

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 Greeks 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.

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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 asphasia. 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.