

Oral History

The Life and Work of Robert S. Wallerstein: A Conversation

Luca Di Donna

Dr. Wallerstein, in speaking with Virginia Hunter (2007), you said that you had come to the United States with your father from Germany at a very young age. Can you tell me about that period in your life?

Robert S. Wallerstein

Well, I was a two-year-old when I came to America. I was born in 1921, during the terrible post-World War I period when inflation destroyed Germany economically for several years. My father was a resident in medicine at the Charité Krankenhaus in Berlin and had intended to stay in Germany, but his money was wiped out by the inflation. He had two possibilities for work: to go to Persia and become the court physician to the governor of one of the provinces (who had been a patient of his in the army) and his family, who were related to the Shah, which he declined because of travel difficulties—a several-day camel ride from Tehran to the provincial capital, which my father deemed too arduous for his wife and one-year-old son—or to try to come to the United States.

He decided to answer a *New York Times* newspaper ad for a physician's assistant in gastroenterology; and once hired, he moved to New York. Within a year, he had saved enough money to bring me and my mother to join him. Of course, German was my language at that time (I was two years old), and I continued to speak German at home until I entered kindergarten.

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At that time there was no preschool but one could enter kindergarten at age three and would have to stay until age six. So when I started kindergarten at three, I told my parents, "From now on we speak English around here!" But German continued to be my parents' language at home, especially with friends who had come from Germany, Austria, and Czechoslovakia. Everything was very formal; you would never use first names even with friends you had known for thirty years. It was always "Herr Doktor" or "Frau Doktor." I read recently of a woman who said about her upbringing in similar circles in America, "When I was a little girl, all my mother's friends had the same first name. It was 'Mrs.'" I lived in that kind of quite formal German household.

I was very involved with school. In New York's public schools at that time, good students were allowed to skip grades, so I literally skipped five grades through elementary and high school, and I graduated from high school at age fifteen and a half. My mother, who felt I was too young to begin college, sent me to live with her bachelor brother, a physician in Mexico City, for one year (1936–37).

Di Donna

What was your experience like in Mexico?

Wallerstein

In those days, Mexico City had only one million people, not the present twenty million, and Mexico City was very different from what it is today. We had a pleasant home in a quiet residential area near Lomas de Chapultepec. You could look out of the window and there was no smog, so you could see the volcanoes. I had a wonderful time there, and I studied art and Spanish.

Di Donna

What brought you from medicine to psychiatry and then to psychoanalysis?

Wallerstein

My father expected me to go to medical school and talked me out of my inclinations toward engineering and architecture,

on the basis that in those days it was very difficult for Jewish students to get decent jobs in those professions. If you went into medicine, you could be an independent practitioner. So when I returned to New York, I applied to just a few undergraduate colleges with the idea of going on to medical school. Although I wanted to go to Harvard and was accepted there, the scholarship that they offered wasn't enough, so I went to Columbia College and could live at home. After graduating from college, I continued at Columbia University's College of Physicians and Surgeons, starting in September 1941. Pearl Harbor came that December. Out of patriotic fervor, some medical students quit school in order to enlist, and the Army's response to that was: "We don't need more foot soldiers; we need more doctors." So they put us in uniform and ordered us to stay in medical school, and they even undertook to pay our tuition.

The Army condensed our medical education by eliminating three-month summer vacations so that we finished in three years (four nine-month years run consecutively). After an internship and maybe an assistant residency, the Army took you on as a medical officer. I did this and got my internship at Mount Sinai Hospital in New York, which in those days was not yet a medical school but was a very prestigious hospital and medical institution.

On the very first day that we got there, the chief medical resident sat down with us interns—there were twenty of us, all men—and said, "You guys come from the best medical schools in the country, where you got the best grades in your class; all that was just good enough to bring you here. Now we'll teach you medicine."

We were thrown into a climate of intensive medical training that doesn't really exist today. We did our own blood work, sternal marrow punctures, fluoroscopy, electrocardiography, liver punctures, and the like. We were responsible for preparing all specimens and for reading the results of all procedures, even x-rays.

After being picked up by the army as a medical officer, I began running an infectious disease ward and an electrocardiography laboratory at Fort Lewis, Washington, a major staging area of 100,000 servicemen returning from and going to the Pacific. I had no thoughts about pursuing psychiatry. Later,

when I returned to Mount Sinai Hospital to finish my senior medical residency, I continued to wonder, "What do I do next? Do I try to stay in academic medicine?" Although I was accepted for an NIH fellowship in the Harvard Medical School blood fractionation lab, I began to have the doubts that I would be doing only the laboratory work that would have been involved, with no patient contact or clinical work of any kind.

I had a fantasy that psychiatry was different from the other branches of medicine. Although clinical work and formal research activities were quite separate in other specialties, in psychiatry the research that I was interested in—how people change, for instance—was part and parcel of the clinical work. I upset my wife by telling her that I wanted to stay in residency for another three years; I was going to go on in psychiatry.

My wife had become a social worker, and she had a book called *The Psychoanalytic Theory of Neurosis* (1946), by Otto Fenichel. It was a comprehensive overview of the neuroses and their psychoanalytic understanding and treatment. I was looking for a residency that would make it possible for me to train in a psychoanalytic institute. So I arranged to be interviewed at the Menninger Clinic, then in Topeka, Kansas. But in those days you did not get any vacations from a medical residency; even on your weekend off, you were supposed to be available on call. Going halfway across the country to be interviewed was just not allowed, even though I told my chief of service that I needed to do so. He asked, "Whatever for?" My reply that I was switching from internal medicine to psychiatry upset him a great deal since he had high ambitions for me in internal medicine. He said, "Well, if you feel you must go, then go, but remember, you may be very, very lucky: they may turn you down."

Well, the Menninger Clinic didn't turn me down; they took me. I finished the senior medical residency on December 31, 1948, but I couldn't start in Topeka until July 1, 1949. In the meantime a psychiatric service had developed right there at Mount Sinai Hospital, and I went down to see the chief of the service to ask if he would take me as a volunteer for a six-month period without salary. Salaries were only about fifty dollars a month. You survived because you resided in the hospital. On your night off, you could go out, but you were supposed to come

back to sleep there. You had your room, board, and laundry; if you were lucky, you had maybe the fifty dollars a month to get a haircut and buy cigarettes and newspapers—everyone smoked in those days.

In July 1949, I went to Topeka to start my official psychiatric residency. I began with a required one-year closed-ward experience. When I arrived, the Menninger Foundation had 100 psychiatric residents in their program at a time when there were only 800 in the entire country.

Di Donna

Was the Topeka Institute of Psychoanalysis already established?

Wallerstein

Yes. The first institutes in the country had been in five cities: New York, Boston, Philadelphia, Baltimore-Washington, and Chicago. Karl Menninger commuted each weekend by overnight train from Topeka to Chicago for his analytic training, becoming the first graduate of the Chicago Institute; no one flew in those days.

Di Donna

What was the psychoanalytic ideology of the Menninger Clinic at that time? What was the direction of the ideas?

Wallerstein

Theoretically, it would be seen today as a very conservative place. It lived in the dominant ego psychology tradition spearheaded by Heinz Hartmann. The chief psychologist was David Rapaport, who created his psychological testing program there, and he taught a course on chapter 7 of Freud's *Interpretation of Dreams*. I heard that he would say to his candidates, "Each time I read this chapter—it's now the fourteenth or fifteenth time—I see different things in it."

Rapaport left in 1947, before I got there, when Robert Knight left. Knight went to Austen Riggs in Stockbridge, Massachusetts to create a psychoanalytic "sanitarium" at what had been a very old-fashioned, organically oriented, private

neuropsychiatric setting. When Knight left, not only Rapaport went with him but others did as well: Merton Gill, Margaret Brenman, Roy Schafer, and Allen Wheelis. There was still so much talent at Menninger's, though, that the place didn't seem to miss them.

