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THE "10/40" CELEBRATION

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EDITORIAL POLICY

The purpose of the Editorial Board is to publish a quarterly bulletin that will reflect a high level of scientific discourse in the field of psychoanalysis. While particular emphasis will be directed toward the psychoanalytic situation in Los Angeles, contributions from other national and international sources will be welcomed and encouraged. The editors will consider papers dealing with theoretical and applied psychoanalysis, reviews of psychoanalytically relevant books, reports of scientific meetings, essay reviews, brief communications and letters. Materials can be accepted for publication only on condition that they are contributed solely to the Bulletin.

All opinions expressed in the Bulletin are those of the authors and do not necessarily reflect those of the Los Angeles Psychoanalytic Society and Institute or its officers. All manuscripts, letters and business communications relating to the Bulletin should be sent to the Editor, Los Angeles Psychoanalytic Bulletin, 2014 Sawtelle Boulevard, Los Angeles, California 90025. Manuscripts should be typewritten, double-spaced on 8½ x 11 paper.

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"10/40" CELEBRATION
AN INTRODUCTION

by Lee W. Shershow, M.D.

On Friday, June 6 and Saturday, June 7, 1986, the Los Angeles Psychoanalytic Society & Institute held a reception and a scientific meeting to commemorate our fortieth anniversary as a Society and Institute and the tenth anniversary of the LAPSI building. The five papers presented at the scientific meeting and the summaries of the afternoon discussion will be presented in this *Bulletin.*

The committee responsible for organizing the celebration faced an arduous task in trying to adequately summarize our long and colorful history. We quickly discovered that the history of the Los Angeles Psychoanalytic Society and Institute was complicated, at times controversial, and that many issues still arouse strong opinions among our members. The committee became impressed with the strength of the opinions and reactions among our membership about certain issues and personalities, not simply present issues, but many past ones as well. We tried to offer an objective, non-controversial view of this history in the presentations, with the goal of informing our younger members about our exceedingly rich and interesting heritage.

The 10/40 Celebration Planning Committee hopes that these presentations will contribute to a renewed interest in the history of psychoanalysis in Los Angeles, help us all to understand our difficult and exciting past, and clarify our thinking in the future.

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"10/40" CELEBRATION
THE FOUNDERS

by Lee W. Shershow, M.D.

I have the honor of presenting the first section on The Origins of the Los Angeles Psychoanalytic Institute. My information is derived from the Oral History Committee of LAPSI which worked from 1960 - 1965 to interview the founders of the Los Angeles Institute. Dr. Albert Kandelin was the Chairman and deserves a heart-felt thanks from all of us because if it wasn't for him and the work of his Committee, this information might never have been formalized. The actual interviewers were Arthur Ourieff, Robert Stoller, and William Horowitz. The following people were interviewed: Margaret Monk, David Brunswick, Mr. & Mrs. Jerome Lockenbrook, Frances Deri, Hannah Fenichel, Charles Tidd, Ralph Greenson, Sam Sperling, Richard Evans, Ernst Lewy, Larry Friedman, Sam Futterman, Carel VanDerHeide, and Diana Howard Atkinson. The latter was Ernst Simmel's secretary in the 1940's and later the secretary of the Society/Institute.

The roots of analysis in Los Angeles I discovered really go back directly to Freud himself as well as to other European analysts in the centers of Vienna and Berlin; and to the American Institutes in New York, Chicago, and Topeka. The early study group started in 1927; there was a brief report earlier of a woman named Mary Wilshire (Wilshire Boulevard is named for her husband or family) who was a Jungian analyst who would greet her patients in flowing Greek robes, or something like that, around 1925. But then she disappeared. Our actual study group, which hopefully was a bit less kinky, was formed by Margaret and Thomas Libbin. But we knew her as Margaret Monk. They'd both received training in Vienna and settled in Los Angeles, apparently because Thomas Libbin was a graduate of Stanford, liked the West Coast, and liked Los Angeles. You always wonder when you read things like that — what if he'd liked Boston or something? History would have been changed. Anyway, they were here starting in 1927 and they gathered around them an early informal study group that met in their home. Anyone bold enough to express interest in Freudian analysis in those days was welcomed. There were many disciplines represented: psychiatrists, psychologists, social workers, physicists, anthropologists, lawyers. In 1930 David Brunswick came to Los Angeles. I was impressed with the subtle, but very important role of David Brunswick in getting an institute started here. He was someone I never knew but I think that he was really one of the unsung heroes of our Institute. He'd been trained in Vienna, analyzed by Freud himself. He doesn't say much about that. His sister-in-law, as you probably all know, was Ruth Mack Brunswick. It was apparently David Brunswick's early vision to organize an analytic institute here. In 1932 two more lay analysts came: Estelle Levy, trained in Vienna where she had known Dr. Brunswick, and Marjorie Leonard, who had trained in Berlin. By 1932, there were five trained analysts:

the Libbins, Dr. Brunswick, Estelle Levy and Marjorie Leonard. They were all lay analysts and felt a need, as you'll understand, to have more and more contact with other analysts. At this same time, of course, the conditions in Europe were deteriorating because of the Nazi influence. Parenthetically apparently a lot of the analysts were not only Jewish but they were also left wing politically so that it was getting hotter and hotter for analysts. It was like three strikes against you: you were Jewish, you were left wing, and you were an analyst.

