose, I would stammer, so that the anxiety was that I would stammer. I didn't stammer much, but I was in a panic. Finally I would reach the point where I couldn't go on any longer and I would get up the courage and I'd ask the teacher. Whichever one I would use, I would stammer a little, not a lot, it was an apprehension that I would. However, any performance situation I was faced with, I would panic. I was bright enough that I was always getting into some situation that I would get called upon to do something. However, in class I discovered that if I volunteered, I didn't have any trouble, so I was always dangling my hand.

AR: Turning passive into active.

CF: That's right. So the teacher would call on me. If no one else knew the answer, I didn't have to be afraid that I was stupid or something, so I didn't have too much trouble. At any rate, I had a terrible speaking problem for many years. It finally got much better. I got into analysis with Edith Jacobson and I got so I could perform. It's very important in becoming known to be able to speak. In my old age, though, some of the anxiety has come back. I still have some trouble when I speak.

AR: You spoke at some significant occasions. Didn't you give the Freud Lecture?

CF: That was years ago. By that time I was very good. I gave the Brill Lecture and the Freud Lecture. I could do it very well.

AR: And with Sam Atkins you were wonderful.

CF: You missed the thing out in San Diego. That was two years ago and I did very well, so I can do it but there are still situations where I can't. Anyway, it was the bane of my existence and when I graduated high school--between the two institutions we lived right in Los Angeles; this paradise I was talking about was in a suburb of Los Angeles. In the intermediate institution we lived closer to the center of town. I went to one of the local high schools called Los Angeles Polytechnic High School. That's where I was in my second and third years; I finished high school in three and a half years.

AR: You must have been about 16 then, since you skipped a grade.

CF: When I finished high school I was still 16 by a couple of months, but I was a year ahead, most kids finished at 18. We had something that was called students' day at high school, where they had a student teach a class, so in every one of my classes I was asked to teach. I almost died, but I managed. And I was asked to be valedictorian, but I couldn't do that and I ran away. Most of the time I would face these things out--at tremendous cost. If I didn't do it, I was overwhelmed with anxiety; if I did it, it was the same. So amongst my motives for wanting to become a psychiatrist was self-cure. It wasn't as simple as that because I also had a profound curiosity about mental disease. I had a mother who was no doubt manic-depressive and a father who was schizoid, affectively so, and I was a little crazy myself. At the institution there was a kid with Tourettes Syndrome, an epileptic kid, a few psychopathic kids, and I was always fascinated by them. Anyway, where am I? I got lost.

AR: You're starting college, your first year in college, you still lived in the orphanage.

CF: I went to the University of California, Southern Branch. In those days there was just Berkeley and then there was Los Angeles and the University of California, Southern Branch, which was a state college, where the tuition was \$300 a year. When I got out of high school, I was accepted at the University of California, and in my first year they let me stay in the orphanage. They had no provision for taking care of you after you left. They had begun to get some kids jobs but I was still 17, and I was on my own. So I got a scholarship of \$150 a year and I got a job in the registrar's office. When I was very young, 13, I learned how to type because I was canny enough to know, because I was a canny child, that the only thing that differentiated me from the other kids or anybody else was that I had something of a brain so if I was not to become like my father--my father whom I admired when I was little gradually became a terrible disappointment. He lived a whole life of regret. Once when I was about the age of four he was in business with a man who had a tin business, a man named Brown. The person he went into business with became an extremely rich man, dealing in sheet metal, a contractor, and another irony was that he did the tin work for the new orphanage. My father didn't like this man, he said he was a crook and my father was an extremely honest person, whatever else he was. Where was I?

AR: You've gotten a scholarship and you're in college.

CF: Oh, yeah.

AR: Did your brother go to college?

CF: No, he left high school at 16. In high school he had taken up printing and he didn't do too badly with his life. He worked for a Los Angeles paper box company where they did very complex color printing, like for color printed chocolate boxes. It was a very skilled thing, so

he ultimately had a business of his own and did quite well. He got married and had a couple of kids plus some tragedies. He had two sons, one of whom died when he was about 18. The other one is a professor of economics at Rutgers with a reputation in the computer field.

AR: So you got a scholarship and then were able to leave and live on your own.

CF: In my second year in high school I enrolled in a course in shorthand, and I learned shorthand. I was in a class with 30 big, beautiful California girls, the only boy. That was an odd thing, a male stenographer. Billy Rose was a world Gregg champion, and President Wilson was, too, both role models. I had to make a living. I enrolled in college as a prelegal because I had the fantasy I would overcome my speaking difficulty and become another Clarence Darrow. I was 16.

AR: A modest ambition.

CF: A modest ambition. So I actually took a couple of legal courses and I just couldn't comprehend what a tort was. I couldn't tell a tort from a tart. And so I dropped that and I took a course in public speaking. I almost died. I was never called on. It was a great big class. Anyway, I went there for two and a half years and I did very well. I got good grades. I took general liberal arts courses. I was now getting to be 18 or so. After my sophomore year I took half of my junior year. At this point there was no intellectual direction.

AR: But a lot of practical skills, typing and shorthand.

CF: That's all. However, in my last year in the orphanage, in this cottage with 20 kids, there appeared a new housemother. Her name was Mrs. Riskin. She was about 40. She was a Russian intellectual, a 1905 socialist, altruistic, idealistic, somewhat puritanical. She was one of those. Her husband had died in the 1918 flu epidemic. She had a son my age, named Al. She had a big influence on me. She would give me things to read. I was going to college, but I was not educated in that way. So she gave me *The Nation* and I can't tell you what a thrill that *Nation* was.

AR: There was Ingersoll.

CF: No, it wasn't Ingersoll. Anyway, she opened up the world. Then she gave me novels by Gorky, it was Gorky.

AR: Was she educating you or raising your political consciousness?

CF: She was just educating me. She was a lonely woman, she had lost her husband. Her son, whom she loved very much, was away and when all the other kids would go to bed, we'd stay up and talk, and gradually she began to kiss me, and I was fairly sophisticated in a way, but I was aware that she was kissing me too passionately. Anyway, it was an interesting thing. She was a sad woman.

AR: You were special.

CF: Yeah. She gave me one of Schnitzler's novels, which was about a woman who has a son who is 17 years old and is away at school. He brings home with him during a vacation period his boyfriend, who is also 17, and this woman has an affair with the son's friend. I don't think she had any idea that it had anything to do with us. Anyway, when I left the orphanage, her son had come back, and he and I got a little apartment together. In those days you could get an apartment for \$20 a month. This was 1926. I didn't particularly like her son.

AR: Soyowereincollegeand...

CF: I was still in the orphanage. That last year I was in the orphan home. There was another housemother then, a 20-year-old Russian girl, absolutely stunning. She had a boyfriend who would come and visit her. He was a young man of about 24, a very attractive man, and they were in love. His name was Sigmund and he said he was a Viennese. During World War I, he witnessed his father being shot to death in a pogrom. He and his mother and his sister escaped. He was about 18 when he left Europe and came to this country, and he tried to get work. This was during the Depression. Anyway, he drifted across the country to Los Angeles, and I think he was a shoe salesman. He was very intelligent and a great reader and he used to give me books to read, Ben Hecht, Dreiser. He married this Russian girl but later they got divorced. Anyway, Al Riskin decided to go back to Berkeley, and I went to live with Sigmund, who was six and a half years older and not exactly innocent, but a very worldly man, a great womanizer. We were very close and it wasn't exactly good for me in many ways. I'm coming to how I met Betty. His girl, Jessica, lived in a certain section of town and she and her mother had a little dry goods store, notions store, and on that same block Betty lived with her family. Betty comes from a family of nine children, two sets of twins, of which she was one. She knew Jessica. She had met Sigmund and a couple of times he took her out, and one day Sigmund said to me, "I met a girl you might like." I was desperate for a girl. Anyway, Betty and I met and we fell in love immediately. Betty was a couple of years older. She was tiny, an extremely pretty girl. She weighed 92 and I weighed 106. By that time I was in a very bad state. I was in a major depression. For the first time I thought of leaving school. I was headed for a lot of trouble. Anyway, Betty wanted to leave home. I thought she wanted to run away from me because I was pressing her too hard. We arranged..... to meet in New York. She had some friends in New York. She wanted to run away to New York, so I helped her run away in the spring of 1927. When June arrived and school was out, I put a pack on my back, and I just left the city without seeing anybody or saying goodbye. At that time I wasn't seeing my father or my brother. I had no ties holding me there. My father had become a recluse. He worked a little. He lived in the rooming house section. There's a square there called Central Park, which is the equivalent of Union Square. I came back in 1930. I had not been in touch with my father. In 1930 I saw my father for the last time. The last thing he said to me was, "Your mother was no good. I was too passionate." In 1932 or 1934 he died. They found him sitting on a bench in Central Park; he had had a massive coronary. He was 64.

I'm back to June, 1927. I bummed across the country. It is a hard thing to do. There were certain parts of the country where I couldn't get a ride but I had read books that romanticized bumming; there was nothing romantic about it; was dangerous. In a period of 16 days I had a sensation that I was running all the way to get to Betty.

AR: And, of course, your father had run from Roumania to Hamburg and he also ran across the United States the other way.

CF: So I ran back to New York. Betty was living in New York and not getting along with her friends. By the time I got to Chicago I had spent my \$30. I didn't beg on the streets. I bummed a meal in a restaurant. I was in jail for two days.

AR: Really? For vagrancy?

CF: No, I was riding the rails. That was a dangerous.

Arnold Richards's Interview with Charles Fisher Part III: From LA to Chicago to DC to NYC

. . . thing to do. There were cops, bulls, and they would shoot at you. I was pulled off the train twice, once in Cheyenne and another time in Grand Island, Nebraska. A big guy stuck a gun in my ribs. What they do is put you in a jail, which is filthy. They kept you overnight and gave you a bowl of mush and said leave town and don't come back again. By the time I got to Chicago I wired Betty and she wired me \$10 and with that I got to New York and we began to live together, and we have been living together ever since for 60 years.

AR: In 1925--you met in 1925?

CF: We met in 1926.

AR: Sixty years.

CF: So Betty really saved me, not just my life, my sanity. She was a very maternal woman, along with her other assets. So what we did then, I still hadn't decided what to do with my life. I knew the only thing to do was to go to school. So I looked up my relatives. They were still alive. The brother of my mother and her sister. I went to see her sister and husband, and they fell on my neck. They wanted me to come to stay with them. There was another uncle named Isadore. He was rich. He was a building contractor. I was living with Betty in the Village. We weren't married. We were very advanced for that time. We lived together for three years. It was in the early Depression years.

AR: There is one area from our discussion last time that I have a personal curiosity about. You told us that you spent these years in an orphanage which was essentially a Jewish institution, and I was wondering about that aspect of your development—your Jewish education, Jewish Identity.

CF: Yes, there were some difficulties about that. My Jewish identity was marred long before I got to this institution. My father was an anti-Semitic Jew. I told you that he was a sheet metal worker. He came here around the turn of the century, and most of the places where he worked he worked with Gentiles. The name he was given by Ellis Island was Herman Cohen, and Cohen got him into a lot of trouble.. He used to get in fights because of anti-Semitic taunts and once almost killed a man. He therefore decided to get his named changed, and he changed it to Fisher because half the Fishers were Jewish and half the Fishers were non-Jewish, and this assuaged his conscience, and he changed the Herman to Henry. On my birth certificate I was named Charles Cohen; the name was changed. My brother, unfortunately, was called Isadore and he had a lot of trouble because Izzy, Ikie, and Abie were names everyone knew were Jewish. It's very ironic that Isadore is a Sephardic-Spanish name, but I have not known any Jew in a long time who named his child Isadore. So, we never went to synagogue, we didn't celebrate the holidays, we never lit candles. And my father got so he could eat ham, but he couldn't get himself to eat pork. His conscience couldn't let him do that. So I had no religious upbringing until I was eight years old. Before my eighth year, although we always were aware that we were Jews and my father was a very Jew-conscious person. What my mother was I have no idea. I told you that my mother could write Yiddish, and I remember her writing to relatives in New York, and I was very impressed that she could write Yiddish, which I don't think my father could. So there was no religious upbringing until we got in the orphan home. The

orphan home was Jewish. The superintendent of the orphan home was a rabbi in his sixties, named Sigmund Frey. He knew nothing about children. His wife was the superintendent. There was on these beautiful grounds a building that had a little synagogue in it, and so we had Friday services and we had Saturday services and we were taught to read Hebrew and I got so I could read Hebrew very well, but we were not taught what the hell it meant.

AR: You just learned it to pray.

CF: Yes. Later on, when I was about 15, when I was at the temporary institution between leaving the old one and going to a new institution, I used to be asked to conduct services, so I was something of a substitute rabbi. I think I told you that the orphan home decided to build a fancier place. I told you about that and why we moved to this more orphanlike tenement closer to the center of Los Angeles. We went to the B'nai Brith Synagogue, which was one of the principal Jewish synagogues in Los Angeles. There was a terrible rabbi there, named Rabbi Magnin, who became rather famous in Los Angeles because he built a beautiful synagogue on Wilshire Boulevard, and I guess all the rich Jews in Los Angeles were in it. Anyway, when he was a young man and I was still an orphan attending this synagogue, when I was still at the orphan home, I was about 15, he approached me once and asked me if I wanted to be a rabbi. He would see to it that I was sent to Hebrew Union College.

AR: This was in Cincinnati?

CF: In Cincinnati. I hated this guy. I couldn't stand him. I knew he was a hypocrite. So instead of being flattered by his asking me that, I was angry and I told him very clearly that I didn't want to be a rabbi, which was stupid. I could have been sent to Hebrew Union College. They have a first-rate education, I understand, and I didn't have to be a rabbi if I didn't want to. But I had no one to guide me in these matters, so I refused him, in fact, was insulted, because being a rabbi to me--I was just a little kid--meant being like this son of a bitch, and I didn't want to be him. So I was confirmed--in those days reform Jews had a confirmation.

AR: At 13?

CF: At 15. Boys and girls together. There was no Bar Mitzvah. All those were post- World War II developments, when reform Jews went back to Bar Mitzvahs. There is no such thing as confirmation any more. So I remember it as--there was practically no preparation, very little Jewish education. I don't even remember that we had any classes. I think we went to services there, but neither in the orphan home or later on in the synagogue were we given any Jewish education of any kind. But I always recognized myself as a Jew and was rather proud of it and I never in my life experienced any personal anti-Semitism. Occasionally my brother did because of his name, but very little.

AR: Maybe we can get into this theme later on. Maybe we should return to where we left off last time, which I think was some time in college.

CF: We left off when I got to New York.

AR: You had returned from Wyoming.

CF: I bummed across the country, and Betty and I began to live together. So we were only in New York no more than about six weeks. I got in contact with my relatives, my Aunt Fanny and her husband, Max, and Rubin and Moe, my cousins, and they got on my neck and wanted to take care of me and come and live with them and they'd send me to school. And again I turned them down. I hated what they were. In those days--this was in the mid-twenties--I

wasn't a political radical. There were no radical movements at that time, the Communist Party wasn't around yet.

AR: What about the anarchists?

CF: There was the IWW, International Workers of the World, they were beginning; there was the Socialist Party of Eugene Debs, that was around, that didn't have very much power. The intellectuals were against the system--people like H.L. Mencken, Upton Sinclair, Sinclair Lewis, Sherwood Anderson, Ben Hecht, Lincoln Steffens, all those early writers. So that was the kind of intellectual atmosphere. You were against the capitalist system, which at that time was presumably at the height of its prosperity, although there were weaknesses, for sure. I was in New York in 1927 and I couldn't get a job, so I had contempt for these relatives of mine. It was also based on other things. They told me some things about my parents and they hated my father.

AR: Were they your mother's relatives?