After my experience in Topeka, at the end of 1951, I applied to the New York Psychoanalytic Institute because I wanted to go back to New York, as well as applying to the Topeka Psychoanalytic Institute. In one of the defeats of my life, I was rejected by the New York Institute. This upset my sponsors—prominent members of the New York Psychoanalytic Society who had offered me all kinds of inducements to get me to come back and finish my training with them.

Di Donna

Why do you think you were rejected?

Wallerstein

Well, I was told, "You are a young man in too much of a hurry." The Topeka Institute took me, so I stayed there and got a job there.

Di Donna

New York didn't recognize your promise, even though you had worked at the Menninger Clinic?

Wallerstein

Many decades later, the New York Psychoanalytic Institute made amends in a way. I was invited to give the Heinz Hartmann Lecture there. It was a very prestigious lecture; you had to wear a tuxedo when you gave it. They also invited me to give the Charles Fisher Memorial Lecture of the New York Psychoanalytic Society, and another time they gave me the Heinz Hartmann Award, a big one.

Di Donna

Did it make up for the loss?

Wallerstein

All that was years later. So I stayed in Topeka and graduated from the residency. Then, since I had had five years of internal medicine, I was given the job of running the psychosomatic unit at the affiliated Veterans Administration Hospital.

Di Donna

One thing that is difficult to understand today is the political position of the German psychoanalytic immigrants when they came to the United States. Many of them had aligned themselves with the Marxist movement in Europe. But they would never discuss publicly what went on in Germany. It became a part of the past that was cut off. Do you think this attitude limited their influence on the theory and technique of American psychoanalysis?

Wallerstein

Well, many of these people in Austria, Germany, Czechoslovakia, Hungary, and Scandinavia had been political radicals. Some of them were communists. Wilhelm Reich was a member of the Communist Party; so was Edith Jacobson. Many more were social democrats. Siegfried Bernfeld was a socialist and a Zionist, and I think there was an issue about whether he would go to Palestine or to the United States. Max Eitingon went to Palestine, and he established the Palestine Psychoanalytic Society, which later became the Israel Psychoanalytic Society. When analysts immigrated to America, they did not want their radical politics to make things more difficult for them. Very few went to England, as the Freud family did, partly because Ernest Jones did not make it easy for people to come to England. Hitler came to power in 1933, and it became less and less possible for Jewish analysts to work and live in Germany. Those interested in the history of psychoanalysis between 1933 and 1945 during the Hitler years would enjoy *Psychotherapy in the Third Reich* (1985), by Geoffrey Cocks.

Some non-Jewish analysts also decided to leave, such as Martin Grotjahn of Berlin. Ernest Jones, who was then the president of the International Psychoanalytical Association, wrote to him and said, "Don't leave—we need some people to

be able to stay there and keep psychoanalysis going. Who knows how long Hitler will last, but psychoanalysis has to maintain itself even under difficult circumstances.”

Grotjahn wrote back and said, “I can’t do that. When my Jewish colleagues leave, I must also leave.” To which Jones replied, “In that case, don’t try to come to England, because I will make it very difficult for you here.”

Di Donna

In *Freud’s Wizard* (2007), Brenda Maddox claims that Jones was extremely influential in bringing Jewish analysts to England and to the United States.

Wallerstein

Preferably to the United States!

Di Donna

Some of the refugees went to South America. Was it that the United States was not seen as a place for those with either a Kleinian or a Marxist orientation?

Wallerstein

It was partly because it was easier to get a visa to go to Latin America, and partly because of the influence of Angel Garma, a Catalan. Before the Spanish civil war, Garma had had two college roommates: Garcia Lorca and Salvador Dalí. When the war broke out, Garma was away in Berlin, doing analytic training. Lorca was antifascist and supported the Spanish government, which fell to Franco’s forces; Lorca was captured and then executed. Dalí, by contrast, joined Franco and became a rightist. Garma—who was Catholic, not Jewish—decided he couldn’t go back to Spain after all these events, and instead went to Argentina.

In Berlin, Garma had been analyzed by Melanie Klein, I believe. He eventually became one of the founders of the Kleinian group in Buenos Aires. Buenos Aires was the first place in Latin America to have an analytic group, and so when people in other Latin American countries wanted to undertake training, they either went to Europe or to Buenos Aires. (Some

Mexicans came for analytic training to the United States.) It became the central point from which the Kleinian influence spread all over Latin America. Garma was a key figure in this, though there were others as well.

When analysts left Europe in large numbers in the late 1930s and '40s, about ninety percent came to the United States, about five percent went to Great Britain, and about five percent to other places around the world. A very good book, Russell Jacoby's *The Repression of Psychoanalysis* (1983), tells what happened with those analysts. When they crossed the Atlantic on ships, they figuratively threw their political feelings overboard like so much excess baggage, according to Jacoby. They felt it would be easier for them to be accepted in the United States if they were not openly political in any way.

You have to remember that 1933 to 1938 were years of the Great Depression, and many Americans resented the new arrivals, seeing them as competitors. There were notable exceptions, such as Bettina Warburg in New York, who spent a great deal of her own money to obtain visas and bring analysts to this country and help them get established.

Some analysts, such as Otto Fenichel, shared their feelings about political events by confiding in other émigrés through circular letters. He or one of the others would type a letter and send out six or seven carbon copies of it to like-minded colleagues from Europe; six or seven was the carbon copy limit in those days. The letters were full of their feelings about political events, which they didn't make public but which they confided to each other. They also documented their feelings about America and recorded psychoanalytic gossip of various kinds. It's all extremely interesting.

When they arrived here, I don't think any of them were really Kleinians; Klein's ideas didn't develop until the latter 1920s, when she was being trained and working in Berlin. She was beginning to formulate her ideas about child analysis along different lines from Anna Freud in Vienna. They represented competing schools of child analysis—the Berlin school and the Vienna school. Then, in 1927, Klein was invited to lecture in England because the Stracheys, who had gone to Berlin to be analyzed, got interested in her ideas about child analysis. So

she brought those ideas to London, and the London school became more and more Kleinian. Ernest Jones invited her to come to England permanently, saying she would immediately be able to practice and teach. His one condition was that she accept two of his children into analysis with her, to which she agreed.

But the full development of the Kleinian perspective didn't come until the late 1930s and early '40s. It emerged in full view in the *Controversial Discussions* (King and Steiner 1991), which took place in the middle of the war, so that nobody outside England knew much about them at that time.

Di Donna

Going back to Topeka, could you say something more about what it was like as a psychoanalytic milieu?

Wallerstein

Topeka was a salaried group practice. Everybody who worked there was on the payroll of the Menninger Foundation, including the training analysts in the institute.

Di Donna

Would they get paid based on their caseload of patients and candidates, or would they receive just a flat salary?

Wallerstein

There was a salary that was fixed according to seniority and experience. The usual fee for candidates at that time was fifteen dollars per hour. If you were a patient and were really affluent, you might be paying twenty-five to thirty dollars an hour, but the analyst's income did not depend on who their patients were.

At first I was on the payroll of the affiliated V.A. hospital, as a teacher in the Menninger School of Psychiatry, and also a candidate in the institute. I paid my fee—fifteen dollars per hour—to the Menninger Foundation each month. I had that job running the VA Hospital's psychosomatic service for two and half years after my residency. In my last year of residency, I worked on a ward for alcoholic patients. They were chronic

severe alcoholics who behaved very well while patients in the hospital. I established an alcoholism research project, which my colleagues and I wrote a book about (Wallerstein et al. 1957).

Gardner Murphy then came to Menninger as the new Director of Research, and he wanted to bring a psychiatrist into the research department. My friend and colleague George Klein, who later became director of the Mental Health Center at New York University, knew my alcohol book and recommended me to Gardner, who hired me.