With their strong desire to bring new analysts to Los Angeles, the study group decided to sponsor whoever they could. They conferred in 1934 with Alexander who was then in Chicago, and with Sachs who was then in Boston, and invited Ernst Simmel to Los Angeles. Now Simmel, again, was somebody I hadn't heard much about, but a most interesting person. He'd been a prominent analyst in Berlin, one of the co-founders of the Berlin Institute, and headed a psychoanalytic sanitarium called Schloss Tegel, a suburb of Berlin. Simmel had been encouraged by A. A. Brill in New York to come to L.A. and "put it on the psychoanalytic map." Simmel was the first medical analyst in Los Angeles.

In 1934 Mr. and Mrs. Lockenbrook came. They were also trained in Vienna, knew the Libbins and David Brunswick. They later would be very helpful to the European analysts, teaching them English. Otto Fenichel dedicates his book, *Psychoanalytic Theory of Neuroses* to her.

Also in 1934, Charles Tidd began his training, the first native American to do so. He began a training analysis with Simmel, although he later went on to Topeka and finished his training there from 1936-1938.

By 1935 then, the Los Angeles Group was getting larger and did become a formal psychoanalytic study group under the jurisdiction of the Chicago Institute. Simmel was President, Margaret Monk Libbin was secretary, and David Brunswick was really sort of the assistant to Simmel in writing By-Laws, etc. They would meet in Simmel's home on Hudson Avenue. Seminars included case seminars for the practicing analysts and general psychoanalytic seminars for the other disciplines — social workers, educators, etc. Simmel also wanted to sponsor a sanitarium based on psychoanalytic principles like the one at Schloss Tegel. His idea was that everybody should be analyzed, even the janitors, nurses . . . everybody. How well it was done, I don't know, but it achieved some prominence until the Nazi interruption.

In order to give you some idea of this man, Simmel, let me read you some of Manny Lippet's notes:

I was an advisor to Ernst Simmel going back to 1937 when I was elected to the Study Group. From then on I was involved in almost all of the day to day events that ultimately insured the establishment of this Society. In those intervening years there were several major issues that confronted Ernst Simmel. First was the problem of the refugees, both psychoanalysts and

friends who were seeking asylum in the United States. He engaged my cooperation in vouching both to the Immigration Department and to the State Department that none of the people we were sponsoring would ever become dependent on American support. The second problem facing Simmel was the number of non-medical analysts that were coming to the West Coast. As a result there developed tensions within the American psychoanalytic community. There were apprehensions that Simmel was encouraging too many lay analysts to settle in California. Simmel negotiated an agreement limiting the acceptance of lay analysts to those who had already settled here, but in the end no one was happy with the solution. Third was Simmel's desire to build the first psychoanalytic clinic in America, similarly to the one he founded at Schloss Tegel. He excited my imagination by describing the events that occurred at Schloss Tegel, foremost of which was the annual visit by Sigmund Freud to support Simmel's contribution to world psychoanalysis. To start such a clinic we needed American know-how and money. Dealing with the former, Simmel arranged for me to visit the Menninger Clinic where I spent a considerable amount of time researching the early history of Menningers with the object of utilizing their experience and to apply that to the California scene. Dr. Karl Menninger was particularly gracious and informative; he instructed his people to share with me any information that would help Simmel. From Topeka I went to Chicago and had a long interview with Franz Alexander. From there I went to New York and had a similar meeting with Dr. Kubie — Lawrence Kubie and other analysts. Upon my return to California and in cooperation with David Brunswick I published a small pamphlet on my research and how it could be applied to Los Angeles. A fund raising campaign was planned, but unfortunately the outbreak of the Second World War and the imminent involvement by the United States deflected interest in the clinic.

In reciting the history of Simmel as I knew him, one must deal with his often mentioned naivete and gullibility. Simmel was indeed a trusting man and it was inevitable that he trusted some persons more than their worth. In assuming a leadership role which he transferred from Berlin and Schloss Tegel to America, he was confronted by a new culture, by a legal system that was new and strange to him, and by his slow discovery that he had responsibilities alternately to the Chicago Institute, to the Menningers and to the American. In my view his mistakes were minimal; his contributions enormous. He was a pillar, if not the architect, of the foundation of analysis on the West Coast.

To give you an idea of the kind of controversies Simmel faced is the famous Louie Montgomery situation. There was a man named Montgomery who appeared on Simmel's door step and said he had a letter from A. A. Brill and was an analyst. Simmel was, apparently, a naive man and said fine, and Montgomery went into practice. Then, to make a long story short, it turned out that Montgomery wanted to train people to be analysts by having them hide in the closet of his office while he was doing analysis. That was his idea of supervision; so they would hear how he did it. When word of this got back to Simmel and Brunswick and people like that — of course they got very upset

and Manny Lippet was enlisted to run him out of town. Fortunately Montgomery disappeared — I think, Leonard, you told me he was a hair dresser, in fact. So, any of you who hide your Clinical Associates in your closet, don't do it — it's not good technique. Simmel then called Brill and Brill had never heard of him, but it didn't occur to Simmel in the beginning that people would do things like that. And some people have suggested that it might have accounted for a little more paranoia or wariness in Simmel later on.

In 1936, Frances Deri came. She's another woman who was enormously important in our history. She'd been trained in Berlin and had been analyzed by Abraham and Sachs. Again, there's Abraham and Sachs to Frances Deri to here and the people she trained. She'd also known Simmel in Berlin at Schloss Tegel and had assisted him there. When the Nazis came to power she too had to leave. She first went for two years to Prague where there was a psychoanalytic institute, and then came to Los Angeles in 1936. When she left Prague, Otto Fenichel took her place in Prague and then two years later in 1938 he, too, came here. So, you see the link between Simmel, Frances Deri, and Fenichel. There was an interesting note from Mrs. Deri:

> *I wanted to go to the United States and I wrote that to Hans Sachs who was then in Boston. And he wrote back and said, "I can give you the names of two cities. One is very ugly but you can earn, very quickly, very much money. That is Detroit."*

Thank goodness. If that had been L.A. I wouldn't have read you the quote. The quote continues:

> *"The other is very beautiful. You can't earn so much money so quickly, but you'd live in a beautiful spot." So I chose. I chose Los Angeles, of course, and I've never regretted it.*

So, you see, we don't make any money here! That was 1936.