CF: They were my mother's relatives. My father had a brother who died many years before. My mother had two brothers and a sister. There was my Uncle Isadore, but I think he was dead by that time. He had become very wealthy, according to some standards. He lived in Flatbush. So my poor relatives didn't know what to do with me and they knew I was living with this little whore in Greenwich Village and were very shocked, and I didn't know what to do with myself. I couldn't get a job. Betty had a job but she'd lost it and we were practically out of money. Anyway, my aunt and uncle decided to send me back to California where I came from. So they said they'd get me a ticket, so they got me a ticket and Betty and I decided we would stop in Chicago, where she was born and where she had one brother and one sister who still lived there, and also I got the idea from I don't know where that I'd like to go to the University of Chicago, but I don't

know how I knew anything about it. So you must understand that in those days one could afford to be daring because you didn't have any alternatives.

AR: This was around 1925, 1926?

CF: This was already 1927. I was 19 and Betty was a couple of years older, and we were just on our own. We had broken off with our families.

AR: Did they give you two tickets or just one?

CF: I think they just gave us one because they weren't interested in her. 'I think she must have had a little money left and she had some friends. All I know is we got the train and got off in Chicago and turned the ticket in. We had \$75, and we started off life with \$75. So since I had decided I wanted to go to the University of Chicago, I got a job. First we found a room near the university on East 61st Street, it's across the Midway.

AR: Yes. 61st and where?

CF: Woodlawn.

AR: Where Burton Judson is now--the dormitory of the University of Chicago, it's right there. Yes, 61st, because the el is on 63rd. Was the el on 63rd then?

CF: Yes. So I forget how but I got a job as a stenographer for a little company called the Stahmer Coal Company. I was Mr. Stahmer's secretary.

AR: Ah, your stenographic background.

CF: That's right. I got \$30 a week or something, which was a lot of money. So I worked there for a month and I was going mad because I thought if I have to do this for the rest of my life, I'd die. I was certain I was made for better things. Betty got a job and I decided I'd go to the university and I would get a part-time job. So I quit Mr. Stahmer and I sent away for my transcript and there was no trouble getting into the University of Chicago in those days. Besides, I had a very good record from the University of California. No one seemed to have any trouble getting in, but the University of Chicago was a very select place and the undergraduate school was always very small. It must have been even smaller then. Anyway, my story is that I was admitted to the University of Chicago and graduated without ever having seen the Dean or an advisor or anybody. I don't know if that was so unusual, maybe it still happens these days. So whatever I did, I did it on my own, I never had any advice, but unfortunately I sort of knew what I wanted to do. But first I had to get a job, so I went to the employment office and I was about to reach--I was standing in line and there was this fat man in front of me and I heard him tell the girl that he was looking for a male stenographer, so I thought this was a gift from heaven, so I tugged at his coat and I said, "I'm a male stenographer," so he looked me over and he said, "Come to my home tomorrow at 2:00." So I went. He didn't ask me any questions, and he turned out to be Samuel Harper. Do you know who Samuel Harper was?

AR: I know who William Rainey Harper was.

CF: Samuel Harper was his son. He lived in a big mansion right off Woodlawn. He was an old bachelor who lived with his mother. William Rainey had two sons. One was psychotic and was in an institution. Samuel was a big, fat, unattractive man without much grace or charm, who was professor of Russian language and history. He became that because--the story used to go around campus--old William didn't know what the hell to do with him so he placed him a field where there was no competition and besides it wouldn't be a bad idea to have a Russian department. So he hired me for 50 cents an hour and I worked for him for about four years. Besides what Betty made, I brought in a little money myself.

AR: So those were the four years you had in Chicago . in college?

CF: No, I entered there as a junior. I had two and a half years at the University of California but I lost a half

year when I transferred; I don't know why. I had a liberal arts course, political science, economics, etc. I took Spanish, which was a terrible mistake and which made trouble for me later on. Everyone took Spanish there.

AR: Where, in California?

CF: Yes. So I signed up in the psychology department. The University of Chicago at that time was a great, free instituion. William Rainey had hijacked many of the better Eastern universities for famous professors because of a heavy endowment from the first John D. Rockefeller. There were splendid professors in every department.

AR: This was before Hutchins arrived?

CF: I graduated in the summer of 1929. Hutchins was there then. I was in his second graduation class. You could take pretty much what you wanted, there were often no special prerequisites. I was only taking things I liked, a course in the French Revolution, a course in paleontology and geography.

I entered as an undergraduate in the summer of 1927. By the spring of 1928 I was taking premedical courses and in two years I finished all the four years of chemistry--general chemistry, qualitative and quantitative analysis, organic--which were very hard for me. At the University of Chicago you were up against students who were superior to the ones at the University of California. I didn't do too well. I got B's and C's. By the time I got through and graduated I had completed a minimum of premedical courses. Three months after I graduated, the crash came. I think I may have started to apply to medical school then. But it was already difficult to get in. In the first place, my grades weren't good enough and I had a lot of courses in social sciences and psychology, which tended to be held against you. So I couldn't get in. I don't know how many medical schools I applied to. What happened was that very shortly after the Depression kids couldn't get work. Medical tuition was \$200-\$300 a year. If one could live at home and pay the relatively modest tuition, it was possible to go to medical school. As a consequence, the medical and all the professional schools were flooded with applicants, making it difficult to gain admission. All the schools had Jewish quotas, just like Czarist Russia. But I just happened to be in the one medical school on the planet Earth where they didn't have a Jewish quota.

AR: You were a psychology major as an undergraduate?

CF: Yes, I was a psychology major. At the time the University of Chicago, that had once had a magnificent psychology department--John Dewey and John Watson taught there and for some period Lashley. I was there when the department had deteriorated, in a period when there was Harvey Carr and Bills--they were second-raters. I took a lot of courses but I was not excited about them, except for several courses in abnormal psychology. I read a lot of Janet. There were no inspiring psychology teachers. But I read things by Morton Prince. I was always interested in dissociative states and hypnosis. I don't remember that I particularly read Freud at that time. I remember there was a guy in the psychology department, a student, who sat in the library all day long and everybody said, "He's reading Freud from beginning to end," and everyone made fun of him, and I had a sort of contempt for him, too. And my developing ambition was that I wanted to be a psychiatrist and that included being an analyst, but I don't know if the passion for analysis started then--how early that started.

AR: When you say analyst--a Freudian analyst?

CF: Yes, a Freudian analyst. There weren't any other kind around.

AR: But you weren't reading Freud?

CF: I don't know how much Freud I read. I don't think very much--excerpts.

AR: What made you interested in dissociative states? Do you know where that came from?

CF: Yes. it came from dreams, from my father's statement that my mother committed suicide because she had a bad dream and she must have done something in a dissociated state.

AR: Ah, I see. So the dissociative state was the waking part of....

CF: The waking part of something unconscious. I struggled with the idea of an unconscious mechanism, some compulsion, propulsion....

AR: Some action that is out of control.

CF: That's the way I explained it. It hadn't been proved. So I very early read Morton Prince on multiple personality. Those things were what interested me.

I was taking all the science courses and it was very difficult. But I persisted with this crazy-notion to be an analyst, that some miracle would happen and I would be able to get into medical school. By the end of my first graduate year I was at my rope's end. However, after I left Los Angeles periodically I wrote to the superintendent of the orphanage where I was raised, whose name was Joseph Bonaparte, like Napoleon's brother. He was a cut above the average superintendent. He had a Master's degree in social work. That was unusual. Things were picking up in the Jewish charities. So he was more educated--a rather cold man but nice, and he took a kind of interest in me. I left the orphanage without any financial support. I got a job as a secretary to the registrar of the university in Los Angeles and I was able to support myself. I would write to Bonaparte periodically, and I wrote to him that I really wanted to go to medical school and he wrote me a letter that suggested he thought I was kind of presuming above my position, but he said he would try to help me. He got some money for me that would support me through a year of graduate school in psychology. I don't know how much money I was making or Betty was making, but I took a year of graduate work in psychology. I started a dissertation. The dissertation was interesting. There was a friend of mine named Daniel Flanagan, a charming Irishman, who did a rather clever experiment that was far ahead of his time. It had to do with perceptual defenses. He may have been the first one to really do this. He would take paired nonsense syllables, one of which was neutral and the other like "FUK" or "COK," taboo words. Flanagan was able to demonstrate an inhibition in perception of the taboo words. That's the sort of thing the leading psychologists were doing in the 50s. Erikson, Lazarus, Bruner. They got hung up on the idea that in order to be able to not see something, you had to see it first. This led to the idea of a humunculus that scanned the unconscious and recognized the dirty words. I was very impressed with Flanagan; I understood the significance of the unconscious aspects of his experiment toward -which I had no resistance. Poor Daniel Flanagan. He was thrown out of school because he had to take a French examination to get his degree and they caught him cheating. I took-a French examination, which I'll tell you about in a little while. I tried to devise something equivalent to Flanagan's experiment using pictures, but I never got a decent thing going. All I remember is a dermatology-textbook with a lot of pictures of disgusting diseases, syphilis of the skin, of the nose, leprosy, but I never really got the thing set up. My advisor was a man named Bills. At any rate, that quarter ended and I was disgusted. I was sick of the field. I didn't want to go on. This takes me to the summer of 1931. I had one year in which I just took courses. During that time I had a lot of jobs. I was a night watchman in a garage. I worked as a night x-ray technician. It was in Pullman, Illinois. Do you remember the famous Pullman porter strike? Betty and I had a little room above a veterinarian whose place was full of barking dogs who kept us awake all night. It was filthy. I worked as a waiter in a fraternity house and did all kinds of shit. So that was the summer of 1931. I was desperately depressed. I didn't know what I was going to do. I didn't want to go on in psychology, so I thought I would make one more attempt to go to medical school. The University of Chicago was an extraordinary place. They had extraordinary rules, some of which were probably illegal. They would let you take your first year of medicine, and if you did well, they'd accept you. I don't know of anyone else but me who really accomplished this.

AR: They would take anyone?

CF: I don't know if they would take anyone but they took me. However, they would predate your admission but they'd only predate it to the time you took your language exam. Most medical schools required French or German, and of course I took Spanish. I didn't know any French or any German. So one day--this was the most important decision in my life--I found out that the next French exam was ten days off so I said to myself, maybe I can learn enough French in ten days to pass the exam. I reasoned thusly. I had had a Romance language, Spanish. The exam was a translating exam. They gave you a page and you could bring a dictionary, so I got myself a French grammar, and I went through it, and I got myself a French dictionary, and the dictionary also had the principal parts of the verb forms. I tried to read some scientific French. It didn't go too badly. Most of the technical words were the same as English. All I had to do was learn the little words in between. So I reasoned, if I had to learn the little words in between, why read scientific French, why not read literary French? So I got out a couple of volumes of de Mau- passant, short stories. I was always quite well-read. So I sat down and I began to read, De Maupassant's stories were short and interesting, sexy, and easy to read, with a nice pure French, so I tried it and I could see that I could read it and make sense. So what I did for the test is I sat and read de Maupassant for ten days. At the end of the ten days I could read de Maupassant. And this was my preparation for taking the exam. So when the day came, I appeared, with 15 others, and we were handed a sheet with five paragraphs on it. You had to translate four of them. And I said to myself, if you don't quite understand, at least write a clear English sentence, but I did understand, I did very well. I translated all five paragraphs. I had to wait for about three weeks and after three weeks they posted the results on the bulletin board and there were three names of those who passed and I was one of them.

AR: Three out of 15?

CF: Three out of 15. You must understand, all these other people had at least two or three years of French, and they couldn't translate, evidently.

AR: You were good at decoding.

CF: I have no gift for languages, no ear, I can't talk, but I'm very good at decoding.

So now I said, all you have to do is go to medical school. I signed up for gross anatomy.

For the first time since Betty and I were together, I was able to devote full time to my studies.

She was working for a man she didn't know was a bootlegger, connected with Al Capone. He had a mineral water sign in the window as a cover. But he was smuggling scotch. Betty worked as a secretary. She was making about \$50 a week, which was a big salary in 1931.

AR: On the South Side was the Al Capone headquarters. 63rd Street was the center of his gang.

CF: I was taking laboratory-courses, especially anatomy. I didn't have time to work; for the first time I was a full time student. The Dean of the medical school was a man named Butch Harvey, an Englishman. He taught the anatomy course. In the summer of 1931 it was ghastly hot. I was given the cadaver of an 80-year-old woman. The student next to me had a huge, muscular Negro with burn marks on his wrists and ankles. He had been electrocuted. Electrocuted convicts ended up in a medical school anatomy lab, often men with great physiques, prized in anatomy classes. My 80-year-old woman was not so prized. I had some terrible corpses.

However, I was very good at things like anatomy, and so I worked hard, and once a week we had a recitation, a lecture, and Butch Harvey, the Dean, was there, so I plunked myself down

right in front of him. This was a terribly hot summer; nobody else was very much interested in anatomy at the time, so I sat in front of him and he would ask questions and I would raise my hand. I wanted to call his attention to me so he would remember me, and so I did that, I made myself very much in evidence. At the end of the year--I liked neuro-anatomy--and I got an A in it, H, Honors, and an H in gross anatomy.

AR: Do you remember the neuro-anatomy textbook?

CF: It was Ranson.

AR: Right, that's the same one.

CF: That book was very important to me because it was Ranson for whom I went to work (I later got a fellowship at Ranson's Institute of Neurology at Northwestern).

AR: I knew there was a reason I asked the question.

CF: I was impressed with the textbook and I was impressed with certain experiments that Ranson had done on peripheral nerves, cranial nerves, and I found that I was full of admiration for certain things on esthetic grounds because they were done beautifully, which was the beginning of my interest in research.

AR: That textbook became Ranson and Clark.

CF: That's right. Clark was a graduate student when I was there.

I got through the first trimester; the second trimester I signed up for head and neck, for histology, and something else. The head and neck course is the important thing. I was a whiz at the cranium and the cranial nerves. However, at the time the professor of anatomy and the one who was actually dissecting was a man who was named Professor Swift, a tall man, about 6' 8". So I ingratiated myself with him, and he saw that I was very good and one day it came up--I probably insidiously brought it up--that I wasn't in medical school, and he said, "How come you're not in medical school?" So I said very resentfully, "They won't let me in." He was indignant. He said, "How come?" I said, "I don't know," and he said, "I'll see about that. I'll go to see the Dean." So I guess he did go to see the Dean. It was nearly the end of the trimester and I got another H, some more H's, and he had gone to see the Dean and I was admitted into the medical school and I was predated back to that French exam. So that's how I got into medical school. It was a chance in a million, the combination of being in the right place at the right time, being lucky and daring and scared.

AR: This was Billings?

CR: No, the way it was then, you could take your first two years on the campus and then you could take your second two years at Billings or at Rush, downtown, so the end of that year when I was firmly in medical school and I could see that if things went well I could come back....

AR: So you went to medical school then with some background and interest in psychology and neuro-anatomy.

CF: That's right, and in psychiatry, psychopathology. I was interested in that.

AR: Were you the only Jew in your medical school?

CF: No, there were lots of Jews. I didn't really have a class. I don't remember any Jews in that particular class. I don't remember knowing anybody, I can't remember a soul. I had other friends who were Jews and other non-Jews. In fact, when I first got to the university I met some nice people. There was a daily paper that was called The Maroon.

AR: The Chicago Maroon?

CF: The Chicago Maroon.

AR: It still exists.

CF: The first year I was there I wrote a book review and there was a famous book called *Companionate Marriage* by Judge Lindsey, who was recommending doing what Betty and I were doing.

AR: So you were still not married at that point?