So I left the V.A. hospital to come to the Menninger Clinic as the assistant director of research on a half-time basis, with the other half of my job being in the department of adult psychiatry, doing psychotherapy. At that time, I was a candidate in the institute, doing my supervised clinical analytic cases as well. As I said, your salary didn't depend on how much income you generated from your patients or your research grants; it depended on what the salary scale was for people of your experience.

In order to get enough training analysts at the institute, Menninger looked for refugees coming from Europe who were not yet established in America. He could offer them a full-time job, and they could practice as employees at the Menninger Foundation without a professional license. If they went to New York and private practice, they would have had to take licensure examinations and get a medical license.

I had two training analysts. One was Robert Jokl, who had been in Freud's circle in Vienna and had been analyzed by Freud. Several times during my analytic work with him he would remark, "At a moment like this, the professor would say to me . . .," and he would give me Freud's interpretation.

Di Donna

Did it fit?

Wallerstein

I felt that sometimes it fit and sometimes it didn't. Jokl didn't speak English very well. In fact, the candidate who saw him just before me, a good friend of mine, came out of his hour one day and said, "Fifteen dollars an hour I pay him, and I give him English lessons besides!"

When Jokl left to go to Los Angeles, I faced the question of whether to leave in order to continue my analytic work with him in Los Angeles or whether to stay in Topeka and transfer to another analyst. I decided to do the latter, and my second analyst was a Dutch lady, Nellie Tibout, who was not Jewish. She had been analyzed by Karl Landauer, who was from Frankfurt. He had started the Frankfurt Institute before the war. He fled to Holland when the war started. But when the Germans overran Holland he was captured. He died in a concentration camp.

Di Donna

What kinds of ideas did these analysts teach you?

Wallerstein

Well, it was initially all Freud's drive theory, modified and added to primarily by two books: Anna Freud's *The Ego and the Mechanisms of Defence* (1936) and Heinz Hartmann's *Ego Psychology and the Problem of Adaptation* (1939). Those two books became the cornerstone for the work that Hartmann, Kris, and Loewenstein were already doing in New York. They and others, including Edith Jacobson, were working in a strict ego psychology framework. That was the only legitimate analysis for the American Psychoanalytic Association in the 1950s and '60s. The hegemony of ego psychology wasn't really threatened in America until Heinz Kohut became popular.

Di Donna

Why do you think Hartmann became so prominent in the United States? Neither Freud nor Fenichel was filled with enthusiasm for some of his ideas.

Wallerstein

Well, Hartmann had the advantage of being in New York. Hartmann was admired by Kris, Loewenstein, and Jacobson, as well as by many other leading figures in New York. The central systematizing theoretician was David Rapaport. Rapaport attached himself to Hartmann in his theories, even though he never worked directly with Hartmann.

The largest psychoanalytic institute at that time, and the most important, was New York. For many years, the president

of the American Psychoanalytic Association was most often a member of the New York Psychoanalytic Society. When I ran for president of the APsaA in 1971 and was elected, I was congratulated for having helped to break the New York monopoly. They dominated American psychoanalysis. Once when I was APsaA president, I had occasion to write a letter to the chairs of the Education Committees of all the country's institutes asking for some information, and the letter I got back from the New York Institute chair said: "In response to your inquiry, when the New York Institute has something to say to the American, I will let you know"!

Di Donna

Was Merton Gill still a student while you were in Topeka?

Wallerstein

No. When he left Topeka in 1947, he was no longer a candidate. He went with Knight to Stockbridge, Massachusetts, and the group at Riggs and at Yale created a new institute called the Western New England. Gill over his lifetime wandered from place to place. He became prominent extremely early, and was very close to Rapaport, and a full proponent of his ego psychology views. Years later Merton Gill, Roy Schafer, and George Klein turned their backs on Rapaport and on his metapsychology, and they all went out in their own directions. If you look at Gill's early monograph, *Topography and Systems in Psychoanalytic Theory* (1963), in *Psychological Issues*, it is a full statement within Hartmann's tradition of the Freudian structural metapsychology that dominated everything.

When I was a candidate, we regularly read the *Journal of the American Psychoanalytic Association*, *The Psychoanalytic Quarterly*, and the *Psychoanalytic Study of the Child*. Only occasionally did we read articles in the *International Journal of Psychoanalysis*; although it was believed to have a few good articles, it also had many Kleinian articles and other crazy stuff that we should avoid. There were, also for instance, only two papers by Winnicott that we were supposed to read: the transitional object paper (1953) and "Hate in the Countertransference" (1949). Other than those two, we were to avoid Winnicott's writing.

Di Donna

When you went to New York, you must have met Winnicott.

Wallerstein

He came to Topeka also.

Di Donna

What was he like?

Wallerstein

Winnicott really seemed to me to be a very humble, shy man. I only saw him one time and never talked to him.

Many impressive people, of different disciplines, came to visit Menninger: Angel Garma, Aldous Huxley, Jean Piaget, Margaret Mead, Konrad Lorenz, Jock Sutherland of London. We would sit and listen to them in dialogues with Karl Menninger.

Di Donna

It's interesting that Sutherland was invited since he was part of the British object relations school that wasn't considered acceptable at Menninger. Could you talk more about why this theory was rejected?

Wallerstein

Well, actually, it goes back to the time of Harry Stack Sullivan, who was in his day a major figure in American psychiatry and psychoanalysis. He had this unit at the Sheppard and Enoch Pratt hospital where all the psychotic patients were in long-term analytic therapy. Sullivan began to develop his ideas there, and they were looked at as deviations from classical tradition. Although some of the people who followed him stayed in the American, most left and started their own institutes in New York and in Washington. Clara Thompson left; Karen Horney left; Erich Fromm left. Those who stayed included Harold Searles. The most prominent of the independent institutes became the William Alanson White, where there was the greatest acceptance of this object relational perspective, which didn't become important in American analysis until the book by Greenberg and Mitchell (1983) came out; they were both from the White Institute.

David Rapaport had a one-line criticism of Kleinianism. It was a little footnote in the first volume of *Psychological Issues* (1959) that was about Erikson's work with a foreword by Rapaport. Rapaport said of the Kleinians, "We have an ego psychology; they have an id mythology." At the time that was the prevailing view of Kleinianism.

Sullivan's interpersonal perspective was seen as moving away from the focus on the intrapsychic, and anything that did this was automatically excluded and seen as having Kleinian leanings. By the time of the publication of Greenberg and Mitchell's book, there were more proponents of the Sullivanian and Kleinian points of view, and these areas were no longer so marginalized by American mainstream psychoanalysis.

Di Donna

How did you see Merton Gill, who was also a very interesting and controversial figure?

Wallerstein

A small group of American psychoanalysts trained in the classical tradition became strong proponents of object relations theory: Merton Gill, Theodore Jacobs, James McLaughlin, Warren Poland, Dale Boesky, and Judy Chused, to name some of them. Merton was one of the first to advocate the object relations viewpoint. He was always able to change his mind—so much so that, when he became involved in the object relations movement, he said to me about his earlier monograph on classical structure and topography, "I wish I had never written it. I'm against every word in it!" This shift in Merton's thinking occurred after World War II; earlier he had supported the classicist view in opposition to those of Sullivan, Franz Alexander, and others.

Di Donna

After Gill's shift in thinking, did he reject Freud's ideas about infantile sexuality and even sexuality in general? This would be in contrast to André Green and other French analysts, who have stayed very close to classical metapsychology even though they accept object relations theory. Do you think

that psychoanalysis is purely a clinical theory, rather than a combination of metapsychology and clinical theory?

Wallerstein

I think that's true. I don't know how much Freud's sexual theory of neurosis played a part in the divergence of viewpoints. Green and some other French writers feel that Americans, and the British to some extent, have largely abandoned the centrality of sexuality in neurotic illness, and that only French analysis has stayed very closely connected to this basic tenet.