In 1938 a number of important events occurred. Otto Fenichel came. What an extraordinary leader he was. Everybody respected him, and he was well liked. He was from Vienna. His first analytic paper was published when he was 19. He then went to Berlin, then to Oslo, Norway, for two years. Eventually he went to Prague, when Mrs. Deri left, then to Los Angeles. When he came here he immediately began training some of the candidates and that's where the expression "the Fenichel boys" came from. The original four candidates were Ralph Greenson, Sam Sperling, Norman Reider, and Robert Newhouse. The training fees were apparently somewhere around $5-$8 a session, which I think we should probably reinstitute!! Well, maybe not! Dr. Henderson and Dr. Richard Evans also became candidates. Richard Evans had been an internist, I believe, from Santa Barbara; his wife is here, Betty Evans. She, too, was in the Study Group and was very interested in children. Betty Evans became instrumental in forming a psychoanalytic-oriented nursery school in 1939, The School for Nursery Years, which I believe some of you attended. After the war it became the Center for Early Education, headed now by Estelle

Shane. In 1938 Hanna Heilborn came. She was an analyst from Berlin and had been in Prague with Mrs. Deri and Fenichel. By this time, there were enough analysts on the West Coast to form a Topeka Institute; and interestingly enough Simmel and Fenichel were Charter Members of the Topeka Institute, as was Charles Tidd who had done some of his training there. Now the jurisdiction for the L.A. Group was transferred from Chicago to Topeka.

Finally the American Psychoanalytic decided to officially not train any more lay analysts. There was a grandfather clause for the European-trained lay analysts, but no more.

In Dr. Greenson's interview he described some memories of the analytic seminars of 1939. They were held in Simmel's home. I guess there were not too many of them because they could all fit into one room. Karl Menninger was around a lot because he apparently married one of the members of the Study Group, Jeanette Lyle. There were memories of summer visits by Lionel Blitzin. Greenson remembers that Fenichel would discuss "Three Essays in Sexuality" sentence by sentence — and take a whole year for one paper. Dr. Greenson's first control case was in '39 and the fee was $1.50 an hour. Interesting.

In 1940 May Romm came, who had trained in Berlin and New York. There was also a scandal of sorts in 1940 when Otto Fenichel suddenly announced he'd married Hannah Hielborn, hence we know her as Hannah Fenichel. In '42 the San Francisco Institute was founded. Fenichel and Simmel were Charter Members. And the L.A. Study Group was now placed under San Francisco. I believe Dr. Greenson became the first candidate to graduate from San Francisco.

But then the vicissitudes of World War II came and analytic training was interrupted during the War. From 1945 on, because there was this center here and because people wanted to come to the West Coast, there began to be this rush of people coming for training. Among the people who arrived in '45 was Ernst Lewy. He had been trained in Berlin, New York, and Topeka where he was the Dean of the Institute. Sam Futterman, trained in New York, came. Norman Levy and Milton Miller, who had been trained in Chicago, came. And Fredrick Hacker. In '46 a few more people: Walter Briehl, Arthur Frumkes, Albert Slutsky, Judd Marmor, and Leo Rangell. Carel VanDerHeide, an interesting man, was a neurologist in Holland, trained in Vienna and Chicago, and then settled here. And Lincoln Rahman, who was trained in New York. His wife, also an analyst, Ruth Jaeger Rahman, was from here. They met in New York, got married, then they both came out to L.A. after the war.

There was a major tragic event in '46 with the sudden death of Otto Fenichel. He was only 48 at the time and many people have said that our history would have been different if he had lived. He was a vibrant, alive person. In '45, he

decided to undertake a medical internship. He was a European trained doctor, but he felt his credentials and his acceptance would be improved if he did the internship. And in January 1946 he suddenly died. He'd just finished his book, *Psychoanalytic Theory of Neuroses*. I'd been told he died of a heart attack, but Richard Evans said he saw him on the morning of his death and he died of a ruptured congenital basal aneurysm. Whether the stress of the internship contributed, we don't know. Simmel was also ill and would die in 1947. Their deaths created a real power vacuum.

By February 1946 there were enough medical analysts here to petition the American for an institute. On May 26, 1946, the Institute was founded. Let me read the Charter Members: Walter Briehl, Ralph Greenson, Fredrick Hacker, Norman Levy, Ernst Lewy, Milton Miller, Robert Newhouse, Lincoln Rahman, Norman Reited, Mae Romm, Ernst Simmel, Albert Slutsky, and Charles Tidd.

THE EARLY YEARS

by Leonard Rosengarten, M.D.

When you begin thinking about the past you kind of fall in love with your memories and each new one looms more wonderful than the last. Cutting any of your memories — and any analyst worth his salt can go on endlessly — that's the tough part.

The fine work of the history committee headed by a dedicated Albert Kandelin and peopled by Bill Horowitz, Arthur Ourieff, Ernst Lewy, and David Brunswick is wonderfully informative and is available to view at the Institute office. But I'm not going in that direction today.