CF: No, not yet. We didn't get married until 1930. So I wrote a review, a favorable review, for *The Maroon*. Through that review I met a number of campus radicals, interesting people, and so I began to make some friends that way. That was 1927-1928. This was the summer of 1932. I had run out of money. I had heard that Ranson had a fellowship so I went up to see him and he said, "Oh, you're just the person I was looking for," with a year of medicine, some training in psychology. I never liked Ranson. He was a sort of cold fish, a Yankee, he had a peptic ulcer, he was cold and ambitious. Anyway, Ranson is the man who had been working on peripheral nerves, and he was the one who discovered the nerves that carry pain. So he got the idea that he wanted to try to correlate the portions of unmyelinated nerve fibers with the sensory spots of the skin; there were different sorts of spots (pain, touch, temperature, etc.), so he wondered if there was a correlation; so what he wanted me for was to map out these spots on the skin. I died at the idea of doing that because it was tedious, an uncertain thing to do.

AR: Was it the staining of the myelin?

CF: No, first you had to examine people and map out these spots, and he would make-- say you found certain areas were innervated by the radial nerve--you would get the branch of that radial nerve and you could count the number of unmyelinated and myelinated fibers to see if the proportions correlated with the number of spots. It was crazy, impossible, so I dreaded doing it, but I looked up the literature and I saw that the crazy Germans, systematic, obsessional Germans, had already done this. They had gone all over the body for touch, warmth, cold, pain, and everything, but the literature was all in German and I didn't know any German. So I thought, "German ain't French," but maybe I could pull the trick again. I'd seen Ranson early in the summer and since he had hired me I was supposed to go to work in the fall and start his crazy experiment, so instead I spent the summer with a German dictionary. I didn't know any German at all. I was reading these articles by men named Strughold and Frey, crazy guys who had done all this, and so I painfully translated these half-dozen articles and they were not bad translations. I had to learn to teach myself some German so I had my own ways of doing that. I read literary works. I got so I could read Thomas Mann's short stories. His early stories were sentimental. I read other writings by Schnitzler, Reigen, for instance.

AR: Schnitzler? Schnitzler always shows up in psychoanalysis, doesn't he?

CF: Yeah. I read the *Christian Science Herald* because it was English on one side and German on the other, and that was what I was doing, learning to translate. So when fall came I brought all these translations and he, Ranson, was so delighted he said I wouldn't have to do the work myself. But that first year I didn't do much of anything.

Now there was a very fine group of researchers, the principal one being Magoun. And then there was a guy named Ingram. They were all full-time researchers. This was in the days when a research person--I was earning \$600 a year. There was some research money around for principal investigators. Ranson by this time had a research institute of his own and quite a

reputation, so I had done a little research in Chicago in anatomy. One semester when I had a little free time I went to Dr. Bartelmez and said, "Do you have some work I can do?" So he put me to work and I did a little experiment and wrote it up. Anyway, Ranson put me to doing something which I was not interested in.

AR: That was your first paper, then?

CF: No, my first paper was written in 1933. I came to Ranson in 1932. In 1933 I wrote this paper on the sympathetic cells in the spinal ganglia. Around that time, the end of 1931, beginning of 1932, what had happened was that Ranson had picked up a so-called Horsley-Clark machine. It's an instrument you put over a head--a monkey's head, not a human one. You can put electrodes into any part of the brain so you can explore the interior of the brain. The Horsley-Clark machine was invented by Sir Victor Horsley, an Englishman, in 1908, and Clark, who was an engineer. It looked like a cap. It has a metal frame which has movable electrodes that you can move in any direction and you had to do the preliminary work to work out a series of coordinates to guide you in placing the lesion. The thing wasn't used because it was too hard to do the preliminary work, but two important pieces of work were done in English, one by Wilson on the basal ganglia. But the thing wasn't used. Three of these apparatuses landed in the United States. One was at Yale, one was at St. Louis, and someplace else, but it wasn't used at all. Ranson picked the one up that was at Washington University in St. Louis, brought it to Northwestern, and he and a young man I worked with named Ingram spent a couple of years perfecting the instrument, and just when I got there, it could be used, and it was a gold mine. Imagine. You could stimulate and make a lesion any place in the interior of the brain you wanted. So I had never been lucky much at anything. I was always lucky in my career, by accident. So one day Ingram came to me and he handed me a big black box of slides and said, "Go through these and see what you can make of them." They happened to be a series of slides, stained lesions in the hypothalamus; they peppered it with lesions, both laterally and in the midline. The only behavioral or other observations they made were they collected the urine of these animals and they found that a certain number of them were pissing their heads off and they found they had diabetes insipidus, so I went through the slides. I was never very good looking through a microscope, I could never see anything, but if I was looking for something I could see it. Anyway, I put together the lesions that cause diabetes insipidus. It is a rare disease, and the whole thing was important because I worked out the pathophysiology of the disease and the connections between the posterior Pituitary gland and the hypothalamus.

AR: The neurohormonal relationships?

CF: That's right. The relationship between the pituitary and the supraoptical-hypophyseal tract, and I did a lot of work from 1932 to 1939.

AR: You had finished medical school at this point?

CF: No, I just had one year. My struggle to become a doctor never ceased.

AR: You couldn't continue medical school? You went to work....

CF: I couldn't, but I did. For two years I stayed out of medical school and during the second I got involved in the hypothalamic work. By 1933, I was making \$1500 a year. You could live on that. Anyway, in 1934 I got my Ph.D. Magoun and I got our Ph.D.s together.

AR: Magoun? He hadn't got a Ph.D. before?

CF: He was a young man. Ranson had an eye for talent.

AR: So in a way you were the first of the M.D.- Ph.D. people.

CF: I was one of the few. Then my troubles began with Ranson. We really had a gold mine. He never let anybody work for their M.D., so when I got my Ph.D. in 1934, I told him I wanted to go back to medical school and could I work part-time? He said absolutely not. However, I always had something hot going, so he let me work there part-time and I could go to medical school. I was taking part-time medical courses and doing research, so from then on I worked for him a year part-time and I went to medical school part-time, from 1934 to 1939.

AR: That was another five years.

CF: Altogether it took me eight years to get my M.D. and my Ph.D. It was well worth the time.

AR: So during this time your interest in dissociative states and psychopathology was on the back burner, so to speak.

CF: It was on the back burner. With all that I was doing, I did a lot of work that became very well known, but by 1937 Ranson got some money and asked me to publish a monograph on diabetes insipidus. One of the important areas that the work on diabetes insipidus led to was the demonstration of the hypothalamic-hypophyseal control of pregnancy, labor, and sexual behavior. The latter processes were controlled by the same areas of the hypothalamus as was water metabolism. We observed that our cats and guinea pigs with anterior lesions in the hypothalamus did not mate or become pregnant, while those that were already pregnant had very prolonged

and difficult labors and some of them died in convulsions. We had found that the hypothalamic lesions producing diabetes insipidus resulted in atrophy of the posterior lobe of the pituitary gland and demonstrated that the latter had a total deficiency of pitocin. Pitocin was known to be a powerful uterine contracting substance and its deficiency probably had something to do with the difficulty our lesioned animals had in delivering their young.

The same anterior hypothalamic lesions destroyed neural pathways having to do with mating behavior and neurohormonal pathways to the anterior pituitary having to do with ovulation. We had serendipitously localized the areas in the hypothalamus that controlled sexual behavior, and it was not until a decade later that the accuracy of our localization was confirmed. Without knowing it, the effects that we produced undoubtedly involved the destruction of the hypothalamic- pituitary areas having to do with the so-called releasing hormones.

By that time I was earning \$2000 a year and all I had to do was write this monograph, but I continued to do other work. Ranson protested every year when I wanted to go back to medical school, but every year he let me go back. Not that he had any special fondness for me--I never became really friendly with him.

AR: Can you think of anyone else, just offhand, who was well known?

CF: I finally wrote this monograph--they were all working on different things.

I had about 20 papers and this monograph, which became something of a classic. So I left an extremely promising career in neurophysiology. Sometimes I regret it.

AR: Yes, as Freud did, as Charlie Brenner did.

CF: But it was a very exciting period. Those were the Depression years. During the worst part of the Depression, from 1931 to 1939, I was able to get my degree, Ph.D., stayed married, and some other things. Betty worked. She stopped working in 1936. We had a little boy who

lived to five and a half, who had an accident and was killed. It was a terrible, terrible business, but it is dubious whether I would have been able to accomplish all this if it wasn't for Betty. From 1936 on I was self-supporting. Betty didn't work then. We got married in 1930. I graduated from medical school in 1939. In 1936 we had our little boy and I had to get an internship, so I applied for an internship with the United States government, in St. Elizabeth's Hospital.

AR: The Public Health Service?

CF: Yes. So we moved to Washington. It was a terrible internship, no teaching, no supervision, psychotic patients. My whole education was somewhat sporadic, in and out of medical schools. The State Boards which I took at Hopkins, I had a 90% average, which wasn't bad. Anyway, it was a miserable internship. In 1940 I became a student in the Washington-Baltimore Psychoanalytic Institute and I was in analysis with Edith Weigert. At the time, Leslie Farber, who was also at St. Elizabeth's, and I became very good friends. Now at that time the Washington-Baltimore Society was still ostensibly Freudian.

AR: Sullivanian?

CF: This was before Sullivan. Sullivan was not on the scene. There was Frieda Fromm-Reichmann. She was rebellious in some way against classical Freud, although she was not enough of a theoretician to know what to do and always attached herself to some man who influenced her, for example, Groddeck, Fromm, and finally to Sullivan.

AR: What about a psychiatric residency?

CF: It was a two-year internship, it counted as a residency.

AR: In psychiatry?

CF: I didn't have a real residency. In 1942 the war came and I got into the Public Health Service and was stationed on Ellis Island for three and a half years, where they had a neuropsychiatric service, and that took the place of a good residency.,

AR: So you were just starting the Washington-Baltimore Psychoanalytic Institute, you and Leslie Farber? Did you both start?

CF: Yes. He started his analysis a little before me. He started in January, 1940. Edith was a classical analyst. She was very nice to me. Leslie and I were both going to her. There was some rivalry.

AR: Sibling rivalry.

CF: I did my first piece of research with Leslie. He had begun to do some hypnosis, and he taught me how to hypnotize, and we decided to do an experiment together in giving people suggestions to dream, and then we added another thing. We would give dreams to some subjects

that had been dreamed by other subjects and ask them to interpret them, which some of them were successful in doing. We published a paper on this work in The Psychoanalytic Quarterly in 1943.

AR: Was that your first analytic publication?

CF: Yes, it was my first analytic publication. It was entitled "Suggested Dreams under Hypnosis." It was superior compared with other things in the literature, such as a paper by Schroetter in 1912. Schroetter was awfully crude. For instance, he gave a suggestion to a female subject to dream about a homosexual relationship. She dreamed of carrying a bag on which was written, "For women only." We didn't do anything that crude. Leslie was a very good

hypnotist and I became a very good hypnotist, too. For example, we would say to a subject, "I am going to remind you of something that once happened to you. You were walking along the street and saw a woman sitting on a stoop nursing a baby." She was then told, "You're going to have a dream. When you wake up you will tell me about it." The subject reported, "I was taking a streetcar up to Mt. Pleasant and I went into a store and bought a lot of apples, fruit, and candy." We asked the girl to draw the mountain. She drew a breast, but she was not aware that she was doing it. The experiment was very successful. We published it and it's part of the classical literature on the subject.

AR: So it's interesting in a way that your career recapitulates Freud's professional development, neuro-anatomy, hypnosis, and dreams. Do you remember much about your training at the Washington-Baltimore Institute? Who were your teachers and supervisors?

CF: I'll tell you about my non-training at Washington-Baltimore Institute. About this time my son was killed in an accident. He fell off a cliff in Rock Creek Park in Washington and fractured his skull. That day my analyst got ill and was away for eight months. About a year later, after the war started, I was drafted into the Public Health Service.

AR: On Ellis Island?

CF: Yes. They gave me a choice of location. I chose the Marine Hospital on Ellis Island so I could be in New York. It was a marvelous service. In Washington there was very poor training, poor supervision. In 1939 the psychoanalytic field was quite well developed, but it wasn't too difficult to be admitted to an institute. I talked to Fromm-Reichmann and she said I had to go into treatment right away. My first supervision was with Frieda. I also had supervision with William Silverberg, the homosexual, at Flower Fifth. I shared an office at that time with Leslie Farber. We were in private practice and both got jobs at Chestnut Lodge as psychotherapists. It was an ideal time to start our careers and was very exciting. You know Chestnut Lodge, it's a famous place (I Never Promised You a Rose Garden). Frieda Fromm-Reichmann was a national figure. Did you ever see her? She was a little Germanic woman and overly sweet in manner and voice. She got famous at a 1940 meeting at the Richmond annual meeting of the American when she gave a paper called "Transference in Schizophrenia," in which she demonstrated that some kind of transference goes on. It was a very touching, moving paper which everyone applauded when she told how she would go and sit on a cold, damp cement floor with a mute schizophrenic for eight hours until the patient talked. At Chestnut Lodge they had real acutely ill schizophrenics. It was a very noble enterprise to attempt to treat them with some kind of modified psychoanalysis.

AR: Who was it, Rosen? What was that fellow's name?

CF: That's a different story.

AR: Not Victor, the other Rosen.

CF: I knew him very well. I've known a lot of the people of historical significance since 1939.

AR: Good, that's good for the purposes of this project.

CF: The other Rosen is, I'll think of his name. It was John. Well, Farber and I were sort of Frieda's favorites at Chestnut Lodge. When you came there, they assigned you a couple of patients. I had two patients, a paranoid doctor and an extremely disoriented and hallucinating schizophrenic woman of about 36, named Cunningham. You were told to go and take care of her but they didn't tell you how to take care of her. Frieda would just give you one

instruction--just go in and listen--so I don't think, anyone there, including Frieda, knew very much about schizophrenia, and no one had any theoretical framework.

AR: Burnham?

CF: No, there was me, Leslie Farber, the Cohens, and there was Stanton.

AR: Of Stanton and Schwartz?

CF: Yes, of Stanton and Schwartz. He was the most original.

AR: Was Bob Morris there?

CF: Yes, Bob Morris was there. He was a very minor talent.

AR: Bob Morris came from Topeka. He trained at Topeka. He trained with Leo Stone at Topeka. I wasn't there then but I knew of him. But I applied to the Washington Institute and I was interviewed by a committee, and the committee included Edith Weigert, Bob Morris, Dan Jaffe, and Howard Searles. Searles gave me a terrible time, but Edith sort of stood up for me. She was very nice, and Searles was horrid. Jaffe and Morris didn't say much of anything.

CF: Edith was nice. She did me some good. She favored me over Farber. AR: Which helped.

CF: And she recognized that he was a son of a bitch.

AR: You were telling me about Frieda.

CF: I liked Frieda to begin with. I liked anyone who seemed to like me. I had two patients at Chestnut Lodge. I had a schizophrenic woman who was not in contact at all, and I would take her for a walk every day. They did a lot of things like that. They were experimental. Their whole effort was admirable. They had the sickest schizophrenics you could imagine. Periodically I'd be able to establish contact with the woman.

AR: Did Frieda learn this from Simmel? Was she part of that--Simmel's hospital? CF: Yes, she had been at Simmel's hospital. I forget the name of it.

AR: In Berlin?

CF: Yes. She had been influenced by Simmel. I was having a hard time with this schizophrenic woman, but she seemed to be getting somewhat better. In the end I thought I'd - helped her a lot, but I forget what I did. Frieda said to me one day, "Your patient has a real Oedipus complex," and I said, "Doesn't everyone?" And she said, "No. Not everyone, just some people!" By this time I was sufficiently psychoanalytic that it wasn't pleasant to hear her judgments. Also, Farber and I had gone into private practice. We were seeing patients for \$5.00 an hour. That (Frieda's comment about the Oedipus complex) made me raise my eyebrows. What really turned me off of her was that we'd have conferences and Frieda would say things that supposedly I had reported to her during supervision that didn't go on, things that I didn't say. She would say things she wanted to hear. So I became suspicious of her. I always said what I thought was the truth, tell the truth no matter what. That supervision was a disappointment. Chestnut Lodge was the most disturbed place. Patients who couldn't commit suicide in any other way would take a needle and stick it in their chest. They did have an arrangement to try to split the transference which was introduced by Stanton.