I agree more with George Klein that we should separate off Freud's metapsychology, considered by many to be grounded in nineteenth-century physiology and to represent a very positivist epistemological framework, and that we should give it up. Instead, we should concentrate on the clinical dimension and develop a more hermeneutic model focusing more on narrative. George Klein was going in that direction, and others who have followed it are Roy Schafer and Donald Spence.

Benjamin Rubenstein was another major figure who, differently, attempted to create a protophysiological model that constituted a modification of Freud's metapsychology. This new model would be a causal one based on physiology as understood in the latter twentieth century, and would have a significant place for a biological substrate.

A shift in American psychoanalysis was gradually occurring. In the 1970s, the British Kleinians began to be invited to present their ideas in the United States. The first to do so were Hanna Segal and Betty Joseph. Segal was seen as the last of the "old" Kleinians, and Joseph as the first of the "new" Kleinians. The earlier generation had consisted of Melanie Klein and the people around her. This second generation included Wilfred Bion, Herbert Rosenfeld, and Segal. The third generation, initiated largely by Joseph, includes John Steiner, Michael Feldman, Ron Britton, Edna O'Shaughnessy, Ignês Sodrè, Irma Beck, Eric Brenman, and Elizabeth Spillius. The signal trio today is Feldman, Steiner, and Britton.

Di Donna

I'm intrigued that Otto Kernberg immigrated from Chile under your sponsorship. How was it that you agreed to bring him to the United States?

Wallerstein

Otto was a young adolescent when Hitler came to Austria in 1938. His family obtained a visa to go to Chile, where he attended college and medical school, and then trained at a very strictly Kleinian psychoanalytic institute. He received a fellowship from the Rockefeller Foundation to come to the United States for one year to study psychotherapy research. He decided to go to The Johns Hopkins University to work with Jerome Frank, who was in charge of a major psychotherapy research program there. While in Baltimore he wrote to me, having heard about some of our research at Menninger's, asking if he could come and visit for a week.

I agreed to meet Otto in New York and I took him to dinner so we could talk about it. He remembers it as the best steak dinner he'd ever had! He was very bright, very energetic, very curious, and wanted to learn everything he could about psychotherapy research in various places. I invited him to come to Topeka for a week, which he did. We had a large project then, about twenty people, with five or six working groups doing different things. Each group met a few times a week and Otto went to almost every meeting of every group; he spent the whole week going from one meeting to the next.

At the end of the week, I met with him to get his impressions. He had absorbed the entire complexity of the project. He understood it well and had a lot of ideas about things he would like to see added or changed. I said, "Look, how would you like to come here and work on this project?" He answered, "Oh, I would love to, but I can't." When I asked why not, he said, "Well, I'm here on a Rockefeller fellowship to learn about psychotherapy research in America, but I have to go back to Chile for a year to teach the things I've learned as part of the fellowship agreement." I replied, "So come at the end of the year." But Otto said, "I can't do that either, because I'm here

on a visa that stipulates I have to go back and be there five years before I can come to the United States as an immigrant.”

I looked at Otto and said, “Look, I can’t wait five years. What I can do is talk to Karl Menninger and see what he can do.” Karl knew a lot of people in Washington.

So I told Karl about Otto and his visit, and how much I’d like to hire him, but also about the problem with that. Karl’s response was: “I have one question to ask about this man. Is he as good as you say he is?” I said, “He’s *better* than I say he is.” “Sit here,” Karl answered, “and I’ll see what I can do.” Well, Karl put in a phone call to Frank Carlson, a moderate Republican Kansas senator who was a friend of his. He said, “Frank, we have a problem here. We have a very gifted young man whom we would like to have at the Menninger Clinic for all kinds of reasons—for our clinical work, our research work, and would be a real credit to Menninger and to the state of Kansas. But he lives in Chile and can’t immigrate here for five years.”

Frank Carlson offered to arrange a private bill—a special law waiving a specific legal requirement for a single person and family. It took two months to be approved by the Senate, but by the time Otto was ready to go back to Chile after his year in this country, I could call him and say, “I’ve got it arranged: you go back to Chile, and I’ll have a job for you when you come back here in a year.”

He came in one year, bringing his wife and children and an *au pair* he had selected by Rorschach. He worked very hard and was very energetic. When I finally decided to leave Kansas and come here to San Francisco, Otto wanted to come with me. I had no job for him. I left him in charge of finishing our project, and later he went to New York. As you know, he has become a very famed analyst.

Di Donna

I’m very impressed by your book about that research program, *Forty-Two Lives in Treatment* (1986). Can you summarize some of the things you learned about clinical psychoanalysis, and about research itself?

Wallerstein

The book is based on a study in which there were twenty-one patients in psychoanalysis and twenty-one in psychoanalytic psychotherapy. They were all long-term cases; the ones in analysis were seen five times a week, with the others most usually twice a week. Almost all the patients continued in treatment for several years. Interestingly, the outcomes with the two modalities, psychoanalysis and psychotherapy, were less different from each other than we had expected. The results of the psychoanalyses tended to be somewhat more limited than initially anticipated, and the psychotherapies often accomplished more than we had expected, so in general the outcomes were overall really close.

I gave a paper, later published (1988b) in the *Annual of Psychoanalysis*, at a meeting that celebrated the fortieth anniversary of the Boston Psychoanalytic Society. When I outlined our project and its results, a number of people came to me afterward and said, "You've given us permission to be ourselves and say what we really do but wouldn't dare tell our supervisors. A courageous paper!" I didn't think it was courageous; it was the report of a research program. It has come to be looked at as a pioneering paper in terms of the state of the art of research methodology of its day. Now things are very different, of course.

Di Donna

In their 1947 book, Franz Alexander and Thomas French said something very similar. So your proposal to look at psychoanalytic ideas in more scientific terms wasn't really radical, but it wasn't part of mainstream psychoanalytic thought in the 1950s.

Wallerstein

You're right. No one paid any attention to Alexander and French because they were looked at as heretics. The idea of doing empirical research in psychoanalysis was not accepted very widely at that time at all. In 1957, the American Psychological Association had a conference on research in psychotherapy. They invited eight groups to present their work, and only two of them were psychoanalytic (ours and Gill's).

When I was given my job at the Menninger Foundation in 1954, I was thirty-three years old. Gardner Murphy had just come to Topeka as director of research. He was basically a social psychologist, not a clinician, and he wanted to establish a closer link between the research department and the clinical operations of the Menninger Foundation. He created a job, very much the same as the job Merton Gill had had when Rapaport was director of research: a psychiatrist in the research department, and that was now me. My charge was to create research that would link the research investigative activity with the clinical operation. A group of us worked together to create the Psychotherapy Research Project of the Menninger Foundation.

Di Donna

How many people were involved in the project?

Wallerstein

At the beginning, about ten. At its height, about twenty people, doing different tasks, most for only about six hours a week. Only one was full-time, Helen Sargent. I was half-time in the research department, and most of that time I spent with this project. The idea was to study the Menninger Foundation's clinical activity, which was psychotherapy and psychoanalysis, both outpatient and also inpatient. We began with two simple, though not simple-minded, questions: first, *what* takes place, that is, what changes occur in psychoanalytic treatments; and second, *how* do those changes come about, through the operation of what factors in the patient and in the treatment, as well as in the evolving life situation? In short, we looked at the changes—the *outcome* question—and then at how those changes come about—the *process* question. We wanted to study both.

Five or six people became the nucleus of the project. One of them, whom I co-opted as chief co-investigator, was Lewis Robbins. He was director of adult psychiatry and had control over the operation of all clinical services. He was not really a researcher, although he became one. The project was established as an official project of the Menninger Foundation, and everybody had to cooperate with it; it wasn't like working

with clinicians in private practice who have to be enticed to participate in formal research. We set it up to study patients when they arrived in Topeka for evaluation, before they began treatment, and at the end of treatment, as well as at a follow-up point at least two years after termination. We had hoped to make the follow-up five years after, just as cancer treatments are evaluated after five years, but we couldn't go on that long. We didn't know how long our money would last, although I was able to get a great deal of it. The first research grant we applied for was from the Foundation's Fund for Research in Psychiatry (FFRP). Merton Gill was one of the site visitors, and that was my first meeting with him.