I know of no way to tell you of these early years except with a very personal viewpoint. I couldn't possibly have known the details of the founding of the Society and the order in which the initial candidates appeared on the scene. Most of us were just returning to civilian life after our stretches in the military service. I came here in May of 1947 — not quite 29 and I may have been the youngest candidate, though my psychiatric and service background had been rather extensive.

Drs. Otto Fenichel and Albert Slutzky had died in 1946; Dr. Ernst Simmel was still active though not entirely well (he died in November, 1947). The loss of Simmel and Fenichel was almost more than this young organization could cope with. The leadership of Ernst Lewy as Dean and Director of the Institute saved the day for this mourning, hobbled group.

About the Society I knew or heard very little, and except for the monthly scientific meetings, as well as the meetings of the psychoanalytic study group, there was nothing here for me but "The Institute."

There were candidates all over the place and the numbers seemed to double every eight or nine months — though that may be an exaggeration. With no particular order intended and just as they come to mind, they were:

Larry Friedman
Eugene Mindlin
Bert Spira
Jack Vatz
Lyman Harrison
Seymour Pastron
Harry Nierenberg
Carol Carlson
Alex Blumstein
Leonard Rosengarten
Rocco Motto
Adio Freeman
Mel McDowell
Rose Fromm
Avon Remington
Milton Lester
Alfred Coodley
Leo Rangell
Sam Sperling
Ruth Jaeger
Albert Held
Alex Rogowski
Mark Stone
Mitch Rosow
John Moegener
Max Hayman
Jack Abrams
Phil Soloman
George Wayne
Ellis Toney
Bill Rosanoff
Jack Lomas
Dan Siegel
Sigmund Gabe
Lew Fielding
Roberta Crutcher
Albert Kandelin
Jerry Shiell
Sam Braunstein
Helen Tausend
Bob Berns
Max Sherman
George Leventhal
Esther Bogen-Tietz
Abe Gottesman
Matt Ross
Jim Rankin
Iz Zifferstein
Henry Luster

And at least a couple of anonymous sons of bitches whose names have escaped me. My apologies, nothing personal. They are no less important than the ones I remembered. Dammit! Elaine and Bob Lince and Purcell Schube, the Pasadena contingent. You know there are some persons like our secretaries of recent years who believed that Schube was a myth, but we old-timers knew better. He sat in seminars with us.

Not all of these people made the grade. A number dropped out, a number were dropped out, a number died, but each played his or her role in our history just like the men I had known in the army.

Speaking of secretaries, there were Ann Barzman, Virginia Smith, and Diana Howard. Ann worked for Dr. Lewy, Virginia for Dr. Tidd, and Diana for Dr. Simmel until the Institute had need of a full time secretary and Simmel had died. Ermalene Yerkes and Jean Kameon came to us in the mid fifties. And of course our librarian, Peter Tararin — dedicated and capable — and indispensable.

"There never was a child but has hunted gold, and been a pirate, and a military commander, and a bandit of the mountain, but has fought and suffered shipwreck and prison, and imbrued its little hands in gore, and gallantly retrieved the lost battle, and triumphantly protected innocence and beauty."

That's Robert Louis Stevenson and one of my favorite passages. In part it's appropriate to our first fifteen years — even that innocence bit.

I started seminars early in 1948 — where? Appropriately enough at the school for nursery years, three nights per week. We often had to sit on the tiny kiddy chairs as there were not enough of the old, uncomfortable, folding chairs to go around. We really were kinda "rinky-dink" at that. I had no idea of any problems or lack of organization when I first became a candidate. I

accepted tacitly and blindly anything that was offered. I was so happy to be here and a part of it.

I knew nothing of the war-like rumblings in the Education Committee — to the credit of my analyst, Ernst Lewy, who never mentioned the problems. He just analyzed.

When I first started seminars we had no books — there had been a wartime paper shortage, but Drs. Simmel, Brunswick, and Fenichel had translated certain special papers from German into English and mimeographed them. The lack of books or facility did not dampen our enthusiasm — enthusiasm was what characterized the first four years. We studied whatever we had to study and we gladly sat in any chair we could find.

All of us, instructors and candidates, were hungry — hungry to learn after five years of war and little else. In late '48 and '49 we had our first office and seminar room on Commercial Center Street right next to a mortuary. We had captain's chairs and books and — enormous seminars. I had never seen a more friendly or welcoming group of people with delightful senses of humor — well, why not? We were all finally doing what we wanted to do to develop our life's work.

For instance, one of the instructors decided he wished to give a seminar on hysteria or compulsive-obsessive neurosis (Van Der Heide was said to have demonstrated it as well as having taught it). Everyone — in all stages of training, attended. At the end of the year a critique was made of the course and this ritual rapidly transmogrified into a brilliant display of wit on the part of the candidates who for the moment fancied themselves to be comedy writers.

The younger candidates learned from the vigorous exchange between instructors and those candidates who had had prewar psychoanalytic training. This kind of mixed seminar helped us to develop a depth and philosophy in our work. I wouldn't mind a reprise — anytime.

They knew a great deal, those older candidates — Gene Mindlin who supervised my psychotherapy at the Mental Hygiene Clinic, Sigmund Gabe was also a staff member there. Then there was Leo Rangell with a big, warm, friendly smile, spontaneous, perceptive as hell, incisive, and very sharp. Leo was most impressive. Anyone who couldn't predict a big future for him was simply blind. I have been proud to be on a first name basis with a President of the American Psychoanalytic Association and the International, as well as a man who has created a formidable list of good publications. Leo's presence lent a hefty touch of class to the "rinky-dinks."