AR: Having a treating doctor and an administrative doctor?

CF: Yes. There was this paranoid doctor, he was a killer. I was in the library with him one day and there was no guard around. He accused me of not letting him see his wife. He threatened me with a cigarette, poked at my eyes with it, blew smoke in my face, yelling at me. Then he said to lie down, so I lay down on the floor. He talked all this mumbo-jumbo. I was shaking so

much and he was scattering ashes on me. He put a vase of flowers on my chest, going through some burial ritual. That's all I remember. I don't remember how it ended. It scared me to death. There were only two patients I tried to treat. All in all, I was there for a year, from September, 1941, to September, 1942. Then the war came and we were all drafted. I saw patients during that year, trying out my skills, such as they were.

AR: So you had good clinical exposure but not much in the way of good clinical teaching or training.

CF: No clinical teaching. There were courses. Very poor training program. In Baltimore there was Lewis Hill. He was smart and quite orthodox. I just remember going to Baltimore once in all that time. Harry Stack Sullivan came the last year I was there. He had been at St. Elizabeth's before. He had a curious history. There is a marvelous article by Farber that appeared in the London Times Literary Supplement.

AR: I read his biography. You know that was published not too long ago?

CF: He was crazy. He had a weird charisma. He didn't have it for me. I was never taken in by him. He had a crazy way of talking--convoluted sentences, more heat than light. But he had a tremendous influence on psychiatry.

AR: Even our friend Merton Gill has recently become enamored of Sullivan. He sees himself as an interpersonalist. Sullivan cornered the market on that.

AR: When we left off last, we left you at Ellis Island in 1942.

CF: I came there in December, 1942, and I was very happy that I got there. First I didn't want to be in the army, and then I was turned down by the navy. I was accepted by the army but then I heard the Public Health Service was looking for people. So I rushed there, and they grabbed me because they were very impressed with my background and my research, and they gave me my choice where I wanted to go, so I said I'd like to go to New York, so they sent me to Ellis Island--that's how I got there. Anyway, it was a very interesting place. I was there for three and a half years. I spent the whole war years there, from 1942 to March or April of 1946.

AR: Do you remember your rank?

CF: Yes, I went in as a P.A. Surgeon and I left a P.A. Surgeon, with two stripes. They never promoted anyone. We were Reserve. We weren't Regular. Anyway, the thing that was interesting about it was they had a wide variety of cases, but mostly they had some genuine combat neuroses, war neuroses, in the merchant seamen and later on amongst the Coast Guard. The merchant seamen were--did you ever treat a merchant seaman?

AR: Certainly. Do you know the Greek merchant seaman's disease? It was abdominal pain masquerading as appendicitis . . .

Part IV: Charles Fisher Interviewed by Arnold Richards

AR: Certainly. Do you know the Greek merchant seaman's disease? It was abdominal pain masquerading as appendicitis . . . and you had a normal abdomen. It was good for the merchant seaman because they got off the ship. The Greek ships were terrible.

CF: We didn't see anything like that. What we saw was, beginning in 1942, that's when Lend Lease was on with the Soviet Union, and these guys were the unsung heroes of World War II. They went on the Murmansk run. Did you deal with anyone on that? You were after the war. You didn't see these guys. These merchant seamen were only happy when they were in a boat and, hey were miserable on land, and so it was easy to keep them on these ships, which were

living coffins. The Murmansk run would start out with a convoy of 60 ships, and about eight would arrive at Murmansk. They'd be bombed from the air, they'd be torpedoed, they'd be bombed when they got into port. They went through all the most terrible forms of death you can imagine; they would be sunk at sea and they would be boiled to death in engine rooms and jump into seas of fire. Some of them would get into lifeboats and they would float around for days, and most of them were never recovered. They would die of thirst and hunger. It was miserable. Anyway, I saw a number of very good war neuroses. I wrote a paper called "Hypnosis and the Treatment of War and Other Neuroses," in War Medicine. You remember there was a journal called War Medicine? And in it I reported a case of hysterical blindness that I treated, a case of hysterical paralysis that I treated, a couple of fugue cases.

AR: All with hypnosis?

CF: All with hypnosis.

AR: They weren't using Amytol in those days?

CF: We didn't use Amytol at all. We used hypnosis. I used it because I was a very successful hypnotist during the war, but not after. So this was published in War Medicine. Then, during the wartime is when you see amnesias of all sorts; you don't see them in civilian life, but the motivation to develop a fugue-state is extremely great during wartime. You see complex fugue-states, like fugue with loss of personal identity, fugue with change of personal identity, fugue with retrograde amnesia. This was my classification. So I saw and reported on more fugue cases than anyone in history.

AR: Since Charcot?

CF: Charcot never reported on fugue-states. Janet talked about them and Schilder wrote a paper on fugue-states while he was at Bellevue. At any rate, Rapaport was interested in them, and the people at Menninger's--Geleerd, Hacker, and Rapaport wrote a very nice paper on them. It was around the time I got in touch with Rapaport.

AR: He had already come from Osuotomy to Topeka.

CF: No, he had already gotten to--when did they get to Austen Riggs? AR: That was much later.

CF: Much later.

AR: Early 50s.

CF: I forgot when I got in touch with Rapaport. It was later. Geleerd I knew. She came to Ellis Island once. She was engaged to Rudy Loewenstein. Geleerd was a very beautiful woman back in those days. She kind of changed afterward, but she came to Ellis Island, and I showed her one of my fugue-states and I was very taken with her. She married Rudy and she had a very disastrous end, as you know. I know lots of gossip about all this stuff but I won't--I could interest you in the gossip but that's not what this is for. I tend to wander too much anyway. So I wrote a paper on fugue-states.

The first thing I did when I got here was I went back into analysis with Carl Harold, whom no one has ever heard of, and I was with him a year, and he got tuberculosis. I was a person who had had significant deaths in his family, my first two analysts got sick on me. Just as an aside, a friend of mine brought a friend of hers over to the house the other night. He was a Pole. He was about 40 years old, a musician, a composer, and he lives in Washington, and somehow he made friends with a group of analysts there who were interested in creativity, and were

interested in how he composed. Amongst the young, not so young, analysts who were interested in him was Wolf Weigert. Wolf Weigert was the 45-year-old son of Edith Weigert. I remember he was either a baby, yes, he was a baby when I was there. I never saw him but I think I would hear him cry at times; it was a curious experience, 45 years later, to be reminded. I was in analysis with Carl Harold. I liked him very much. He helped me a good deal, but then he got TB and quit, which may be one of the reasons I waited--it took me up to 1947 before I got into analysis again with Edith Jacobson.

I don't know how it came about but I got invited to give my paper on fugues to the New York Psychoanalytic, when I was not a member. I don't know who engineered it or how. It was before 82nd Street, it was an interim period. The original Institute was on 86th Street on the West Side. They used to meet in one of the smaller rooms at the Academy of medicine. I appeared there one night in uniform. It was very good to be in uniform in those days in the presence of people who weren't. By that time I'd already had dealings with Gregory Zilboorg. I can't even remember how it came about. I got to know Kubie, who was interested in hypnosis. I got to know Zilboorg, who also took an interest in me. They both took an interest in me.

AR: Kubie wasn't in the service?

CF: No, Kubie never got into the service. He, of all people, felt guilty about it.

AR: So he stayed in New York and practiced during the war?

CF: That's right. He practiced, and he offered to supervise me, free of charge. I said okay, and I used to go to see him once a week for a while. It was terrible supervision.

AR: Was this in his office?

CF: It was in his office at 7 East 81st or 82nd. He lived in a marvelous little house there, and Kubie was a very kind of aristocratic man, at least pretentious. He studied in England for a while, and he came back with a kind of British accent, and he was an elegant man. I don't think he really knew how to analyze, frankly. Anyway, he'd drag himself up in the morning, come out in his bathrobe. I was sent two patients by Sam Atkins for analysis. one of these I supervised with Kubie.

AR: How had you met Sam?

CF: I met him through his wife, Edith. I knew Edith in Chicago. I've known Edith for over 50 years. I knew her and her first husband, and I got to know Sam when I got here; it's a long story. The first patient was a young Italian boy who had the delusion that people were looking at the right side of his nose, and he was convinced that there was an inequality in the two sides of his nose.

AR: Would you believe that I'm treating a man with a very similar symptom? He had had a nose job.

CF: This one didn't have a nose job.

AR: He spends his time--he thinks there's a line right here and he looks in the mirror and he's convinced that's the reason women don't like him, because of his nose.

CF: This kid was about 21.

AR: My patient was about 23

CF: An infantile kid with polymorphous perverse inclinations. He was a

voyeur, exhibitionist. He used to look through the telephone book and call up girls, and he would talk to them in a girl's falsetto voice, and he was extremely subtle, clever. He would get them to talk about their sexual experiences. They would think they were talking to another girl, and then he might get them to masturbate, and he had these wild experiences over the telephone, but it shows the possibilities if one is ingenious enough. I think he only once arranged to meet anybody.

For some reason my first impression of Edith Jacobson was not so good, but I used to go to the Institute meetings when I wasn't a member and I heard Edith a few times, and I finally became obsessed with the idea that she was the person for me because she had a marvelous, warm voice. Did you know her?

AR: Sure.

CF: And I knew that her special field of interest was depression, and I was getting very anxious and depressed for one reason or another, and I finally decided I had to go back into treatment. I went to see Edith, and she really didn't have time to take me. Then she suggested I go to see Kronold. I saw Kronold. It was the best interview I ever had with anybody. I thought he was the most perceptive person, and I liked him. I thought he was really sharp--not that Edith wasn't. So other things being equal, I would have gone to him, but other things weren't equal. I was determined to go to Edith because I thought, for whatever reason, she was the person I needed. I hounded her a little, and she finally took me. My analysis lasted from 1947 to, I guess, 1955, about eight years--which I consider short.

AR: That was short for Edith, too, wasn't it? Didn't she tend to see people for a long period of time?

CF: Well, she never hesitated to let you come back, and I have such a practice myself, never being very confident that I've finished with anyone and never thinking that anyone really does get finished. I am as of now seeing my star patient--I've seen him many times. He's in his 40th year, with long gaps. He's been back about two years. I've had a number of patients I've followed for many years, which is fascinating because things happen which you could never predict. There are improvements that go on, there are changes that go on which you would never have predicted. I've followed the patient from 24 to 64, and he's in trouble now, but you follow people through various phases of what Erikson called the life cycle, and there are many crisis periods. I have a half-dozen people who come back to me. It's not that anyone really exploits the transference, but they come back when they're in trouble. I saw Edith over a period of 40 years actually, whenever I needed her. It should be done more often. We exaggerate our results. In the first place, we don't measure them. We don't know how good or bad they are. Granted, it's an impossible task. But that's another story.

Up to this point in my history I'm almost 40 years old. I'd not had either any supervision or any course work that I considered had great validity.

AR: Of course, you'd read a lot.

CF: Yes, I'd read a lot and I'd listened to patients a lot by that time. So, I told you I was with Edith about a year, and she sort of urged me to get into the Institute, a process which she fostered, and I said, well maybe I can do a little supervision, and in 1947 or 1948, our standards weren't too high, but I always thought that I was sick enough that under rigid standards, I wouldn't be admitted to an institute. But I was admitted to Washington without trouble, and after

a half- dozen meetings with Annie Reich, I was labeled a mature analyst, which I didn't feel I was, and I was admitted.

AR: As a member of the Society?

CF: Yes, and now this gets us to the end of the war, which was 1946. I went into practice. In those days it was easy to build up a purely analytic practice, starting at \$10 or \$15 an hour, but in the spring of 1946 I got on the Mount Sinai staff. For eight years I was in full-time practice aside from spending six hours a week at Mount Sinai.

AR: Moe Kaufman was the head?

CF: Yes. That was a wonderful experience because Moe collected around him a marvelous bunch of men, all of us just out of the service. Vic Rosen was there, Paul Brower, Lou Linn, a lot of them had been in the army. You know that wars make psychiatry, but wars especially make psychoanalysis. The First World War helped psychoanalysis. By the time of the Second World War, the United States psychoanalytic movement was already well developed, and this was before the Europeans came over, and then a great thing happened. Bill Menninger was made Surgeon-General of the Army, and the first psychiatrist ever to be made a general. In World War I no one got above a colonel, so he had tremendous power, and he appointed a lot of very good analysts to the more important psychiatric jobs in the army. Moe Kaufman was in the Pacific, and a bunch of young analysts, men who had just started in analysis, like Vic Rosen, Lou Linn, all kinds of other people, a lot of people who became professors of psychiatry.

AR: Like John Romano.

CF: John Romano, a couple dozen of them. Maurice Levine, Milt Rosenbaum, all the guys who became heads of departments of the principal medical schools in the United States. You were just a kiddie at the time, but you don't know what it was like after the war for psychoanalysts. We were real heroes. Why did we become such heroes? The reason for it is analysts, psychiatrists in general, but especially analysts, played a big role during the war, and since they showed more understanding and sympathy for war neurotics and managed to get them medical discharges more readily than other doctors, our reputation spread. Also, during wartime millions of people and their families became aware that the perfectly normal American boy next door can break down in what was called shell shock in the First War and combat fatigue was another euphemism in the Second World War, so that by war's end, we were real heroes, not only amongst the population but amongst the medical profession. The attitude toward us at Mount Sinai was simply astounding. We were looked upon with considerable awe. It didn't take many years before everyone become disillusioned with us. It didn't last. Anyway, Moe established the first inpatient psychiatric ward in a general hospital. I think Massachusetts General had started one earlier, maybe about the same time, but Moe was one of the pioneers in inpatient care in a general hospital, which then gradually spread all over the United States and in every city the principal hospitals gradually had inpatient services. It took the load off the state hospitals and also kept many people from ending in the state hospitals. And then things like liaison services, psychiatrists on every service. We were supposed to perform miracles.

AR: George Engel I guess must have been-there. CF: George Engel, Mirsky.

AR: Yes, the cure of ulcerative colitis.

CF: Mount Sinai was the place. We had this ward with mostly psychosomatic patients, because Mount Sinai collected a lot of psychosomatic cases, especially a lot of ulcerative colitis cases. The delusion was then present that you could treat psychosomatic diseases with psychoanalysis--asthmatics, hypertensics, etc. First the field was dominated by Alexander's psychosomatic profile; Alexander really started it, and there was a definite kind of profile for each disease, e.g., the ulcer profile; that was a man denying his dependency reactions.

AR: Was Flanders Dunbar at Sinai?

CF: No, she wasn't. She may have been at Sinai for a while but not in my time, but she was very important, she wrote a big book, and things were very exciting. We were going to cure these things. Well, gradually we began to notice that the ulcerative colitis patients would die like flies, and no one seemed to be getting better, and then Sidney Margolin--do you know that history?

AR: No.

CF: Do you know who he was? He was an analyst, Kubie's favorite man, and very important at Mount Sinai and a kind of flawed genius, very conceptualizing mind. It was just that he was grandiose. It wasn't quite clear if he

was psychopathic or delusional. Ordinarily when you do an experiment, you do the experiment and you collect your data and you go through it and you come to conclusions, the Baconian method. Well, Sidney Margolin did it differently. He formulated some ideas and then he tailored the data to fit the conclusions.

AR: Was he a member of the New York Society?