I was the principal investigator and the chief spokesman. Characteristically for Merton, he asked very incisive, difficult questions, and you had no way of knowing from his facial expression what his response was. About a month after that visit, however, we received a letter saying that we had the money. Altogether we received a total of \$1,000,000 by 1954, from that grant, from the Menninger Foundation, the Ford Foundation, and the National Institute of Mental Health; today that would be about \$5,000,000. At that time there was a small window of opportunity with the Ford Foundation, in that they were funding research on therapeutic processes, but two years later they had stopped giving money for this kind of research. They explained, "We spent a few million dollars and we don't see any results"—although of course two years was too soon to see the results.

Anyway, we were well funded, although in those days there were a lot of things one couldn't do in psychoanalytic research. It was forbidden in many places to make audio recordings of therapy hours. That came much later—there's a whole history of its slow acceptance. At the time it was felt that it would interfere with the ongoing therapy and perhaps vitiate it.

So we created a project in which there would be no interference of any kind with the natural treatment as it usually took place. It would just go on as it always did. In those days you didn't have to get informed consent. We never had to ask permission; we just did the research, although we were guided by the ethical stipulation that you have to safeguard the inter-

ests of the patient, you should not do anything harmful, and you have to maintain the privacy of the patient. The patients therefore did not know they would be in a research project, and neither did the therapists. So there would be no awareness and nothing of the usual progression would be altered.

The way we did that was to set up an elaborate initial study, before treatment. The initial study came from the evaluation that was done at the Menninger Foundation. People would come to Topeka from all over the country and the world to be evaluated for a two-week period. They stayed in hotels, unless they were so ill that they had to stay in the hospital. They came with their family members—parents, spouses, or adult sons or daughters. During the two-week period, they would have a psychiatric interview every day, for ten hours altogether. They would be given an extensive battery of psychological tests devised by Rapaport. The people who came with them would also have to give a family history, every day, for two weeks, for ten hours.

All of this was recorded, and the transcribed material—the psychiatric interviews, the social history, the psychological test report—would be brought together at case conferences held at the end of the two weeks.

Di Donna

Was a diagnosis already assigned, or was one arrived at later on?

Wallerstein

In the case conference, there would be about ten people, led by one of the senior psychiatrists. This group would put together an understanding of the patient and recommend a treatment plan. In some cases, it would be for the patient to return to his or her home city and undergo outpatient treatment, that is, to go back to the therapist who had referred the patient. In others, it would be for hospitalization, or to stay in Topeka and be treated as an outpatient. Some of the patients came from small farming towns in North Dakota and would literally move to Topeka with their families, staying there for several years to be treated. They were of course the people who could afford to do this.

So, if the recommendation was to stay in Topeka and have psychoanalysis or psychoanalytic psychotherapy, the patient would be put on a waiting list and meanwhile would find a place to live. It was a huge commitment that the patients made. We decided to see as many of them as we could—half in psychoanalysis, half in psychoanalytic psychotherapy—by taking them as they came, with certain exclusions. You had to speak English well. You had to live in the United States, because we didn't want to have to follow up patients who had moved back to another country. You couldn't be so sick that you had to be hospitalized for a significant period, so you had to be able to tolerate essentially outpatient therapy, and you couldn't be on significant medication. Also, you couldn't be a member of our own professional community because that would raise privacy issues.

Di Donna

How many times a week was the psychotherapy?

Wallerstein

Psychotherapy would be once, twice, or three times a week, and it would not be short-term treatment. I would receive records of people who had had a full evaluation for mental illness and were staying on in Topeka for treatment. Usually, patients didn't remain on the waiting list for longer than four to six weeks before treatment could start because there were so many therapists. We would get a lot of written material, and from that we would construct our *initial study* of the patient. Nobody except me knew which records I had selected. The patient would go into treatment, and nothing research-related would take place as far as the treatment was concerned. According to the rules of the Menninger Foundation, every patient had to have at least a monthly written summary, a minimum of a single-spaced typed page. The ones who were being treated by psychoanalytic candidates in the institute had daily process notes, usually one or two single-spaced typed pages; these might cover a total of 600 or even 800 hours of treatment. Even when notes were only written monthly, they would still be voluminous after four years or more of treatment. (The Menninger

Foundation in those days had a very large secretarial staff that could handle that volume of dictation.)

At the Menninger Clinic, you had to notify a particular secretary each month about whether you expected any terminations within the next two months, so that they could plan for new patients to be sent to you. That secretary kept a list of the forty-two project patients on whom we had done our elaborate *initial studies*. When I got a signal that one of them was about to terminate, I would inform the therapist, "This patient, whom you've now been treating for three years"—or five years or whatever—"is one of the patients in the psychotherapy project." At that time, approximately 300 patients were in regular treatment, and it was known that there was a project and that about forty-plus patients were in the project, but nobody knew the exact number. The likelihood that any single patient was one of them was small: 1 in 8 to 1 in 10.

When I told the therapist that his or her patient was in the project, I also explained that we needed both the patient's and the therapist's cooperation in order to do the *termination study*. We admitted that the patient's involvement could be clinically contraindicated, but the therapist could not say merely that either the therapist or the patient didn't want to participate; rather, the therapist would have to demonstrate that it was indeed clinically contraindicated for the patient to be interviewed by the research group. We said we wanted all the written records made during the treatment. Then we wanted to interview the patient and do the whole battery of psychological tests again, and we wanted to interview the therapist as well. If the patient had been committed to the hospital for any period during the course of treatment, we wanted to have the hospital record and to speak to the hospital doctor. If there was a supervisor for the therapy, we wanted to interview the supervisor. In short, we accumulated as much material as we could at the end. If the patient could not be seen at that point, we still interviewed the therapist and the supervisor, and at least got all the written records.

We had tons of data. There were whole books of it. We had on average several hundred pages of data on each treatment. We would do a complete *termination study* of what happened

in this therapy. We would then say to the patient that we want to see you in several years for a *follow-up study*, because at the termination point the patients would usually be returning to their hometowns. "We have the money to bring you back, at our expense, to come be with us for a whole week." In almost every case, they were happy to come back because they could visit old friends, people whom they had gotten to know while they were patients in Topeka.

During the week of the *follow-up study*, we would get any record of treatment the patient had had in the meantime, and permission to talk to whomever he or she had been seeing, and we would do another battery of psychological tests. The whole treatment was called a "natural treatment." Lewis Robbins and I would regularly meet for ten hours each week and would study all the accumulated initial material. It took us about a month to do the *initial study* for each patient, which meant that we could complete about ten a year. The total number of patients, forty-two, had to be a multiple of six for technical research reasons, and so it took us four years to accumulate forty-two comprehensive *initial studies*.

The shortest treatment turned out to be only six months. The longest went on for eight to ten years. We would wait until each treatment finished naturally. Once we had been doing this for a dozen years, a handful of them were still in treatment. And so with them we did what was called a cutoff termination: we determined that the situation had stabilized, and we did the termination studies at that point with those patients, still in ongoing therapy.

It ended up that, over a twenty-year period, about seventy or eighty articles were published by different people involved in the project, presenting its findings. I was the author or co-author of about thirty of them. Six books were published from our work, of which my own, *Forty-Two Lives in Treatment*, was the last; in it I included a summary of the findings of all the other books. The project became known in the psychoanalytic world because of these publications in mainstream journals and the books. I became known as a major psychotherapy researcher, along with a few others, including my friend and mentor, Merton Gill. We saw each other regularly, in various contexts, over

the years and always dreamed of working together someday, but it was never to be.