Sam Sperling, one of the Fenichel boys, witty, droll, and dry — as acute a thinker as I had ever met in medicine, fearless, articulate, and frequently the only person not to be taken in by the charm of the speaker. Sam was never afraid to shoot down a phony — with words, of course.

Lew Fielding knew everything ever written in psychoanalysis and seemed quite capable of quoting it verbatim. What was truly most remarkable was his perfect timing — just when the debate needed a sensible voice.

Larry Friedman was self-confident, wise, wonderfully warm, informed — he had pipes to the inside — and he knew more than Fielding. In fact at first I called him Dr. Friedman because I thought he was a training analyst. He talked a lot, but then he had a lot to say — and I listened. Larry was truly a Renaissance man.

Honestly I am not certain that Larry was not born a training analyst. Any man who was later to write so eloquently as "It is human nature to bare pain alone, but to need company for full pleasure" was a good model.

When these guys spoke — boy! I thought, "I gotta' be as good as they are." Never made it! But in striving for that goal, I found my own strength and made my contributions in my own way — as my being here on this special occasion must in some manner attest.

And Jack Harrison seemed to take me under his big wing and help me to get started both socially and in practice. We officed together for nearly 15 years. He was my special friend and the one I talked to when my head was bloodied. I shall miss him very much.

Seymour Pastron was also a special friend who always encouraged me to speak up. "You know your stuff — you're good." He never opened his mouth: I spoke up.

This is an appropriate time to thank these friends and colleagues publicly for giving me inspiration, competitive edge, encouragement, and helping to make an analyst of this erstwhile Texan. Incidentally I was the fourth Texan to take this path in life. I was preceded in order by Bertram Lewin, Charles Tidd, and Harlan Crank (how 'ya like that name for a psychoanalyst?). Now I'm inclined to say that I only know of four ways to make a living — cattle, oil, cotton, and psychoanalysis. As an aside, I might tell you that Bert Lewin, the last great Kulturmensch, always enjoyed playing Jewish geography when we'd meet.

But it was fun. All that hard work was fun because we were young, ambitious, and dedicated students. Why, I read every word of every assignment over and over as well as anything else I could lay my hands on. I had a love affair with Freud's prose and I still have. Finally Lew Fielding sold his old set of the collected papers to me. I was in heaven, and within two or three years every other line was underlined and every page annotated with my own notes. They are priceless.

In seminars I watched Esther Tietz knit while the old pros discussed psychoanalysis. And I resented each evening — three per week — as Phil Soloman would stand up and call time on the seminar at ten o'clock sharp. Phil hated psychoanalysis. It broke our rhythm and we couldn't get going

again until the next time. But out on the curb we stood and argued about analysis even until midnight.

Oh we were hungry, receptive, ready to grow and change. I worked so hard and so happily learning Freud that it has never been clear to me why I should try to second guess him today with Klein, Kohut, or what have you.

Let me tell you about some of my teachers: Romi Greenson — he was exciting, brilliant, creative, charismatic, amusing, witty, involved. He was Capt. Newman, M.D. His spontaneous discussion of any paper was in a class by itself. His enthusiasm for teaching was such that given a young and receptive audience, Romi was like an old bird dog ready to hunt.

Romi was the Institute — well, that's unfair to all the other fine teachers who really "put out." Romi was an Institute all by himself. The mantle of Fenichel did fall on him and dammit, he wore it well — as a teacher, as an author of original thoughts, as a leader. It is unreasonable to expect another like him — ever. We were just plain lucky for over 35 years. In all my years as a student or teacher anywhere — I've never seen another half so good.

Martin Grotjahn. Perhaps the most widely read and informed teacher I've known. Always witty, entertaining, but with serious intent to teach.

Supervision with Hannah Fenichel or Frances Deri was nonpareil. Both possessed rare, incredible mixtures of intellect and intuition. David Brunswick and Milton Miller handled dreams like Pancho Gonzales handled a tennis racquet. Brunswick was kind, gentle, patient, and always in touch with the unconscious. Miller was cold, aloof, but always bright, creative, and a good teacher who taught you how to anticipate what was going to happen.

Herbert Kupper, Dick Evans, Lincoln Rahman, and Bob Newhouse were four lovely gentlemen who labored mightily to stay ahead of the eager candidates and to find something new to stimulate them. They were good men — all.

Sam Futterman was the most underrated, underhonored teacher around. Sam created and headed the V.A. Mental Hygiene Clinic where most of us learned from scratch how to make a living before becoming analysts. We learned from him in practice how to differentiate psychotherapy from psychoanalysis. I'm afraid that distinction is being lost today.

Carel Van Der Heide had perhaps the finest background in medical as well as psychoanalytic training of anyone of our faculty. He was a knowledgeable, classical scholar and analyst who in spite of jokes about his compulsivity was a fine and sincere teacher of the old school. His organization of the curriculum and the faculty during his directorship in the mid-fifties was a significant contribution. He expected work from his students and the faculty. One thing for sure about Van Der Heide — he always laid the truth on you — good or bad.

Ernst Lewy was Dean of the Training School and Chairman of the Education Committee. He taught no classes but his function as a teacher was implicit. He had a major function for me — he was my Training Analyst and it was through the experience with this classical analyst that I learned how to analyze and what it meant to recover the infantile neurosis, see various manifestations of transference and resistance. I can't remember his ever breaking technique. His function as Dean created some resistances that had to be worked through.

Ivan McGuire succeeded Grotjahn in current literature. What a fount of knowledge! And he thoroughly enjoyed imparting it. He, Larry and I officed on the same floor at 360 N. Bedford Drive and in between patients we often met in the hall and talked psychoanalysis. We ironed out many problems — what a unique learning experience that was. To me there was only one Larry — Friedman, of course.