CF: Yes, he was a member of the New York Society. And he thought that he was going to inherit Moe Kaufman's position, although Moe had many years left--I shouldn't bother you with this story. It's just the history of psychosomatic medicine that's important. He had a case like Beaumont's--a woman with a stomach fistula, a black woman, and he set up a famous experiment, a joint venture between the physiology department, the GI department, and Margolin would analyze the woman and the GI experts would take the secretions, measure acidity, motility, and vascularity of her stomach mucosa, and they would correlate the changes with the psychological changes. Now this was a chance to do a marvelous experiment. Do you remember? This was at the height of Sidney Margolin's fame, so after some time he gave a paper at the New York Institute, a wonderful paper. I forget the title.

AR: What was the paper?

CF: It was on this case. It was a very impressive paper, in which he claimed that there were certain correlations that could be made 100 percent of the time--he used the word 100 percent--between what was going on psychologically and what was going on in the stomach. There might be certain dissociations but you could always predict what those would be, that is, the vascularity or the acidity or the motility would change. At any rate, Sidney was a great man for theory-making, he was very impressive in the formulations he made, very articulate. He said that, one way or another, when drive was active you would get, let's say, more motility, more vascularity, less acidity or something, but when defensive processes were more in evidence, you would get the opposite or some other pattern, and it was a very impressive thing, and he said you could make these predictions 100 percent of the time, and Kubie discussed the paper, called him a new Beaumont, and he was hailed as a conquering hero.

AR: He put analysis on the physiological map, so to speak.

CF: That's right. The article was published in the Quarterly and something unusual, Kubie's discussion was added to it. Discussions are never given with the paper--the one which hailed him as a new Beaumont. So some time passed and the experiment was continued, and I was very suspicious of this, knowing that to get something as complex as this 100 percent of the time, that doesn't happen. After all, if science demands that you get something at the .05 level significance, it permits you about five percent error in the so-called null hypothesis, but with Sidney it was 100 percent of the time. So I knew that he was faking and I knew enough about him from my own judgment of him that I said to him one day, "Sidney, did you really get those changes 100 percent of the time?" and he said, "Absolutely." I had something to do with Sidney Margolin's downfall. He was at the peak. He was considered throughout the country one of the leading psychosomatic theoreticians, and this brought him a lot of notoriety. So one day-- in those days Moe Kaufman was very accessible--we could always go in and bullshit with him--one day I went in and we were talking about this experiment, and I said, "Moe, is it really true that you got these correlations 100 percent of the time?" He said, "Who said that?" and I said, "You know, this paper that Sidney Margolin gave, he said he got these correlations 100 percent of the time." He turned white, he ran to the bookcase and pulled out the volume that had the article and he looked through it, and I showed him where he said these things. He said, "Well, nothing of this sort ever happened." What used to happen was that Margolin and Moe and others who were working on the psychological aspects of things, making predictions, would meet with the physiologists who were giving their reports on physiology, so Moe said, "Nothing of this sort ever happened, we could hardly ever make a correlation." I said, "Well, didn't you ever read the paper or hear it?" Now, Moe was a kind of a son-of-a-bitch in many ways, he never read the fuckin' article. Furthermore, it turned out that the physiologists were very pissed off at Margolin because he gave this article on his own, and he was the only author. He may have mentioned the physiologists but obviously they never read the paper or they wouldn't have let it go through. Moe was in an uproar about it. Within a couple of weeks Margolin was let go.

AR: Really? He was fired from Sinai?

CF: I don't know if he was exactly fired, but he began to disappear and look for another job elsewhere, and he would tell me he had been asked to come to many, many places. He ended up in Denver, the University of Colorado, and nothing ever happened to him any more. He got an extensive psychophysiological laboratory with all kinds of apparatus. He never published any more on this stuff. He did some anthropological work studying aggression in a tribe of Indians out in that area, and since then he was just a ruined man.

AR: Is he still alive?

CF: Yeah, he's still alive, he's a man about my age, very impressive, a great big man, football player, and nothing ever happened to him. After that they got another case at Mount Sinai. Aaron Stein did the study. I don't know what happened to that but they didn't come to the same results, and some apology was written for the Margolin paper. From then on (it may have been retracted from the Quarterly in the second paper, I don't remember whether the second paper was published), but around that time, Sidney Margolin had also developed a new theory of treatment, a regressive treatment.

These were the kinds of ideas he'd get into his head. He'd regress people

back to a stage, an oral stage, before the presumed conflicts--who the hell knew what the conflicts were?--but before the presumed conflicts that caused the ulcerative colitis or whatever.

AR: People did that in Topeka when I was a resident. Ethel Baum, who then went to Denver. I remember we would take these sick people and feed them and baby them.

CF: Really? Margolin was giving them a bottle or something. AR: Right, exactly. I didn't do that but they were doing it. CF: Did it work?

AR: No.

CF: It didn't work. Well, then one day a famous gastrointestinal surgeon whose name begins with a G asked Moe to call a special meeting of the psychiatry department, he wanted to give a lecture on ulcerative colitis. He was a very handsome and charismatic man and a great surgeon. He begins by showing us some pathological slides of what the gut looks like in an advanced state of ulcerative colitis and he was treating advanced cases of ulcerative colitis, and he said you can't ever reverse these changes. Once they go this far all you're doing is making patients worse. What he was advocating at the time was total colectomy. He then brought up on the stage three or four of his patients whom he had treated with total colectomy. They happened to be a couple of handsome men and rather beautiful women, whose guts he had cut out, but they just looked marvelous. They had gained weight and they were well, and he had a special erotic relationship with all of them. He'd hug them and pat their behinds, he was very seductive with them, which probably didn't interfere with their getting well, and it was a very impressive demonstration and also an admonition to us that we were barking up the wrong tree. He said there may be some cases of ulcerative colitis early that you can do something with.

AR: The same thing happened in Brooklyn with Melitta Sperling.

CF: Yeah, they were doing the same thing, but with an advanced case it's much too late.

CF: Isakower wanted me to give a course in early ego functions. I hardly knew anything about mature ego functions, let alone early ones, so I consented to give it, but I wouldn't give it on early ego functions because I didn't know what the hell to talk about. Anyway, he sort of liked me. It was during the time I was working on my subliminal experiments, and I was giving a paper at the Institute. Do you want to hear this?

AR: Absolutely, fine. I have no objections.

CF: This will give you also something about the atmosphere of the Institute and about some of the leading characters. So I asked him if he would discuss my paper. This was a paper on sub- and supraliminal influences on dreams. And he said, yes, he would. He had never discussed anybody's paper— after he gave the Mudler paper—for 25 years. I gave him my paper to read, and the night arrived. Robie Bak was President of the Society at the time. My paper was a really good paper—it was well received.

Isakower got up, and he had a kind of elegant way of talking, sometimes circumstantially. He went on talking, and I could see that he was getting more and more critical and hostile, and finally he said things that were really rather psychotic. What in effect he said was that I was showing a slide tachistoscopically of a snake and a vase with a swastika on it. These are

powerful stimuli. I'd made one supraliminal and the other subliminal, and I'd alternate them, although at this time I had only shown the subliminal snake and the supraliminal vase with the swastika. Now the subjects had some interesting fusions between the snake and the vase. The subjects produced dreams of a homosexual nature, as if they were being orally attacked by the snake. One of the purposes of the experiment was to see if the dream handled the subliminal stimulus differently from the supraliminal stimulus, and I had evidence that it did. The subliminal stimulus was much more connected to primary process, the deeper wish fulfillment aspects of the dream, whereas the supraliminal stimulus, which was also represented in the dream, had to do with the swastika and would produce associations of concentration camps and Hitler, Nazis and things like that were perfectly rational secondary process associations. The snake had to do with id stuff. The dream also showed certain defensive processes in the subject and attempts to deal with the anxiety about and desire to be homosexually attacked. Isakower said these are not dreams at all. He said these are dreams from "above," not dreams from "below." These were all dreams from above, and he said they weren't even dreams. He said the subjects had a right to protest because they were being homosexually attacked. What in effect he was saying, he was mistaking the symbol for the thing symbolized, you understand? I didn't quite gather all that he said until I thought about it afterward, and I told him. He didn't pay any attention to me and never talked to me again after that; he was such a crazy character. He acted as if I was actually putting a penis in their mouth—as if this was a real event. I said I was just showing them a picture of a harmless snake. The rest was dream, the unconscious did all this. I wasn't attacking them, but this was a "psychoanalytic" interpretation on his part, and it was weird. It tells you something about him which fits in with everything you know about him. It was a disastrous evening because he hadn't been up on that podium for a long, long time. He went on talking and talking about 40 minutes,

and he and Robie Bak didn't get along at all, and Robie Bak never took any nonsense from him, and he hinted that he was going past his time. It's late, there are other discussants. There was a good deal of discussion that night, as a matter of fact, and Bak finally simply told him he had to stop. Isakower never appeared again, and he never talked to me again. If I passed him he would ignore me, as if I had done something bad to him, that's how nutty he was.

AR: But he stayed on and taught his course for many years after that.

CF: Yeah.

AR: I presume he was a training analyst, and he analyzed and he supervised.

CF: Yes, he did. Some people thought he was quite wonderful, although I asked a lot of students, what did you really learn from him? And everyone always had trouble telling me just exactly what. But he did have a kind of subtle mind, and his three papers have some interesting things in them. However, a paper I wrote much later after he was dead and so he couldn't read it was on "The Spoken Word in Dreams." He never gave a single example of such a dream in his paper on the subject in 1948. He gave examples of psychotic productions, he gave some examples of things that Freud reported somewhere, I think in his book on aphasia. Several times Freud was threatened with death. He heard out loud some thoughts like "Your time has come," something like that, but it was out loud, and he gave that as an example. He gave a few other things, but in the one example dream he gave there were some spoken words but not in the dream, but after the dream. Isakower was interested in a certain kind of spoken word,

ominous, portentous, the voice of the superego, speaking in its most punitive, harsh terms, the kind of dreams that don't occur very often but they have occurred with him or he was very acutely aware of them because they probably represented the kind of superego he had, harsh, portentous, ominous, whiplash, like the way he'd treat students. He'd make a fool of you, destroy you. Well, that was my adventure with Isakower. My relationship with analysts was more in connection with their pathology, these notable people.

AR: Well, there was a very special group at New York connected with the dream, and Isakower was one, Bert Lewin in a sense, Nick Young, and yourself.

CF: Bert Lewin I had troubles with, too, although Bert I admired. He was really something special, but I never had much confidence in his dream screen. It was only a kind of metaphor, although he treated it as if it were real. And Nick Young, that monster. There is today a world industry in REM sleep, and it's an accepted phenomenon, but he still doesn't believe it's real. But you knew that.

AR: Yes, I had him for the dream course. Didn't you give a special thing on REM sleep that was added because Nick wouldn't talk about it?

CF: For about five years I gave a course on the psychophysiology of sleep and dreaming.

AR: Exactly. That was to make up for the aspect of the subject that Young wouldn't talk about.

CF: That's right, and nobody else has talked about it either. I think Bill Grossman talked about it a little, but I've seen the bibliography that's given out in the dream course.

AR: I've taught it in my course at NYU in my course on Chapter 7.

CF: Yes, but you're not New York. Although the students used to like the course.

AR: It's an excellent course. It's a wonderful bibliography. I used it in my course on Chapter 7.

CF: You know, I've made some revisions to Chapter 7, which have never been incorporated into the teaching at the Institute.

AR: They've never been published?

CF: Oh, sure. In 1957.

AR: Oh, yes, of course, I've read that, I know it well.

CF: I made some much-needed corrections in the Picket model and that's another story, I'll come to that.

AR: We'll get to that later. You gave your paper at New York, you came in uniform, you were clearly invited, you had no connection, and the next paper you gave you were already a training analyst, so there's a gap that has to be filled between those two presentations.

CF: I gave my first paper as a member at New York in 1953.

AR: Was that the paper on fugue-state hypnosis?

CF: No. I have a period to fill in. Fugue-states and hypnosis was in 1944. I wrote three papers on fugue states, one with Ed Joseph in 1948. The first paper I gave at the New York Institute was in 1953.

AR: The current New York Institute.

CF: Yes.

AR: But the first one was given to the Society, I thought, the one on fugue- states in 1944.

CF: Yes, that was given to the Society in 1944.

AR: So that was the first paper.

CF: That was the first paper, yes. Anyway, to come back to Ellis Island. We saw a lot of stuff there, a lot of neurological stuff. Sam, Wortis used to come over once a month, and we saw a lot of psychiatric cases of one kind or another. One of the most interesting things we had to deal with was that Hitler was sending over some of the flotsam and jetsam out of the concentration camps. There was a certain amount--we had captured a number of German submarines in the war, and some of them were imprisoned in Texas or somewhere. The State Department arranged a trade of some of these prisoners for some of the Americans who were in concentration camps but who had married German women or vice versa, and some of them had kids and periodically a couple would be sent back. And we had about six or eight doctors there at Ellis Island, all of them Jews, and a couple of them became members of the Institute, like Dave Kairys. Herb Kupper, who is out in Los Angeles, was there.

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.

AR: Is that in relation to the dream as wish and so forth? I've used your material in teaching a course on just chapter 7 at NYU.

CF: The dream as wish I still cling to, with modifications.

AR: Well, the purpose is the preservation of sleep.

CF: I modified that, that the dream does not act to preserve sleep, because you can do away with dreams by suppressing them with drugs or the so-called waking method, and sleep continues. Freud said that the dream work fails in its function when anxiety is produced so that one awakens from the REM dream. If the dream work is successful, let's say in binding anxiety, then REM sleep is preserved, but not sleep in general. People can sleep quite well during the night, even if REM sleep is suppressed altogether. Nonanxious dreaming thus preserves REM sleep.

AR: Yes, you don't need dreaming to preserve sleep. You need dreaming to preserve REM sleep.

CF: That's right. And then I made modifications in the Picket model, which I think are important, and I made it way back in 1957. And it's a model now that the cognitive psychologists, using different terms, have picked up. Theirs is not as good as a model as I made.

AR: What was your model?

CF: You remember the Picket model? Freud made one mistake, one inconsistency; although he said that all mental life begins in the unconscious and some of it ends in consciousness, he equated perceptual with conscious. The greater part of what psychologists would call perceptual processing or information processing goes on in the unconscious and in a final phase some of it reaches consciousness. In his model,

Freud equated perception with consciousness, e.g., if you look at something you're conscious of it at the moment of perception. All the work that's been done since Poetzl shows that conscious perception is preceded by a phase outside of consciousness. Shevrin and others have shown that between the instant of registration and the arrival of a percept it into consciousness takes about a half-second, and that's quite a long time. These findings have important implications for the theory of perception and the theory of dream interpretation.

AR: That was an exciting new field.

CF: This goes back to 1917. This is what Poetzl said: With a dream, he said, that parts of the percept that aren't seen nevertheless register. It's a very weak stimulus which becomes a memory trace, and in the middle of the night there's a second activation of that preconscious memory trace, and it gets into dream consciousness as a delayed delivery. The same is true for images, for hallucinations, and for dreams.

AR: Isn't that like Palombo's ideas about dreaming as a system for the sorting of memories?

CF: Palombo? I haven't read Palombo lately. He is a believer in information processing, and I think he's coming closer to combining it with psychoanalysis. I have been in touch with some information processors, and conscious perception and dreaming can be put in information processing terms. They are now making diagrams, quite like the diagram I made 35 years ago, although mine is more complicated because it shows how the dream is formed. What I would call a precocious schema, they would call short-term memory, and they equate long-term memory with the unconscious. They now agree that these first two phases of perception and the dream process take place outside of consciousness.