We had three main consultants for the project. The chief methodologist was a very gifted woman, Helen Sargent, a psychologist at the Veterans Administration Hospital in Topeka. I offered her a job, full-time, in the Menninger Foundation so that she could become the project's methodology person. She had been trained in psychology by Carl Rogers. She was not psychoanalytically trained, but she understood empirical research and the philosophy of research, and she was our main person in that area.

Our two other consultants were John Benjamin and Wayne Holtzman. John was a very prominent analyst in the 1950s; he was doing long-term developmental studies similar to what Daniel Stern does now, although much earlier, as a member of the psychoanalytic community in Denver. He came to us regularly, two or three times a year for a week at a time. Since our project was going on for many years, we were treating it like a developmental project. John tended to wait until he had accumulated all possible data before he published any of his work, and one of the things I learned from his example was not to wait until the project ended to try to publish, so we began writing up our findings while the project was still going on.

The third consultant, Wayne Holtzman, was a professor of psychology at the University of Texas and director of the Hogg Foundation. He was an expert in statistics, and would come up two or three times a year for several days at a time.

At the end, when we were analyzing all the data, I was in Israel, giving a talk about it to the Israel Institute of Applied Social Research, which was run by Louis Guttman. Guttman was very excited about the data we had. He said, "You have wonderful data, but I don't think you have good methods to analyze it. I have wonderful methods, but I don't have your data. Can we come to an agreement?" I then sent four people—Otto Kernberg was one of them—from Topeka to Jerusalem for six weeks. We had money to do a great deal at that point. In six weeks, these four people were to learn the technique of "multidimensional scalogram analysis," which was Guttman's method for taking a small sample of data with many variables

and plotting it out graphically, using computer methods. Guttman had created this instrument with James Lingoes of Michigan. After Otto and three others took the data to Israel and spent six weeks analyzing it with Guttman, they published their findings.

Di Donna

How did Otto Kernberg's work on the project contribute to his understanding of the borderline illness?

Wallerstein

When I left Topeka in 1966, I left Otto in charge of finishing the data collection of the project, and he became the principal investigator. In studying the patients in the project, Otto formed his initial ideas about borderline functioning and wrote his first papers on the subject. These papers were based on the same patients whom I had written about in *Forty-Two Lives*. He disagreed with me about some of the interpretations of the clinical material, as I mention in my book.

Di Donna

When Kernberg's first book came out in 1975, I had the impression as a young student of psychoanalysis that it was part of a shift in American psychoanalysis that had been dominated up until that point by Hartmann, Kris, and Loewenstein. Kernberg brought to bear his psychoanalytic training in Chile; there was much about Kleinian ideas of projective identification and countertransference. Would you say that in American psychoanalysis at that moment a new paradigm was coming into being?

Wallerstein

Yes, you're right. Otto came to Topeka as a Kleinian analyst at a time when there were no Kleinian analysts in America, and in his writing he dedicated himself to creating an amalgam of ego psychology and a more relational point of view. He felt that the one person who had tried to do this in America before him was Edith Jacobson. Earlier, people such as Harry Stack Sullivan and Frieda Fromm-Reichmann had been bringing

an interpersonal approach to psychoanalysis. But they were viewed as deviants and not accepted within the mainstream. Otto was one of the first within the mainstream to incorporate a more object relational perspective by theoretically working out his understanding of how the ego, id, and superego—as structural entities that Hartmann had elaborated upon—were themselves created out of internalized object relationships. He had a major role in this, though I don't know how much that's been recognized as such.

Di Donna

I was quite surprised that Edward Weinshel and Victor Calef, in their 1979 review of Kernberg's book, didn't seem to like his ideas. As a student, I had the impression that they didn't understand what Kernberg was trying to say because Klein had not appeared on the American scene until that time. Can you explain why they took the position they did?

Wallerstein

Well, they had been brought up in the traditional ego psychology approach. They were very firm advocates of it and part of the group that did not want to see these Kleinian influences come into psychoanalysis. They got very angry at Otto's book and wrote a very negative review. They looked at it as a shift from the centrality of the oedipal to that of the preoedipal, and as though it were emphasizing object relations as more important than the unconscious fantasies within the id. It was a deliberate attack and a personal one. They didn't like Otto as a person, and unhappily when Otto wanted to run for president of the American Psychoanalytic Association, they were able to block this. He later became president of the International Psychoanalytical Association, but never of the American. It was partly political because he had these "terrible" ideas that could be traced to Klein.

At the same time, two other things were happening in American psychoanalysis. One was a rise in the recognition and support of Heinz Kohut. And the other was that a few British Kleinians came to Los Angeles. Finally, Wilfred Bion came to Los Angeles and stayed there for a while, and so the Kleinian influence in America began.

Di Donna

What do you think of Kohut and his writings?

Wallerstein

In his first book (1971), Kohut used the language of ego psychology to bring forth his ideas about the narcissistic character. By the time of his second and third books (1977; 1984), he had abandoned that language.

Di Donna

Hans Loewald, in his 1973 review of Kohut's first book, was not very complimentary. Can you tell us something about Kernberg and Kohut as people?

Wallerstein

Heinz Kohut was on the one hand a very gifted man, a brilliant man, a great Freudian scholar. If I was trying to locate a particular Freud quotation, and all else failed, I could always call Heinz Kohut and he would instantly tell me the article, the year of publication, and often the exact page in the Standard Edition. But on the other hand he was in many ways very difficult. His own narcissism was overwhelming, and he had many prejudices; he would easily take offense. He felt that Kernberg was too harsh in his criticisms, and he was unwilling to debate this difference with him. Kernberg, like Kohut, also had his narcissistic issues. Kohut had a kind of retiring manner, and Kernberg had a much more open and confrontational manner. Kernberg always wanted to debate with Kohut, and was very unhappy that Kohut was unwilling to talk with him. Kernberg would ask me, "How come he talks with you and he doesn't talk with me?" Kohut lost many friends because of the bitterness that arose in him when his views were initially so largely rejected. I was one of the few people in the American Psychoanalytic Association who were not self psychologists who he thought were personally friendly towards him.

Di Donna

What was Kohut's main influence on American psychoanalysis?

Wallerstein

I think Kohut influenced people like me clinically rather than theoretically. There are particular transference stances, and then there are the countertransferences that arise in response to them—all called by Kohut selfobject transferences and countertransferences. We had perhaps paid attention to these configurations earlier, but Kohut made them a specifically prominent feature and gave them a specific name. He created a real awareness of these selfobject transferences as separate from object-related transferences.

As far as theory goes, I differed with Kohut, who felt he needed to create the whole new theoretical superstructure, self psychology. But I felt he had made a major clinical contribution in locating the origin of these selfobject transferences in an earlier developmental period—preoedipal, not oedipal—and in elaborating their important place especially with narcissistic personality disorder.

Di Donna

What has been Kernberg's major contribution?

Wallerstein

Kernberg made one great theoretical contribution: seeing a way in which an object relational perspective and a structural ego psychological perspective could be amalgamated. He advocated paying attention to internalized object representations—meaning the self-representation, the object representation, and the emotional valence that connected them. He has continued to elaborate this idea. He first utilized it as a better way of understanding borderline patients, which I think is really helpful. He then applied it to analytic studies of groups and of aggression as a social phenomenon, creating a psychoanalytic corpus of sociological and ethnological significance.

Kernberg has also been one of the major critics of the psychoanalytic educational system. Critiques of analytic training date back to 1948, when Michael Balint first voiced his criticisms. Siegfried Bernfeld (1962) wrote in the same vein. Kernberg's critique has focused on the training analyst system, which he indicates becomes self-perpetuating and authoritarian, creating

either very conformist or very rebellious candidates. He calls for major reforms in this system. From my point of view, all that is important and he is correct, though when taken alone, his educational criticisms are too limited.