Norman Levy presented a most completely worked out course in the development of the personality. The point of view was strictly classical and it lent a background for what was to follow in seminars. He was a fine teacher and his efforts were invaluable as far as I was concerned.

The Society was a growing organization which was totally without authority when it came to the matter of the Training School, though in many instances the membership was identical. The Education Committee and the Board of Trustees ran the Training School. Papers presented at the monthly meetings were mostly clinical and inspired lively questions of what was and was not good psychoanalysis. Greenson and Marmor tangled often over just this.

Most members came here between 1946 - 1950:

Sam Futterman	Milton Miller	Ivan McGuire
Herbert Kupper	Martin Grotjahn	Bob Newhouse
Arthur Clinco	Ernst Simmel	Walter Briehl
George Frumkes	Norman Levy	Ernst Lewy
Judd Marmor	Ralph Greenson	Norman Reider
Charles Tidd	Carel Van Der Heide	Charles Sarlin
Frances Deri	Lincoln Rahman	Otto Fenichel
Hanna Fenichel	Fred Hacker	Albert Slutzky
Mae Romm	Dick Evans	

The psychoanalytic study group, which was the anlage of the Society and part of the original training group, consisted of all the Society members plus a number of outstanding lay analysts and persons in allied fields. Papers presented to this group were rarely clinical, but rather dealt with subjects that were closely related to psychoanalysis or ran a parallel course to it. David Brunswick seemed to me to be the prime driving force behind the psychoanalytic study group.

Some of the members that I remember:

Margrit Munk	Estelle Levy	Marjorie Leonard
Marie Briehl	Betty Evans	Manny Lippet
Ruth Lachenbruch	Jerry Lachenbruch	T.A. Adorno
Max Horkheimer	Herbert Marcuse	Dr. Arthur Timme
Dr. Harry Friedgood	Ruth Tolman	

The monthly meetings of the Society and also the study group were attended by nearly everyone. Always the seats were filled and we had to stand in the back of the room. We kinda' eyeballed the room to see who was not present. If we didn't see someone we might inquire if he was ill.

Papers were given by members or candidates presenting a thesis, and they were usually discussed spontaneously from the audience. Not uncommonly 8 or 10 discussants arose several times for lively debate. We were obviously living as part of a group of dedicated, involved, and for the most part terribly bright people. Believe me, they were impressive!

If I missed one meeting in three years it was for damned good reason. I think most everyone felt as I did. I thought it was a rule of the American Psychoanalytic Association and also our local organization that we were required to attend at least one scientific meeting per month. This rule, like the rules of free association, seem to be honored more in its abrogation than its fulfillment.

I have taken the time to mention all these names because each earned the right to notice and because I wanted to do it out of an analyst's respect for the past.

Well, many of these dedicated and involved people are no longer with us, but I remember them with warm thoughts as they were not only respected colleagues, but some were my friends as well.

The guest speakers that I particularly remember: Robert Waelder, Karl Menninger, Erik Homburger-Erikson, Phyllis Greenacre, Rene Spitz, Lawrence Kubie, Bertram Lewin, Margaret Mead, D.W. Winnicott, Michael Balint, Rudolf Loewenstein, Mark Kanzer, David Rapaport, Merton Gill, Margaret Brenman, and of course, Anna Freud.

Meetings were held at the Gold Room of the Beverly-Wilshire Hotel, the Beverly Hills Women's Club, and in the back room of the Mercury Bookshop until about 1953.

Yearly we had joint meetings with the San Francisco Society — one year up north, the next year down south. Outstanding in my memory was the Santa Barbara Biltmore meeting where Eric Berne in an incredible acting out of negative transference verbally attacked his analyst, Erik Homburger-Erikson, for his brilliant paper, "Hitler Imagery and The German Youth."

That night Berne did a kazatsky on top of the ballroom piano with a bottle of wine on his head. It was noisy and drunk out and it is needless to tell you that we were never able to use the hotel again — and oh, yes, Eric Berne did not complete his training — and that seems to have been the beginning of transactional analysis.

We were all new here and just as we clung to each other professionally, we also clung to each other for social outlet. Indeed, it did help us to adjust to a new situation, but it also gave rise to an incestuous little community of analysts and their wives. But the years helped us to outgrow this problem.

We wanted to be psychoanalysts, not psychiatrists who happened to have analytic training. We developed a philosophy, a conviction, and personal depth about psychoanalysis. We knew how to wait and how to dampen our therapeutic zeal for the sake of analyzing. We fought with enthusiasm our battles over what was and what wasn't psychoanalysis. Clearly it was a time of definition and we were all coming out of the slime together.

Shipwreck — the "split" was announced February 16, 1950, and we imbued our little hands in gore for the first time. Friendships and professional ties were in some instances painfully mangled or died from lack of contact and fertilization.

Almost from the 1946 beginning, tensions existed among the members — primarily controversy arising out of the issue of lay analysis. Simmel had offended Mae Romm by asking to see her credentials (she had medical and APsaA credentials and a California license). At the same time, he who had no license, was getting approval from the APsaA for training analyst status for Mrs. Frances Deri.

Dr. Romm offered very strong opposition to lay analysis. Within the Institute, the lines were drawn. Drs. Milton Miller, Martin Grotjahn, and Norman Levy all from the Chicago-Franz Alexander influence joined Romm. Frances Deri called them the May Company and the Beverly Hills Billies. Mae Romm called Deri — Madame Deri.