Erdelyi has written a book on Psychoanalysis: Freud's Cognitive Psychology, and he puts it in information processing terms, but he's read a good deal about psychoanalysis and he understands it, so he's been translating these things into information processing and attempting to reconcile them. I have no objection to it. What I'm trying to do is put together the old subliminal discoveries of the '50s and make some kind of synthesis with the recent work of the cognitive psychologists. They've shown something that Poetzl was on to long ago, a thing called blind sight. Have you heard of blind sight? A man is blind, he can't see.

AR: But yet there's some awareness of where things are.

CF: You tell him there's something out there that he can't see but he should try to reach for it.

Marcel, an

Englishman, takes pictures of these blind men reaching for a glass, everything he does, the movements he goes through, reaching for it; he found that they made the motions that a person who can see would be making, and not just random. It's as if he can see without knowing that he can see. What that means is that his vision is perfect but he can't see. He has a cortical blindness. If you have cortical damage, the pathway that registers percepts and interacts with memory traces is intact and can inform one there is a glass out there, but the path way from there to consciousness is damaged so that the person can't have any awareness of it. However, he has registered it so that in some way he "sees." It's just the final step of getting into consciousness that is missing. There remains a preconscious knowledge of the glass.

AR: They've done these studies with the split brain.

CF: The split brain stuff has something to do with this. Anyway, it's just that there is an interruption of the complete visual pathway, but there is enough of the pathway that registers and makes a memory trace. There is a memory trace there without visual recognition. Dream formation involves related processes. Poetzl put all this together, this kind of blind sight, because he was essentially a neurologist. He put together blind sight, idetic imagery, hallucinations, the normal dream, and normal perception.

AR: The definition of geniuses, right?

CF: He was a crazy genius.

AR: He'd seen things before everyone else. Freud had it.

CF: Freud had some of it. Freud kind of lost sight of it. I didn't have it, but I had enough of it so that as soon as I read Poetzl, I had a profound conviction that this was true, and I said this is the way the brain and the mind work, must work.

CF: At that time I had become closely collaborated with George Klein, Bob Holt--Hartvig Dahl claims he was influenced by me to go into research many, many years ago. The group around George Klein at the Research Center for Mental Health was Irving Paul, Leo Goldberger, Wolitsky, Spence, and Klein, and they all became research students at the New York Psychoanalytic Institute. I was Chairman of the Research Students' Committee.

AR: By the way, you know that in the early, 1960s Howard Shevrin was doing research based on your work in Topeka?

CF: Yes, that's how Howie and I became friends. In 1953 I gave my paper before the Institute, which was the first replication of the Poetzl experiment. In 1954 it was published. They were just preparing to repeat Poetzl themselves. Actually, Rapaport had tried to repeat it but Rapaport was a theoretician, he wouldn't know how to set up an experiment. I'm not attempting to criticize

him, but... So George and Holt never repeated it. Aside from Poetzl there were a couple of Germans in 1922 named Allers and Teller who did an imagery experiment, that is, they'd flash a slide tachistoscopically and then have a person close his eyes and ??? [what is missing here? Barbara] image-it was more complicated. They'd have a first image and then have a word association test and were able to show that the unseen portions of the picture emerged into consciousness. I repeated their experiment myself and modified it and made it simpler to do--that became a thing that the psychologists took up and, in fact, the actual Poetzl dream experiment was hardly replicated, which is in many ways more important, though the imagery experiment was also important.

AR: You know the work of Shevrin and Stross?

CF: Oh, yes. Then Howie in 1956 got in touch with me and they were fooling around with hypnosis.

AR: Right.

CF: And then they read my paper. They were elated and so they repeated these things themselves in a very nice way, and then they went on; they were psychologists who had ways of doing things that were foreign to me. They wanted to quantify it, score it, and scale it, and they did their famous Rebus experiment. Do you know about that?

AR: You know who the subject for that experiment was.

CF: You?

AR: No, Arlene. You know the tiny rebus dream that was Arlene. CF: Is that right? Isn't that cute?

AR: Yes, in Topeka, in 1960. She could not understand how after she woke up and they asked her to tell a story, she said she began talking about this tiny, teeny town. A picture of a tie and a knee were presented, and they unconsciously fused to make the rebus of tiny, these tiny, teeny, people, and kept on in that vein.

CF: That was a marvelous experiment. They wouldn't always get the Rebus effect but they would get the associations to tie or knee, especially with people who had a looser unconscious.

AR: Arlene was a great hypnotic subject.

CF: They were the ones who would condense and make rebuses. So in 1963, I went out to Topeka and spent a month with Howie.

AR: Were you a Sloan Professor?

CF: Yes, I was a Sloan Professor. I was squeezed in between Margaret Mead and Aldous Huxley, I think. Were you there when I was a professor?

AR: Yes.

CF: Do you remember me?

AR: I really don't--yes and no--I remember some lectures.

CF: I gave a couple of lectures and I gave a seminar. I put in a month's work there, because Howie and I did an experiment.

AR: We had Huxley over for Passover, and Margaret Mead (she was not that involved with the residents) but I was there then. Huxley came for Passover, that must have been about April, and I think Mead came just as he was leaving, and that was in June. You were probably there in May.

CF: No, I went the day Mead left. She had a party and I went to the party. It was in June, and then she left. I had a very bad impression of her. She never stopped talking and the yield was about 30 percent. She talked and talked and a lot of it was just nonsense.

AR: I had just finished my residency and we left, because we took our leave at the end of the year, at the end of the residency.

CF: They had some very great people. I was flattered that I was asked. AR: Lorenz came.

CF: Yeah, Konrad. Well, I was included in that whole gang. About 1960 the subliminal stuff was blowing up in smoke. The academic psychologists attacked their own work viciously. They were doing something they called perceptual defense. Some people were doing subliminal advertising, which gave subliminal perceptual research a bad name. I was running out of ideas. I didn't know quite where to go, and then we were under attack from the psychologists, who repudiated their own work on perceptual defense. Now 25 years later it's coming back amongst the cognitive psychologists. In 1955 Bill Dement wrote me a letter and asked me for a reprint and then he sent me his thesis.

AR: He was in Chicago then?

CF: He was in Chicago, so I read that, and that was like another revelation. I said, "Jesus Christ, what a thing." There was a built-in neurobiological cycle in the brain, in the central nervous system, in the neurochemical system of the brain, which--it seems that there is a cycle that every 90 minutes develops into a REM period, and is there every every night from birth to old age with some variations, and it's during that time that you dream. You dream two hours a night, and whoever knew that--we all thought that dreaming, and Freud thought it, too, was a fleeting experience that occurred once in a while, although Freud in his omniscience had expressed the idea from time to time in *The Interpretation of Dreams* that we may dream all night long. The REM cycle was discovered by Kleitman and a graduate student named Aserinsky in 1953. It was a serendipitous finding, and they didn't know how important it was. Aserinsky was given his Ph.D. very reluctantly, on the basis of his work, and he ran away to Pittsburgh and he wasn't heard from for 10 years. After 10 years he emerged and discovered that a whole revolution had taken place in sleep research. He came to the annual meeting of the sleep researchers in the early 60s and was greeted as a conquering hero, and he didn't know all this stuff was going on. Dement picked up the work after Aserinsky quit. He worked for five years, and he understood the significance of what was going on. I began to communicate with him in 1955 and we got friendly. He had his Ph.D. and his M.D. but he never had an internship. This work was so little known at the time, in the 50s, it had no particular prestige. He wanted to take an internship. so I said, "Why don't you come to Mount Sinai and we can do some work together?" So that's what he did. He came to Mount Sinai in 1958, took an internship. In 1959 we got a joint grant from the National Institute of Health. My grant from the Research Foundation at Yale had come to an end. Dement took an internship there, and, after he was through, we established a little laboratory and we began to do some work together. I was 20 years older than Dement, and we began to have trouble from the very beginning. He was bringing his sleep research to me and he had done a lot of work, and by collaborating with me, he was afraid I would get the credit for it. The first thing we were both interested in was to learn more about the REM cycle that beats away every night. When you want to know what something is for, you suppress it, e.g., you cut out an organ to see what happens.

AR: Or you make a lesion in the brain.

CF: That's right. So we thought of ways of suppressing REM by giving drugs or using hypnosis. Dement had noticed that if he suppressed a REM period, the next one came in sooner, so he got the idea that there was a push to get REM sleep. He developed the following method: Every time the eyes began to move he would wake the subject, keep him awake for a while, and then let him go back to sleep. If he did that the first night, the subject's eyes would begin to move seven times and he would be wakened seven times. If we'd keep doing it, the second night it would be 10 times, the third night 16 times, and the fourth night 20 times, the fifth night 40 times. And so pressure builds to get REM sleep, and if you get REM sleep you get dreams. So this was a remarkable psychobiological finding. That's what set off the whole worldwide industry of dream research. Now, we were afraid to do this suppression, and I was afraid because Freud said a man deprived of his dreaming would undoubtedly go mad, and that the dream is the normal psychosis. I coined a phrase which is often quoted. W. H. Auden put it in his book of quotations that he published--he'd keep little things that he found, and he once quoted me as stating, "Dreaming permits each and every one of us to be quietly and safely insane every night of our lives." We were afraid that suppressing REM sleep would cause dreaming during the day or psychosis. Dement stayed up 250 nights the first year. I would see the subjects every morning and go over them and talk to them and make sure they were not going nuts. We did detect certain things. A couple of guys went into a state of depersonalization, a lot of them showed a certain amount of hostility and anxiety. We only suppressed at the most five days, on one person for a week and one person for 15 nights. This was a joint experiment. It was my laboratory. He had come to work for me, and I did a lot of work. I watched these subjects to make sure we weren't doing them any harm, and Dement needed me. He wasn't a psychiatrist. But I began to see, he began to get more and more secretive and wanted to do experiments by himself and not let me know about it. So he came to me one day and wanted to publish some preliminary results, and he said couldn't he publish these preliminary results under his own name? Because it was the first original thing he had done and he wanted to get the credit for it, so I thought about it. I was the senior person, I helped him set up the lab, and we collaborated. I said, "You can publish these original physiological results, but we'll write a second, joint paper on the psychological results," that is, the observations I had made. He agreed to that, but then he never acted on it. He wrote his paper and didn't even give me a footnote. He's always gotten credit for the work, and for years it was kept rather secret that I collaborated with him. I regretted that I had not insisted on a joint publication. Our relationship went from bad to worse. He became paranoid. Arguably, he became paranoid because of chronic sleep loss, which can make one paranoid. He wasn't getting enough sleep, and then he did other nasty things. He would go off and give a lecture without telling me he was going and people told me he would never mention me at the lectures, or invite me to come along, so his fear was that I would absorb his reputation--and I used to say to him, "Look, Bill, this is your work, you began it long before you ever came here and you are going to get very famous for your work, quite independent of me." And I tried to reassure him that I would not displace him. After two and a half years, I said, "Bill, you don't belong here. There's no future for you. This is a hospital." It wasn't a medical school yet. I said, "You need to get into an academic department," and so he

agreed finally. David Hamburg quickly became interested in Dement and he became full Professor of Psychiatry at Stanford, and he and his laboratory are famous.

This was a time when they were beginning to give the title of professor of psychiatry to people who were not psychiatrists. It started with Seymour Kety, who went to Hopkins, and he was made professor of psychiatry, but he only stayed a little while. He said he felt uncomfortable being called professor of psychiatry when he didn't know any psychiatry. But that didn't interfere with Bill. Anyway, Bill probably has the biggest sleep laboratory in the world. He made a big success out of it at Stanford, and his laboratory has produced extremely important work. But I had a really lousy time with him.

I was left with the sleep laboratory by that time--this was 1962. I was the sole director of the sleep laboratory, which I have remained all these years (until 1985), and I proceeded to do important work on my own. As an investigator, I didn't need a Bill Dement, although he was a first-rate one, very imaginative. The first thing I did was begin to work on nightmares, which I was interested in, and night terrors. Before that I demonstrated the REM erection cycle. I had heard that Aserinsky had mentioned that some Germans named Ohlmeyer et al., back in 1943, had done a curious experiment, which I always suspected was done in concentration camps, because they were always doing sexual experiments. However, this was a harmless experiment and a very good one. They hooked up males with some sort of gadget, some sort of ring which could expand, and if you got an erection it could end up in a tracing on a smoked drum. That's how primitive it was. They published a tracing showing just when these erections occurred during the night. Aserinsky, I think, had noticed that the erections seemed to occur every 90 minutes and he got the idea that they may have been occurring synchronously with the REM periods, so that idea was around. Several people tried to repeat it. One was a very good English psychiatrist named Ian Oswald, a very sardonic but bright guy. He and a man named Berger tried to repeat the experiment. Oswald wrote a little book on sleep in which he said they tried to repeat the experiment but they couldn't get the apparatus right and they couldn't make it work. And then he went on, nastily and ironically, to say that this is grist for the mill for psychoanalysts, but they probably don't read the right journals. Ohlmeyer, Hilsprung, and somebody else published two papers in Pfluegers Archive, an old, old German journal. So I hastened to go and get that article. I could read enough German in those days. I became excited after reading the article by the German workers and was convinced that the periods of erection could be correlated with the REM periods and felt that this was an important psycho-biological finding which had implications for psychoanalytic sexual and drive theory and important for understanding the neurophysiological basis of dreaming. I determined to repeat the Ohlmeyer experiment, and, with the assistance of an ingenious resident named Joseph Gross, succeeded in constructing what we called our "plethysmographic bagel," a doughnut shaped plastic device that enabled us to place it over the penis which when it erected at night exerted pressure on the walls of the bagel, forcing water in it through a tube hung on the wall by which we could get a manometric reading of the extent of the rise in the tube, thus obtaining a quantitative measure of the degree of the nocturnal erection. With this device plus the use of several other methods including direct observation we obtained very good results. We found that 95% of REM periods were associated with nocturnal erections, that they began and terminated in close temporal relationship to the beginning and end of the REM periods, that two

thirds of them were maximum, that they were present from birth to extreme old age, that nocturnal emissions occurred during them, etc. Nocturnal penile erections are associated with nearly every REM period and are present from early infancy to old age, though diminished in the 80s and 90s. Somewhat later on, I and my competitor simultaneously discovered nocturnal erections could be used as a test for impotence.

AR: Who was your competitor?

CF: A guy named Ismet Karacan, he's out at Baylor. What we demonstrated was that in psychogenic impotence these erections may remain intact. In organic they are partially or wholly impaired. However, it turned out to be not that simple, and the test has to be used cautiously because there are intermediate formations where it's partially organic and partially psychogenic. A high percentage of organic impotence is due to diabetes, but there is no reason why a man with diabetic impotence can't have a lot of psychogenic factors also. Anyway, it's a long story. My name is mentioned more in the urological literature than in psychoanalytic. Urologists are doing a lot of work with the NPT method. When used cautiously and with other means, it is a useful method. It's one of the practical results that grew out of sleep research, one more thing that I did--my final piece of research. The question kept arising, is there anything in the female that corresponds to nocturnal erection in the male? I was convinced that there was; there just had to be some way of getting at it. The logical place to look was to see whether there was clitoral tumescence during early sexual arousal. I explored that possibility and couldn't get any consistent results, and then someone produced a gadget, an intravaginal gadget, by which you could get a recording. The essence of sexual arousal in male and female is vascular engorgement. When the penis engorges, erection occurs. In women there is increased pelvic engorgement. There is a lot more erectile tissue in the female than one would suspect. The vestibular bulbs which surround the distal aspect of the vagina are highly vascular, resembling erectile tissue. The clitoris is composed of erectile tissue but it isn't very big; the so-called crura of the clitoris are rather large and probably are composed of erectile tissue. There are large venous complexes surrounding the vaginal walls which engorge with sexual arousal. The clitoris also erects. The clitoris is a kind of trigger; the easiest way to arouse a woman is to stimulate her clitoris. I think that's the method universally followed by the male, and with that she will also engorge. The first thing that happens in sexual arousal in the female is increased secretion associated with increased vascularity and tumescence. The last paper I published, in 1983, is called "Sexual Arousal in the Female, Diurnal and Nocturnal Sexual Arousal in the Female." It was published in the Archives of Sexual Behavior. If you want to read it I'll send you a copy. All these studies were long and arduous.