The whole educational system is wrong, I think, because ideally I feel it should be located inside a university. Historically, that was not possible. When Freud created psychoanalysis at the turn of the twentieth century in Vienna, his ideas about the sexuality of children were considered too scandalous. Furthermore, since he was a Jew and the entire Austro-Hungarian empire had a more or less official policy of anti-Semitism, he could never obtain a salaried university position, which was something he desired all his life; and in which he was always disappointed. Only once—during World War I, in 1916–1917—was Freud invited to give a series of university lectures in Vienna. Those were his *Introductory Lectures on Psychoanalysis* (1917). Many years later, his *New Introductory Lectures on Psychoanalysis* (1933) appeared, but only in written form; they were never delivered as lectures.

And so psychoanalysis was created not within the university or the medical school but as a private practice enterprise. It was a night school, carried by people who were working all day and who either taught or were students on their own time in the evenings. Thus, available energy was overall too low, and the men and women involved were often too tired; they used their clinical practices as their basic material. In the early days, the neurotic patients typically seen in analytic treatment were open hysterics. They were the ones whom Freud started with; they were not seen by psychiatrists in those days but by neurologists, because it was important to differentiate organically based symptoms from hysterically based ones. In fact, it was his differential diagnosis of hysterical illnesses that brought Sigmund Freud his first patients. Freud never really saw psychotic patients; they were treated by psychiatrists working in mental hospitals. So psychoanalysis came into being, working with the private patients of these new practitioners who, for training successors, created independent institutes that would have no academic base.

Di Donna

There was a time when Franz Alexander worked at the University of Chicago, although only for a very short time.

Wallerstein

When Franz Alexander came to Chicago in 1930 or '31, he obtained a major academic teaching appointment. However, Dallas Phemister, a professor of surgery who was chief of staff at the teaching hospital at that time, felt strongly that doctors should not accept fees from other doctors; he claimed that the Hippocratic oath impels doctors to treat their colleagues without charge. Phemister felt that Alexander was violating this oath by charging his analysands who were psychiatrist psychoanalytic candidates, and had him thrown out of the university after only a year or two.

Some years later, Alexander and some followers left Chicago for Los Angeles. Then Kohut gained prominence within the Chicago community. But the main figure at that time, the carrier of the ego psychological position, was Max Gitelson, who was chair of the department of psychiatry at Michael Reese Hospital. Gitelson was very involved in the fighting against Kohut and his influence.

Di Donna

Ralph Greenson was very interesting and controversial because of his book on technique (1967).

Wallerstein

I knew Ralph. He was an actor; his lectures could overwhelm an audience. Nonprofessional audiences would also be very taken by Greenson, and he was a charismatic speaker. He had been schooled in the full tradition of ego psychology by Otto Fenichel. Leo Rangell, too, identified Fenichel as his central teacher, but Rangell and Greenson were at odds with each other; both were struggling to be the dominant psychoanalytic figure in Los Angeles. There was bitter warfare that still has not completely died. Even today, if Rangell encounters Greenson's son—also a psychoanalyst—he turns around and walks away.

Di Donna

There was also a conflict between Herbert Rosenfeld, a British analyst in Los Angeles, and Greenson.

Wallerstein

I heard that story from Greenson, and I don't know what Rosenfeld's side is. What I heard is that Greenson contacted Rosenfeld and said, "You are a leading figure among the Kleinians in England, and I am a significant figure here in America among the Freudians. Let's try to get together—I will present my material to you, and then you present your material to me, and we'll see where we have similar or different ideas, and whether any rapprochement can be reached between Kleinian and Freudian perspectives." But Rosenfeld was an arrogant man according to Greenson; he responded by saying, "I'd be happy to have you come and present your material to me, and I will discuss it with you. I will charge you such-and-such for the supervision, but I have no reason to present my material to you because you have nothing to teach me."

According to Greenson, that angered him so much that there was no further contact. Greenson was very, very close to Anna Freud as a friend, and he also raised very significant money for her clinic. There were three prominent American analysts who worked very closely with her over the years, the other two being Douglas Bond in Cleveland and Al Solnit at Yale.

Di Donna

Arnold Cooper (2006) wrote that, when he asked which of your papers you considered the most important, you chose "One Psychoanalysis or Many?" (1988a). Is this paper an extension of Kohut's and Kernberg's lines of thought, or is your position a paradigm shift that looks at psychoanalysis from another perspective?

Wallerstein

Let me think about this. When you are president of the American Psychoanalytic Association, you have to give a presidential address. When I was president in 1971—forty years ago

now—I chose the topic of psychoanalytic perspectives on reality. I noted (1973) then that, as the ego psychoanalytic model came into being, there was a great deal of attention paid to the impulses of the id, the drives, and the instincts, and to the ego following the work of Anna Freud and Hartmann, and to the superego. Comparatively little attention was being paid to reality, psychoanalytically; we were taking it for granted as, in Hartmann’s words, an “average expectable environment.” But if we look at the model of the ego created by Freud, Anna Freud, and Hartmann, we see its interface with the id, with the superego, and with reality. In the interface with the id, the dominant affect is anxiety; you feel the inner impulses distorting the ego. With the superego, it’s guilt, and with reality, it’s objective fear. I started paying attention to the fact that reality is never just an average expectable environment; it, too, can be studied psychoanalytically. I wrote a number of papers, but somehow that work, which to me was promising and I hoped important, did not get that much attention.

Later, when I was president of the International Psychoanalytical Association, I gave my presidential address in Montreal in 1987. At that time, a controversy was stirring the American that we no longer had the hegemony of one point of view. We had Kohut, we had the Kleinians, and some people were paying attention to Lacan. I proposed that we should examine the topic of psychoanalytic pluralism from a serious perspective. We fight about our state of pluralism: some people say that it shouldn’t exist, and others that we had to live with it as the vitalizing state of affairs in our field. My position was that, because of our theoretical pluralism, we don’t pay sufficient attention to what we have in common. I said that in the face of our different metapsychologies—Freudian ego psychological, Kleinian, Bionian, Lacanian, Kohutian, interpersonal—we need focus on what we have in common that holds us together as psychoanalysts.

By the time of the IPA’s Rome Congress in 1989, there were enough references in the literature to this controversy and this address so that the question of “common ground” became the focus of the meeting. I wrote a paper (1990), my second presidential address, to the 1989 IPA Congress in Rome, supporting

more strongly that the psychoanalytic common ground was in our shared clinical theory and clinical practice. The theories of transference and countertransference, resistance and defense, anxiety, and compromise formation are all seen clinically; you see resistances and you experience transference and countertransference reactions. That's where our common ground is.

Where we don't have common ground, but instead have our diversity, is in how we explain these clinical phenomena theoretically. We explain them in terms of whole-object relationships and part-object relationships, or the depressive position and the paranoid-schizoid position, or in terms of the interrelations of the ego, the superego, and the id, or we explain them in terms of changing internal object relationships. We have different explanatory systems for the same phenomena, and the proponents of each system treat the phenomena to a large extent in very similar ways, although they explain what they're doing in different theoretical languages.

Di Donna

So you don't think there may be different psychoanalyses? André Green disagrees with your view that psychoanalysis is inherently pluralistic.

Wallerstein

André Green has a very French perspective, which is that psychoanalysis, created by Freud, is something unique, *sui generis*. Freud thought of it as part of psychology. Green says it's not psychology; it's totally separate. That's why he believes that observational research, "baby watching," may teach us about child development and growth, but doesn't tell us anything about psychoanalysis. But to me, psychoanalysis is part of psychology, although it's far from all of psychology.

Di Donna

In *The Freudians* (1989), Edith Kurzweil says that each country shapes a particular kind of psychoanalysis. Do you agree with that idea?