Fenichel, Slutzky, Deri, Brunswick, Lewy, Greenson were unopposed to lay analysis. Tidd was on the fence having serious reservations as to how lay analysis might interface in medical circles. In voting, however, he sided with those who were unopposed to lay analysis. When Fenichel and Slutzky died in 1946 it left a 5-3 split in the Education Committee. Levy was not yet a training analyst.

Throughout the period from 1946-1950 analysts from these two factions fought with each other — even sometimes shamelessly informing their candidates on the couch of the battles within the Education Committee.

Rumors spread among the candidates, fears of interruption of our psychoanalytic training dominated our conversations before and after seminars, increasingly interfering with our "curbstone" education by 1949.

Most of the candidates took the side of their training analysts and became openly critical of analysts from the other side. For candidates, regardless of the side, it was a *fait accompli* and only time has lessened the pain and disappointment.

Dr. Lewy carefully studied a plan which he proposed as a possible solution to avoid a split and to avoid hardship to students. He proposed what he referred to as the London Plan. This plan proposed two subcommittees, to operate in the manner of the London Institute, each subgroup to operate its own training program. Miller and Romm could not see the plan as feasible and they favored a definite split which would result in the organization of a second Institute.

To facilitate the formation of the second Institute, Norman Levy was appointed training analyst by the Education Committee so that the Miller, Romm, Grotjahn group would now have the minimum four training analysts, could seek approval of the Board of Professional Standards, and gain recognition of the National Organization For Training.

At the same time, Carel Van Der Heide was appointed training analyst for the original group.

The separation into two Institutes took place immediately. On March 3, 1950, this action was explained to the candidates by Dr. Grotjahn who spoke for the newly named Institute for Psychoanalytic Medicine of Southern California. Their opposition to lay analysis and the need to identify with medicine was clearly stated by the new name. The Los Angeles Institute for Psychoanalysis retained its name.

If I may be forgiven I should like to read to you a most important statement.

STATEMENT FOR THE CANDIDATES' MEETING,
MARCH 3, 1950

This communication is designed to present a clear definition of those factors responsible for the decision to change the structure of the present Education Committee and Institute. The views expressed herein represent the deliberations of the following training analysts: David Brunswick, Frances Deri, Ralph Greenson, Ernst Lewy and Charles Tidd.

It is the function and responsibility of the Education Committee and the Training School to teach the basic principles of psychoanalysis. We feel that disagreements about these important concepts make it impossible in the present framework to accomplish this aim. Our group feels that inroads have been made against what we consider to be good analytic practice and teaching.

After much consideration, all the members of the Education Committee concluded that in order to eliminate interference with what each group considered the best standards of psychoanalytic teaching, the formation of a separate Institute was indicated. The following scientific concepts are involved in this controversy:

1) We believe that the analysis of the infantile neurosis is essential to achieving a structural change within the personality of the analysand. Changes of behavior achieved in any other way we consider transitory and not structural.

2) In our opinion, the libido theory, which describes the instinctual development of man in terms of zone, aim and object, is a basic component of the genetic point of view. The interaction of the culture upon these drives determines the structure of the personality. We believe that important amplifications of this theory will be made, but at the present time attempts to discard the libido theory are premature. Adherence to this point of view has been construed as "orthodoxy," "rigidity," and "obstructionism." Actually, the use of such terms obscures the fact that the truly scientific attitude requires rigorous testing before major changes in theory are accepted.

3) We have found that transference phenomena, in order to be of maximum therapeutic value in analysis, must be consistently interpreted. Every other manipulation of transference, particularly transference gratification, complicates the analysis of the transference neurosis.

4) It is our opinion that working through is an essential part of the psychoanalytic therapy and requires a high frequency of visits and an analysis of long duration. As yet no substitute method has proven to be effective.

5) We maintain that the single most important element in the candidate's training is his personal analysis, for it is here that he has the opportunity to experience the Unconscious, Transference, and Resistance. No other training opportunity is equally as valuable. We recognize that seminars and controls are important adjuncts in learning certain aspects of theory and practice, but only after sufficient personal analysis.

6) We believe that it is our responsibility to teach those basic principles of psychoanalysis which have withstood the critical tests of time and experience, as against those which have not. The innovations in technique as suggested by individuals of other institutes may be of value as a form of psychotherapy in certain cases, but are not in accordance with what has been considered the dynamic processes in psychoanalysis proper. Therefore we believe that at the present time the classical procedures of psychoanalysis should be employed in training candidates. It is the responsibility of all teachers of any subject to teach basic principles and to differentiate them from hypotheses and experimentation.

These points represent the views of the training analysts named above. It is our opinion that each member of the other group disagrees with one or more points which we consider basic. Working together for three and a half years has failed to diminish the disagreement on principles, but has made the maintenance of training standards more difficult. It was therefore reluctantly agreed that only the formation of a new Institute would make it possible to maintain the high standards of psychoanalytic teaching and practice. It has been agreed that all candidates shall be allowed to participate in the seminars of either group until and exact and final policies of both Institutes have been worked out. There is no change or delay contemplated in training

or graduation procedure during the period pending the recognition of the new Institute.

The question of personal and emotional factors as the basis for this separation has been frequently raised. There is no doubt that personality factors are interwoven with the scientific differences, which does add to the incompatibility. Nevertheless, it must be recognized that whatever their origin, the differences of opinion about fundamental issues are essential and warrant a change in the structure and function of the Education Committee and Institute. It is believed that the formation of two separate Institutes will make possible a more cordial scientific atmosphere, since it will do away with much of the latent hostility that existed until now.