AR: What did that show, the last study?

CF: It showed that women engorge in 95 percent of their REM periods. However, there is not as much engorgement as there is in the male. There are some differences. I have some records that I published. One record is of a woman who had an orgasm during sleep, and I have some records showing graphs of two women who masturbated in the laboratory. In a laboratory you can get men and women to do most anything for science. They masturbated to orgasm; these are unique demonstrations. Then I found out that vascular is not infinite, it reaches a certain point. The engorgement that occurs at night without orgasm is as great as the engorgement that occurs when a woman masturbates, the same as in the male. A male gets a hard erection

during the night, and they are as hard as any erections you get, and that's erection without ejaculation. I have one recording of a wet dream in a male. The psychological purpose and function of all this is obscure. What do we need all that erection for? It's clear that when you have a wet dream, it occurs in a REM period, but why do we need 20 percent of the night in a state of full erection? It's a good question. I haven't been able to answer it. That's the last of my research.

AR:., By the way, what about Stanley Friedman?

CF: Yes, I forgot to mention Stanley. I collaborated with several people who have been very helpful. Irving Paul and I did a study trying to make the method of subliminal perception more objective. Irving Paul is a very clever experimenter. We did a couple of very good papers together. Stanley and I did a couple of papers together also. The idea was around that the REM cycle beats away during the day. Oral cycles and even the erections have been shown to go on during the day, but hidden by the waking state. Stanley did an experiment which I didn't think would work but it did, in which he had people in an isolated room, gave them food ad lib, and just kept a record when they ate, over an eight-hour period. They seemed to eat in a cycle, a 180 minute cycle. This has been confirmed. Oswald, who was always making fun of analysts, got very impressed with the fact that I demonstrated the erection cycle, which he wasn't able to do. Then he got even more impressed with the fact that Stanley and I demonstrated there was an oral cycle. He then repeated the experiment himself and, to his surprise, found that we were right. Oswald respected me, but he hates psychoanalysts. I always had a special position amongst these sleep researchers. They knew I was an analyst but also that I had done a lot of work in neurophysiology years ago, and my old work and reputation helped me a lot. But none of this tells you anything about my role as an analyst. One thing was certain. What I did was very hard to do, to do this kind of hard-nosed physiological work is antithetical to the kinds of intuitive talents required to be an analyst.

CF: What I had from Tarachow was the equivalent of a good course in the nature of the transference and in supervision in general. And that's really the best supervision I ever had. Later on, when I was admitted to the New York Institute and was told I ought to have a little supervision, I had six hours with Annie Reich. Annie Reich was a great woman, but she hardly said anything to me during those six hours. After six hours she said I was a mature psychoanalyst, which I never believed, and I wasn't a mature psychoanalyst. I really needed a lot of help. I was very uncertain of myself and, however, I reported to her precisely what went on with me and the patient and what I said and what the patient said, and she seemed sufficiently impressed. The only thing I ever remember that she brought to my attention, which was then a new idea to me, was about the pregenital defense against oedipal material, that is, there was a regression to pregenitality, as you know, against talking about oedipal things. And I hadn't heard of that, no one had ever spoken about that in Washington, and I hadn't read enough, so I knew about that kind of thing. But as I remember, that's the only thing she said to me that struck me in my six hours with her. There was a misunderstanding. She had kind of believed that I was going on with the supervision, but I thought I was supposed to stop at the end of the six sessions when I was admitted to the Institute. I should have gone on with her. She was a very good, brilliant analyst, and she also wrote some interesting things.

AR: I think Tarachow had a profound influence on the Downstate NYU Institute. People had been analyzed by him, and he was really very much a role model and an ego ideal.

CF: Yeah. Kanzer is also brilliant, but he is a very peculiar gent. I know him fairly well. He's a real schizy person. He is now very depressed. He had a son who committed suicide. Do you want to know these details? He had a son who committed suicide, and his other son won't talk to him. His wife either died or left him. He's a lonely person, and he speaks about himself freely that all he has is his work. He recently gave the Freud Lecture. Were you there?

AR: No, I missed that. He's also very bitter about many analytic organizations--ours and NYU.

CF: Maybe. I think he was President of Downstate. I think some of their leading members were bitter that they did not get the recognition they thought they deserved from the New York Institute. They all were talented, but I think they either made some enemies or were disapproved of because it was felt they were too neurotic.

AR: What about Maury Friend?

CF: I knew Maury Friend. As a matter of fact, we followed one another across the country, but I didn't know him.

He went to the University of California, Southern Branch, around the time I did. He was in Chicago when I was, also in Washington, and I got to know him some here, but we were never particularly good friends. We had some patients, I mean families, in common, and I know he did a very good job on a real paranoid kid. He had the son of a

patient of mine, and he was a decent guy, and I think somewhat competent, no ball of fire.

CF: You know the Communist Party had learned that a small, organized nucleus who worked together, if there were just four or six people, could enter a factory and organize it and take it out on strike. Amongst psychoanalysts in New York, small groups played a rather important role.

The first group was the most important and the most prestigious, which was the Arlow-Brenner-Wangh-Bores group. Those four used to meet together and they were very influential. Three of them became President of the American. They decided to--I don't know if it was a decision--put their efforts into holding office at the level of the American. In doing so they neglected their politics at the New York. However, I think all of them have been--I don't think Arlow has ever been President of the Society or the Institute. Now Brenner has been President of both.

AR: None of those were ever head of the EC or the head of the faculty.

CF: No, they were never. They were all on the EC at one time or another, but they never had real power, and they thought they deserved power. They were very arrogant, knew their value. They said that they had new ideas but they never had a chance to put them into practice. '

AR: In regard to training, they were against starting at the beginning, from what I have been told, reading all of Freud. They wanted to stress Freud post 1925.

CF: Really? I never heard any of these ideas expressed, and they certainly never had a chance as a group to put them into effect. But Brenner, in later years, managed to carry a majority of the Institute but he missed a two-thirds vote by about eight votes, and his power ended there. He really organized all the malcontents, the malcontents being those who lived in the suburbs, those who were disappointed because they weren't made training analysts, those who thought our Institute was too big to give everyone a function. And a lot of people thought that they had nothing to do. The Institute didn't mean anything to them, and it was too remote an organization.

AR: In a sense that's being repeated now as well, without Brenner. The current group that controls, the Board of Trustees, is also an alliance--I'm talking about George Gross, who is taking the same political posture that Brenner took.

CF: Things have a tendency to repeat themselves. AR: It seems that way, yes.

CF: And once a group gets in power, the group that was on the outs, when it gets in power it begins to behave like the group that they replaced. That seems to be the way things work. Anyway, they never did attain the power that they wanted. Jack Arlow for some years sort of boycotted the Institute. He had been asked to run for, I know, the EC. He's been on a number of times but has refused on the grounds that he doesn't get his way. I think he openly says that and I'm sure they've wanted to run him for President of the Society, which, of course, he deserves more than anybody for his scientific contributions, but he's not been. I don't know whether he's refused that or what. He's gone around and done a lot of lecturing, maybe in defiance, to lay groups. At the time it was disapproved of, so I wonder if there wasn't some rebellious thing in that.

AR: Well, you know--did you know much about the episode that caused the great problem? That was the Waldhorn problem.

CF: I told you I was instrumental in getting Waldhorn finally made a training analyst. Did I tell you that?

AR: I think so--but what did you do?

CF: He had been turned down 11 times. I was on the Faculty Advisory Committee a few times. The last time I was on, Dora Hartmann was on, she was chairman, and I think there was Donadeo and Lily Busell. Anyway, Dora Hartmann was a very nice human being. I think she was not any ball of fire but she was a decent person. So she insisted that we investigate these people more thoroughly than they were investigated in the past and the whole thing was often very perfunctory. And I brought up--why was Waldhorn rejected 11 times, what did they actually have against him? The only same year or a year apart, and I may have been the first, although no one ever said that. I know Edith Jacobson and Annie Reich had a lot to do with seeing to it that I was made a training analyst, a post for which I did not at all feel qualified. I didn't seek it or welcome it or want it, and I was very anxious when I was given it. And I have my anxiety problems about such things and a lot of self-depreciation. I never thought I was good enough, but in fact I wasn't good enough in my own estimation. And then what they asked me to do--they asked me to teach a course. And I guess it must have been Dr. Jacobson's doing because she had an overestimation of me, so it seems. She had a lot of countertransference, which she controlled very well, but I knew that she cared a lot about me, which was a corrective emotional experience much needed. So she promoted me.

AR: They asked you to teach a course.

CF: Yes, the course was a reading course. The students had to read four papers, The Problem of Anxiety, the book, which, by the way, had never been taught until then. It's impossible to imagine why that happened, so I had to teach that, the paper on the "Unconscious" (the 1915 paper), "Mourning and Melancholia," and a paper by Bibring on the instincts--all those four papers. I was not a good teacher. one of the things I must say about the Institute--there's been some correction since--in those days a person might be appointed a training analyst and be

permitted to teach without any inquiry whatsoever as to whether he was able to teach, whether he wanted to teach, whether he had any experience in teaching, whether he had a talent for teaching, as if teaching was something anybody could do. Teaching is not something that anybody can do.

AR: That continues to this day.

CF: I wouldn't be surprised.

AR: I was appointed training analyst last year and then I get a call from the Curriculum Committee telling me that I'm going to be teaching a course on obsessive-compulsive neurosis next year, which is something I think I can do, but it seems to me it would have been more appropriate for someone to ask me.

CF: Do you want to, are you interested in teaching it?

AR: Yes, there's always been the attitude that what they have to offer is very desirable and they just make the decisions.

CF: It's nonsense. There are geniuses who make terrible teachers. There are especially a lot of research men who have no gift whatsoever for teaching.

AR: And some of the great analysts, the great theoreticians and writers as teachers.

CF: And some of the great teachers are not creative people, but they're good teachers. They know how to teach what is known. So here I was confronted with--what I had to do was keep a step ahead--my first class was with six very bright students.

AR: What year was this? CF: 1956.

AR: What class?

CF: It was a first-year course. That may have been the time I had Manny Purer, Horowitz, Bill Grossman, I'm not sure. I may have got them mixed up, but I had all these people, some of the brightest students we've ever had--Joan Erle, who was extremely brilliant at the time. She's no dummy, Joan; I don't like her. Then there was another woman who died, who I think was a student, who I thought was just the most brilliant thing we had--I forget her name, she died of some disease. Anyway, I had students of that caliber who were as bright as I was, and all I had to do was keep a little bit ahead of them, and I never felt comfortable teaching. Then a little later on when Isakower became Chairman of the Curriculum Committee and, you know, the arrogance of the New York Institute carried over into things like, we boasted that we had the most superb curriculum of any institute in the country. I don't know what the proof of that was--probably it was--but it was something we seemed to be especially proud of, although I didn't see that there was anything so terribly great about it. What was so great about it, the greatest thing about it, was that there were some great teachers, and it was supposed to be a perfect curriculum, presumably imitated by many other institutes, which is probably true. But Isakower, who at that time had an interest in me, and this was long before--1958 or 1959--when he discussed a paper I gave at the Institute and never spoke to me again after that, as if I had done some harm to him--he was such a crazy human being. Anyway, he wanted me to teach a course in early ego development. I didn't even know anything about later ego development, and I also told him that we don't know much about early ego development, I didn't know what to teach. So what I did was--he insisted that I go ahead and do what I could-- so while I was doing my subliminal perception work, I spent a good deal of time talking about my own work, which had to do with perception and memory and at least certain

ego functions of that kind. And as I remember, the students used to give reports on their teachers. I got some fairly good reports. They used to complain that I had a monotonous voice, sometimes sounded very tired, and the part they seemed to enjoy most was my own work, when I talked about perception, when I must have come to life, and I showed some slides and things of that sort. So I stumbled through that course, and I taught it for a number of years.

AR: Were there students who stood out at this moment?

CF: Oh, yeah. The students who stood out were Manny Furer, Bill Grossman, who I was always very impressed with, Horowitz, Joan Erle, I told you- And then for about five years I taught a course on sleep and dreaming, "Recent Research in Sleep and Dreaming- That was very successful. I knew that it was all new. The students were especially enthusiastic during the early years. Then everyone seemed to get tired of it.

AR: Was Howie Roffberg in your class?

CF: No. I know that Howie Roffberg was a student for a while, but they let him go. You know one of the reasons they let him go? He was interested in dreams, you know, but he never could remember a dream of his own. They figured he was very tightly defended, but I don't know what the real story is. It was probably a mistake to have let him go. He was a very bright person, and I think had a lot of problems. But he had a real brilliance as a researcher and was humble enough to want to have analytic training. Why did they let this talented, original man go and take in a lot of hacks? We have a couple of papers together. There was one paper on the ontogenesis of dreaming from infancy to old age. He did the essential work. It was done under me, and then he did the best paper ever done on the relationship of eye movements to dream content. There's still a controversy over the problem.

AR: Yes, the dream where someone is going upstairs? (associated with a series of vertical eye movements).

CF: Yes, and the eyes would go up. I had suggested this experiment to Dement in 1955. He had done some preliminary work on a man looking up, at an airplane or someone climbing a ladder or watching a couple playing tennis, where you get back and forth eye movement. There's no doubt there's some relationship, but it isn't a one-to-one relationship. The whole trouble with the attempt to relate the events in a dream to the occurrences in a dream, that is, the direction and motion and so forth of events in a dream to the physiological concomitants has been fraught with particular difficulties. It isn't as simple as anyone thought it would be, and there's a correlation of low significance when it's not a one-to-one relationship.

AR: How did your course end, after five years?

CF: I wanted to end it but I remember there was a meeting of the Curriculum Committee on which I was then serving. At that time the committee was in charge of these various students--Aaron Esman was one his the students, too, whom I was impressed with. There were a lot of them, all that group that's now in power. I remember I wanted to give up the course. The reports on the course were getting more critical than they had been, and I was getting tired of giving it. Anyway, and so I told them I didn't want to give it any more. Maybe I'd give it again later. So I remember that Horowitz and Esman and all those people leapt on the idea that I was going to stop giving it, and the implication was that I would be asked to give it again later on. But I never was asked, and I think they wanted the slot for some other course. The interest in the

psychopathology of sleep and dreams made very little impact. People would ask, "Well, does it alter the way we interpret dreams?" I would assure them that it really has nothing much to do with the way you interpret dreams. You can go on interpreting dreams the way you always have. Everyone would give a sigh of relief and then dismiss the whole matter from their heads. And no one seemed to be interested in the theoretical implications. All the problems that this remarkable psychobiological cycle built into the central nervous system and neurochemically regulated, what the hell it was doing there, why it was there, what its function was, and the many, many problems that it raises.

AR: Did you have any discussions with Nick Young about this?

CF: Nick Young, yes. When Dement and I first gave the paper at the Institute--that was in 1960--Dement and I presented the first findings on REM deprivation, which I think we got Rapaport to discuss, he came and discussed it favorably. Ostow got up and denied that the whole thing had any validity. He insisted that dreaming occurred in stage four, all with that booming, rabbinical, self-assured voice of his, and Nick Young, who never said anything at meetings, he was afraid to get up and discuss a paper because he had such murderous ambitions and aggression toward everybody. He probably quivers in his boots. He has never given a paper, written one, but he's been a tremendous force for evil in that institute of ours, if you want my private opinion. Well, he from the very first scorned the whole dream research business. He thinks that Freud said the last word about dreams, and there isn't any more to be said about it. And to this day, I think that he thinks the whole business of all these major discoveries, which have created a whole new look at the way the central nervous systems works and its relation to sleep and waking and to dreams and all kinds of phenomena, it's spread all over the world and the basic findings are as certain as anything in science. . This son of a bitch seems to deny that it exists, and he doesn't mention it in his dream course.