Wallerstein

To a certain extent I do. On the one hand, psychoanalysis and the human mind are basically the same all over the world. But it's truer of the French than of many others that they have a distinctive culture, and they see psychoanalysis quite differently. I follow Freud's original idea that psychoanalysis can and should be a science and that it should grow through research, as science does—which is why it should be in the university, in my view. It's not necessarily a natural science in the way that Freud often thought of it as a biological science. But I think we can study the mind scientifically, and we can do research on the way the mind functions. Green does not believe in the place of empirical research in psychoanalysis. The French have a very linguistic, philosophical perspective on psychoanalysis, which from their point of view has nothing to do with traditional science. Instead, they feel that we learn by increasing our conceptual understanding of ideas and of language, which will guide us toward the proper understanding of psychoanalysis. Formal research reduces psychoanalysis to what can be counted, destroying its essential spirit, and therefore teaches us nothing of psychoanalytic value.

Di Donna

What do you think of the American opposition to lay analysis, which European analysts favor? Freud felt that lay analysts are an intrinsic part of psychoanalysis and that they have made great contributions to psychoanalytic ideas.

Wallerstein

That issue, of course, has been one of long-standing controversy in the International Psychoanalytical Association, and the Americans had seen it very differently from the rest of the world. South America was a bit like the U.S. for a while, but one by one the opposition to lay analysis in South American countries was overcome. When I was president of the International, we had a lawsuit because the American was almost the only psychoanalytic association in the world that was officially against lay analysis. But at the same time, opposition to lay analysts in the American was dwindling. It was overcome partly

for economic reasons—there were fewer psychiatric analytic candidates—and partly because a new generation was changing its mind.

Di Donna

Why in American psychoanalytic thinking did there develop such a dislike for the structural model? Why did Jacob Arlow, Charles Brenner, Merton Gill, and George Klein all want to give it up?

Wallerstein

That's where I never agreed with that view of Arlow and Brenner. I felt the topographic perspective has its place, and the structural perspective has its place; we don't have to choose between them. Arlow and Brenner (1964)—some years back—said you've got to throw out the former and develop only the latter, though Brenner at the end of his career (2003) changed his mind and argued that the structural theory was no longer useful because all mental phenomena were simply compromise formations resulting from intrapsychic conflict.

Di Donna

Was this because in the topographic model there is more sexuality? Do you feel, as Green does, that we are retreating from Freud by ignoring infantile sexuality?

Wallerstein

Of all the French psychoanalysts, Green is the best-known spokesman in the American world. What they feel is that the structural model is wrong in two ways: it diminishes the importance of the drives and it diminishes the importance of sexuality. Green writes articles with titles such as "What's Happened to Sexuality?" The other objection is that it leads to a psychology of adaptation, and that adaptation means a superficial adjustment to reality. That was not what Hartmann meant, but Green and other French analysts reject the entire structure of ego psychology; they feel that Hartmann's understanding was a real disaster for psychoanalysis.

Di Donna

Why did Hartmann's influence last so long in American psychoanalysis?

Wallerstein

When Hartmann came to America, he was looked at as the most important of Freud's followers here. He was a teacher at the New York Psychoanalytic Institute, which saw itself as the leading analytic institute in the United States, and Hartmann became its intellectual leader. Hartmann was quite a charismatic figure, despite many difficulties. One of them was that his writing was very complex, hard to read, and contained almost no clinical material. You could read one paper after another by Hartmann and think he never had a patient. That's so different from the British Kleinians, who always start with a case no matter how theoretical the article may be. Loewenstein and Kris were associated with Hartmann, and they were easier to read; they offered more clinical context. The three of them were powerful in the New York Institute, where they were surrounded by other important figures. Edith Jacobson was certainly one, though the others are less well known today: Lillian Malcove, Edward Kronold, and Max Schur—a whole generation. They dominated things. Their successors, among others, were Arlow and Brenner, who went to the logical extreme, they thought, of throwing out the earlier topographic model and building only around the later structural model.

Di Donna

My impression is that you write as a historian, and I wonder why history plays such an important part in your work.

Wallerstein

I regret that we forget our history, that today's generation does not know the people who were major figures in the past. Consider Erik Erikson, who at one point was the best-known psychoanalyst in America and, perhaps, in the world. He had a great influence on the college scene; his courses in human development were given at Harvard and copied everywhere. Now he's almost forgotten. You can hardly read a paper today in which the author refers to an article he wrote.

Di Donna

A few last questions. Where do you think psychoanalysis is going? And where would you like to see it go?

Wallerstein

First, as a profession and a way of making a living, I think psychoanalysis is going to be much more difficult in the future than it was for me. I grew up in an era when everything was expanding, where there was no problem getting patients. For example, during the seventeen years I was in Topeka, I would regularly go to the meetings of the American Psychoanalytic Association and I would meet colleagues who had left Topeka to go to Los Angeles, New York, or Seattle, and they would say, "All you need to do when you decide to come is to make a trip in May for three or four days, and we will have patients, people waiting to be interviewed and to begin analysis, when you arrive in September. You will have a full practice the day you come." You could earn an income pretty much equal to what internists and pediatricians made, although never as much as surgeons would make.

Nowadays, there are many fewer patients, and many fewer people can pay what are called full fees. People who want to make a living doing this type of clinical work will have to realize that there will be fewer analytic patients, that they will have to live much more with psychotherapy once or twice a week, and they will have to accept a lower fee structure. They will no longer be able to live like other doctors, and those will be the conditions, as I see them, for the next twenty or thirty years. In that sense, it's no longer a golden avenue to such an affluent living as it once was. As an intellectual discipline, however, I think psychoanalysis is in a very exciting period.

Di Donna

How so?

Wallerstein

There's a struggle among ideas in psychoanalysis, an effort to develop each metapsychological paradigm, with much focus on convergences and divergences. It all points to the pos-

sibility of a coming together—perhaps the creation of a more overarching and encompassing theory. There are a lot of ideas in the literature that are struggling to converge. One perspective that remains still very divergent is the Lacanian one. Who knows, though, what will happen in the future? There's a rich tradition there of linguistic studies, and there have been a few people in psychoanalysis who have been very good linguistic scholars, such as Ted Shapiro and Victor Rosen.

Anna Freud said strongly that there is no other discipline that is both a serious intellectual one and a therapeutic one, that tries to do its training on a part-time basis. Like any serious study, it should be full-time, and the fact that we, for historical reasons, still meet at our part-time educational gatherings at night, she said, is analogous to modern churchgoers still doing what the original Christians did, reciting their prayers secretly in a catacomb.

As I said earlier, I think psychoanalysis should be in the university, where it could develop linkages with other disciplines, including biological ones such as neuroscience, but also with the social sciences and the humanities. But that will be very difficult for a number of reasons. Psychoanalysts are wary of the university because they feel that they might lose control of psychoanalysis. Control means basically that only we, the trained analysts, know enough about psychoanalysis to pick our successors, and only we who are the presiding faculty can pick the new faculty. But in a university setting, the department of medicine doesn't choose the new faculty; the dean sets up a (often interdisciplinary) committee. There will have to be some kind of accommodation. Not all universities would welcome psychoanalysis, but some would.

And then, of course, there are all the fiscal issues involved in bringing financial support for psychoanalysis and its training structure within the university orbit.

Di Donna

Is there a fear on the part of universities?

Wallerstein

Well, if there is, it is partly because psychoanalysis maintains that even if it comes into a university, it's still a uniquely

separate discipline and must be treated as such. And the university says it can't be such a distinctly separate discipline. To give an analogous example, there are Marxist economists. And Marxist economists can get positions in economics departments. But they don't have a separate department of Marxism. For the same reason, we don't have a separate psychoanalytic discipline; we are part of psychology, part of psychiatry, part of the humanities and of the social sciences.

Di Donna

What would psychoanalysis in the university be like?

Wallerstein

Well, it would be quite difficult to work it out. It will be a new psychoanalysis. It will be founded on the principle that not only does psychoanalysis have something to teach anthropologists and other social scientists, but it can also learn from those disciplines. The biggest problem will be how to finance it because right now psychoanalytic training is very expensive, and people pay for it by earning a living concomitantly. In the university they would be required to be full-time students for years while not having the opportunity to be earning all that much.

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