It was on account of Nick Young that Steve Ellman got into a fight. It was just when he was about to graduate and I think he wanted to provoke a fight and wanted to get some excuse to resign, out of his own peculiar motives. Anyway, they got into a fight over me. They began talking about REM sleep, and Nick Young made some disparaging remarks, and Steve, who respects me and my work a lot, began to defend REM sleep. I don't know the details, although he told me. They got into a bitter fight, and Steve then resigned from the Institute. There was some attempt to make him stay in which Arlow was active. Arlow thought highly of him, enough so that he defended him, but he insisted on resigning.

CF: Sidney was in analysis with Hartmann, and he used to tell me about his analysis. What I had absolutely no supervision in was transference. I knew it was around but in the very poor supervision I had--Kubie, for instance, never mentioned transference once and no one in Washington mentioned transference either, so I didn't know anything about analyzing transference, and to this day I think I'm deficient in picking up the subtleties of transference. It's like I have some sort of block about it, but Sidney used to tell me about the kind of things--if the patient came late, Hartmann would indicate that it had to be analyzed, or if he entered the room in a certain way or he took a certain position on the couch--he went into lots of details. Sidney was a good teacher, as you can tell from the book. So I had a long course with Sidney.

AR: What was the point of talking about Sidney under supervision? What was the point you were trying to make?

CF: The point was that that was the only good supervision I ever had, aside from the sessions with Annie Reich. Somewhere I told you that Annie Reich never said much. She made the interpretation about preoedipal defenses and oedipal memories.

AR: We've done that. So you were a training analyst for 20 years.

CF: Yes. I was a training analyst for 20 years, and during this period I only really analyzed three students. What that represented was an average analysis of about six to seven years. I told you about the first patient I analyzed, who turned out to be a psychopath and the Admissions Committee passed on him, except for Nick Young, who had an extremely astute eye for paranoid psychopaths. And it was alleged that when he first began supervision and was on the Admissions Committee, the first 12 students he saw, he made the diagnosis of paranoid psychopathy, and they were all rejected. So that if he made the diagnosis often enough, he was bound to hit on one. The one I got really turned out to be a psychopath. He was a physician. The Admissions Committee, as I said, approved of him, all except Nick Young, who made a very good case against him, but nobody paid any attention because by this time his credibility had been exhausted. But in his first session with me, he confessed to all kinds of terrible things he had done as a doctor, like performing an abortion on his wife and engaging in a lot of dishonest things involving money. And so I kept him on in analysis, trying to give him a chance to change himself, but at the end of two years, I concluded that I wouldn't be able to analyze him. I reported to the Admissions Committee that I thought he ought to be given a chance with another analyst to see what he could do. So he was turned over to Arnold Pfeffer, who kept him for another year and ultimately concluded the same things I did, that he was unanalyzable and a psychopath.

AR: Were you at that point a reporting analyst--how did you handle that issue? CF: At that time we used to write rather lengthy reports.

AR: Everyone did.

CF: I think so. It was Robie Bak, when he was Chairman of the EC, who kind of pressed for secrecy, to the point where we were finally given the option that we could write one line, like "the analysis is progressing." You didn't even have to say backwards or forwards, or you didn't have to record anything at all. But what went on all the time is the nonverbal kinds of communication that were passed on by training analysts to other analysts and to one another in committee meetings and other places, by a tone of voice, an expression of disgust on their faces, or other ways. And there was always a debate about "how should we do this?" There was a considerable amount of gossip, betrayal of confidence, and all kinds of things. I don't know that the matter was ever successfully resolved to this day. Somehow with some cases you feel there ought to be some reporting and with other cases that are going on successfully and you think the guy is going to make it—you might not have to say much, but it's a very thorny matter and one of those unresolvable things.

AR: Who was aligned on the other side from Robie Bak?

CF: I don't know. I don't remember that there were divisions in the matter. I think in general people were rather reasonable but no one really knew quite what to do, as I remember. I don't remember any desperate battles about it.

AR: I think the people who left to form Downstate tended to take the reporting ethos with them.

CF: Really? Well, anyway, what ended up with us was a sort of mishmash. Some people would report, some would not report anything, some people were lazy and didn't care to report. And there were different motivations for it, and I'm sure you know more about how it is now. But it's not resolved, there's no good way to do it. So that was my first case. Now there's a gap of some years there because I only had him for about two years, then I had another patient for six or seven years who was of dubious quality, but his supervisors finally thought enough of him to recommend him, and it was a difficult analysis, but he was made a member and he has found a niche for himself. Shall I name names? I won't name names. He had some special training that gave him a special position and he's done all right. I didn't make a mistake with him. The third one was a man who had a severely pathogenic history with a psychotic mother but with a good father who served as mother, who never remarried. His mother was hospitalized when he was five and he never saw her after 13, but he had a father who was both mother and father to him. The one good object seemed to have saved him, and after a long analysis, he did okay. For a long time he would lie rigidly on the couch and not move a muscle. I remember I considered it a great analytic triumph when he finally raised an arm off the couch and his affect in general loosened up, and he was a sensitive guy with a certain amount of talent and a very decent human being. And in spite of his background, he was approved for graduation and it has turned out very well, very nice. So those are my three cases. Since I was doing research and had only a part-time practice, I didn't think I could take more than one student, partly for financial reasons. All the years I did research, I did this at a considerable financial sacrifice. I always did some supervision, which I liked to do, but I used to take one case--for a long time I think I took two cases, even, which was about average--but in later years I took one case, but I supervised up until relatively recent years. I enjoy supervision. But as I say, I did it all at a financial sacrifice. What I got paid--people had some delusions about how much money I made doing research. I never made much for half-time, 20 hours a week--that was just what I gave in the laboratory. I didn't count all the times I gave nights and staying in touch with the laboratory or all the time I spent weekends and all the days and nights I spent writing, because I was tremendously productive in those years. So the most I ever made was \$20,000 a year, whereas in private practice I would have made three times that for an equivalent amount of time. So that's the kind of sacrifice I made. At the same time I was giving a lot of time to the Institute, but I was not one of these giving three or four times a week at the Institute. I held a lot of offices, and also was for years Chairman of the Research Students' Committee, which represents a certain amount of work, a lot of work, and you had to write reports and have admissions interviews. The Research Committee had the role of the Intake Committee, Students Committee, Graduation Committee. But finally when the waiver for a full training was adopted, it was decided it was too risky to have the Research Committee take that responsibility, that the EC had to do it. So the EC took it over and changed the rules on them, causing a tremendous amount of resentment because the research students were only required to supervise two cases, and by this time the research students were having a hell of a time getting any cases. Practice began to get bad, and they couldn't get good cases, and they weren't all gung-ho for doing more supervision. Many of our regular students in those days couldn't get enough supervision. They would not only do their three, they would do four or five, and keep supervising for years, and in the end-- this is a story worth telling. The chief research

students were six from, the Research Center for Mental Health and George Klein was the first, Bob Holt was always believed to have been a research student, but he never was, he never applied. The six were George, Spence, Pine, Irving Paul, Wolitsky, Leo Goldberger. These were six very talented men who had all produced some creditable research but for the most part they did not enjoy their training. They were all full-time students. They were all engaged in research. They finally had to supervise cases, had classes three times a week and five times a week analysis, which ate into their research time. And, as you know, they have all ended up not doing hard-nosed experimental research-but, like Fred Pine at Einstein, Wolitsky and Leo Goldberger at NYU, are important in clinical psychology departments around New York. Irving Paul at City College, Spence at Rutgers. Spence has done a little and he has recently gone back to doing a little subliminal research. He was in many ways the most original and talented of those people, but they were all very talented. The theory was that they would do the hard-nosed psychoanalytic research which we were unable to do. Their interests all stemmed from Rapaport, and George Klein during those years in the fifties; all of them were engaged, including Klein, Wolitsky, and Spence, in doing subliminal research of one kind or another. Rapaport was interested in memory and perception and in ego functions and in the thought process, primary and secondary process, and he influenced all of them. He was interested in how secondary process grew out of primary process, an unresolved question to this day. And all kinds of questions of this sort. So several of them told me, "We probably went into analysis because we were becoming rather disillusioned with the research we were doing and didn't exactly know what our own goals were and where we were going." So you see how they ended up. They ended up doing something very valuable. The protocol for being a research student emphasizes not only research but teaching, so all these people were teaching and supervising theses in various places and writing books on psychotherapy, and whatever deviations they have--George Klein had a lot of differences and all of them have a lot of differences...but they are in some ways basically Freudian.

AR: They enriched psychoanalysis, I think.

CF: With some of their writings, some of them have. I think George Klein wrote some interesting things, which I don't think anyone pays much attention to. One of his best papers is a paper on consciousness, which is something that is practically ignored. One of Freud's allegedly great papers was supposed to be on consciousness, which I think maybe it's been discovered since....

AR: No, it was a paper on hysteria.

CF: But amongst those metapsychological papers there were about five more which never....

AR: That was one of the missing papers.

CF: But he never wrote the one on consciousness, but George Klein wrote a very good paper distinguishing the functions of consciousness and preconscious and unconscious, and George was a very bright man and an influence on me. I don't know if I told you that story but I'll get around to it.

To come back to the research students. They felt like second-class citizens as against the medical ones and they had the reputation of not being as clinically savvy as the medical ones but having better theoretical minds, which is about true. They could handle theory better because they were more critical, more methodologically oriented, and that kind of thing. So

when George Klein was the first one to get ready to graduate, he put up a fuss. He said he would not accept graduation unless they changed the rules, and the rules were that they be graduated and given full membership, that they be allowed to teach, and that they be allowed to practice. At the time they had to take a pledge that they would not practice and they were not allowed to teach, and also that

they would have the right to vote, they wanted full rights. And so I remember Sam Atkins and I and some other people supported them, and we managed to push it through the EC. And I don't know if it ever came to a vote by the membership, but finally they were granted full rights. However, they then changed the rules on them. They said they would have to have three supervisions. They had come in with the idea that they only had to have two. So I remember we had a meeting and I chaired the meeting, and they protested violently. They said they had trouble getting patients and, by the way, none of them had much of a practice.

AR: But three supervisions was the requirement for everyone else.

CF: Yes, but they were never told they had to have three supervisions to begin with and they resented, one, because the rules were changed, and secondly because they had such trouble getting suitable cases. So if you look at the roster of membership, you see the only ones who are members are, I think, Spence and Fred Pine. Fred Pine is and Rogaw is, you remember Rogaw is a political scientist. He was not with this group at all, but on his own.

AR: From this group, Spence and Pine became members. Wolitsky could have become a member but he either decided not to or resigned. Paul never did it nor did Leo Goldberger. George Klein died.

CF: That's right, George Klein died. But I think he was posthumously made a member because he died about that time. And Wolitsky supervised with me. He'd gone to Nick Young. Nick Young just had contempt for him and wouldn't say much of anything. And the guy had little experience, even doing psychotherapy, and what he needed was instruction about how to go about doing therapy, which he had never really had. So I supervised him, and he turned out to have quite a bit of talent. He did very well with a somewhat difficult patient he had, and I think he had three supervisions. I don't know why he never became a member.

AR: I think he decided he didn't want to be a member.

CF: Maybe. There was something funny about it. But I was friends with all of these people, and I liked them, especially I liked George, and I had a lot to do with Bob Holt. So on the whole I had come to the conclusion that having research students was a problematical thing and I questioned whether we should continue to do so. We didn't seem to do them much of a favor. They were all full-time academics with their own research careers and doing very well. They got waylaid for six or seven years by the demands--by this time they were in their early thirties and when they ended they were 40 years old. There was no way they could make a change and furthermore they were dedicated psychologists and had their own careers, and, by this time I guess, a lot of them had tenure and stuff. Whatever problems were in the imaginations of the people who thought up the idea of having research students were never resolved.

AR: How do you feel about full clinical training for people without M.D. degrees?

CF: I'm not opposed to full clinical training. I have a bias toward medical training, but it's perfectly obvious that there are nonmedical people who can make perfectly fine analysts, and some of our best analysts have been lay, right? I also have a bias against the trivialization of

therapy to the extent that's going on because in my day we used to believe, or certainly I believed, that you couldn't get enough training. I approached this treating of human beings with a certain amount of trepidation and awe, and I never thought I could know enough, whether neurology, neurophysiology. All the training I had I thought was valuable. I just thought I never had good enough psychoanalytic training to satisfy my standards. But I always thought that if provisions could be made for lay people to get decent training, I would have no objection to it. And, of course, Freud was all in favor of it, and it's done in Europe without causing any great revolution. And half the London Society were lay, weren't they? Although it seems to me that the London Society always had a lot of first-rate women analysts, like Brierley and Payne.

AR: And Segel.

CF: No, Segel was Kleinian. These brilliant women, most of them were M.D.s, not all of them.

One of the best of them was Ella Freeman Sharpe. Was she an M.D. or a Ph.D.?

But her little book on dream interpretation is one of the most superb things that anyone has ever written and has a lot of original things. She taught that symbolism was not the only indirect mode of representation. She was a literary person, and you remember she introduced the idea of other forms of indirect expression.

AR: Metaphor and Dreams?

CF: No, the book is called *Psychoanalysis of Dreams or Dreaming*, but the whole first section is on metaphor, simile, synecdoche, part for whole, and all of the mechanisms, literary mechanisms, such as plays on words, punning, and all those things. She wrote a couple of beautiful chapters on that stuff, and she gave examples of how she analyzed. It's a beautiful little book, there's nothing better, and she talked about names and how a name like hers, Sharpe, appeared in disguised form, and I thought it was superb. I don't know if it's read very much or thought of very much. Anyway, how did I get onto that?

AR: Essentially, the question of lay analysis, full clinical training for lay people.

CF: I don't know what happened to the experiment that Bob Wallerstein started out in San Francisco.

AR: There were some administrative, political problems. CF: It seemed to be a good idea. Did it work out?

AR: It did not work out.

CF: They were going to give a quasi-medical thing.

AR: The point is, they were going to train these people, but then these people couldn't get the right licensing and so forth. There were a lot of administrative problems in California and so forth, and therefore the program didn't work.

CF: So I've never been opposed to that, although I say my own bias--I have a biological bias and my work tends to be connected with psychophysiological matters and the mind/body problem, and I'm pretty well rooted in the brain and body and my old neurophysiological training has always been influential. And there's a continuity between me Did you read Mort Reiser's book on mind-brain-body?

AR: I haven't read it yet.

CF: I believe the medical model has been neglected. There is a myth that's gone around that Freud said--Freud did say it--that we have biological roots and the biological roots lay in the somatic sources of the instincts. And Freud, as you know, in his early years was concerned with

the origin of the somatic sources of drives, so his first theory was they were in the erogenous zones. Then he gave that up and he spoke vaguely about libido but he didn't know from where libido arose. But in "The Three Essays," which is one of the works of Freud's that influenced me most, which I think is one of his great creations, next to The Interpretation of Dreams,--he makes a remark, one of those little side things that he tossed off, little sparks of genius, often in footnotes--this he said right out in the text. "The chances are the libido is generated by the action of the sex hormones on certain parts of the brain," and that's one that turned out to be true. It has now been known for a long time that the neurophysiology of drive, that is, drive in the physiological sense, not in the psychoanalytic sense, involves the feedback of the male and female sex hormones acting on the sexual areas of the hypothalamus.

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