This statement is submitted to the candidates in this written form since we feel that it does present the content of the controversy of ideas in the most dispassionate way. If there still remain any important unanswered questions, we hope that every candidate will feel free to approach any member of this group. If the candidates prefer meeting as a group with one of our representatives, this can be arranged through the Students' Committee.

David Brunswick
Frances Deri
Ralph Greenson
Ernst Lewy
Charles Tidd

I have omitted details of the bitter and acrimonious confrontations that occurred in meetings of the Education Committee.

The importance of classical analytic training, the European tradition of forwarding the "analytic movement," the importance of becoming a training analyst and of teaching new analysts dominated the "old group's" position. The treatment of patients, newer Alexandrian concepts of short-term analysis, the corrective emotional experience along with transference manipulation, minimizing the importance of working through, and closer ties to medicine dominated the "new group's" position. The old group saw the new group as heretical and a threat to classical analytic training. The new group viewed the old group as intolerant, stultifying, and as a threat to academic freedom. Ernst Lewy and David Brunswick have prepared a documented history of the split and at some time it may become available for limited viewing inside this building.

It has always been our claim that valid scientific differences caused the split. The Southern California contention has been that personality problems dominated the split and that their scientific stand was a form of progress. It is no longer important except historically. Both groups have grown and thrived. There is a friendly and respectful attitude toward one another — and that's as it should be among mature analysts.

Following the "split" each Institute pursued its own training program and its own philosophy. While there was a rivalry for candidates, both fared well.

The many faculty positions at UCLA held by our members made the residency training program a steady source of applicants. USC was the spawning ground for the Southern California group.

It is to the credit of both Institutes that psychoanalysts played such a significant role in the residency and academic programs in both medical schools. This truly was the "golden age of psychoanalysis" in California.

We moved our offices from Commercial Center Street to the back of 360 North Bedford Drive — a small but conveniently located office. It was inadequate for large meetings so we held those in a small auditorium at the back of the Mercury Bookshop. If we really wanted to "put on the dog" for some very important guest, we used the Beverly Hills Women's Club for our meetings. Seminars were held in the evenings at the homes of the instructors.

In 1954 we enlarged our quarters to nearly 15,000 square feet — all the space that was available — and in consideration of a 10 year lease, Sam Klein, the landlord, agreed to install some permanent seating. I designed and had constructed a library, reading room and area for executive meetings and a seminar, and an auditorium with 180 wonderfully cushioned, permanent theatre seats and 30 cushioned folding chairs that enabled us to expand our seating to 210. We had very little money and every yard of carpeting or drapery fabric and every foot of paneling was dear. David Brunswick had told me of an old German cabinet maker — Otto Malz, an amazingly compulsive little man who with great precision carried out my every wish, building glass covered bookcases, walnut and formica library tables, card catalogues, lectern, desks. When he mitred a corner you better damned well know it was really mitred.

The facility was beautifully understated and dignified, a practical place for every function of our Society and Institute. Larry Friedman, who had been housing chairman before me, helped me to fight for every penny. In one of these fights, an unkind person referred to the project as "Rosengarten's folly." But the vast majority were grateful and supported my efforts, and naturally rewarded me with more difficult assignments. Isn't that the fate of the hard workers? I was very proud of that place. It was used almost constantly day and night until we built our home here. The offices were a meeting place at lunch and throughout the day. Practically everyone's office was within two or three blocks of the Institute — somewhere in "couch canyon." Empty hour? Stroll over and read, drop in on an ongoing class, "schmoose" with a colleague or a secretary, or take care of some administrative detail — and oh did some of us have them. It was a social center, too.

It was a damned important place that was a cohesive force in our organization!

Our first speaker was Charles Fischer whose experimental work on subliminal stimulation and tachistoscopic impressions was important. I, in

my eagerness to be a gracious host, may have been obsequiously accommodating. He treated me like a servant and finally provoked me with an audible criticism of "my baby" when he became angry that we couldn't move the front row of permanent seats back to accommodate him. Intolerant S.O.B. Had all I could do to refrain from, "who the hell do you think you are?"

We, of course, felt that we got the best candidates though somewhat fewer in numbers. And since transference gratification had been a factor in the "split" we mostly surmised that people migrated in their direction because they were seduced by promises of shorter training or easy admission through the "back-door route" of conversion of a therapeutic analysis to training with the same analyst — no time or money lost.

Naturally we were entirely innocent of such goings on. Actually I'm not sure that what I started to say tongue-in-cheek wasn't absolutely factual. I believe we practiced our psychoanalytic fundamentalism as much as we preached it. And in a sense it was the right thing for the time.

Our Society membership ranks picked up several transfers during the 50's — Carl Sugar from New York, Morris Beckwitt from Detroit, Robert Dorn, Margaret Rubin and Miriam Williams from London, Gottfried Bloch from Israel, Milton Wexler and Rudolf Ekstein from Topeka and Harvey Lewis from Chicago.

Candidates of the 50's other than Menninger transfers:

Alfred Goldberg	Sidney Fine	Hilda Rollman-Branch
Richard Casady	Henry Hamilton	Arthur Malin
Bernard Brandchaft	Melvin Mandel	Heiman Van Dam
Robert Stoller	Seymour Bird	Marshall Schechter
Bernard Bail	Sidney Hulbert	Marvin Berenson
Ted Schoenberger	Horace Mooney	Donald Perry
James Grotstein	Norman Atkins	Milton Bronstein
Genevieve May	Lee Gold	Justin Call
Richard Edelman	Rita Spies	Leonard Gilman
David Thiele	Fred Weaver	Morton Shane
Joshua Hoffs	Tom Mintz	Ron Mintz
Philip Pennington	Donald Siegel	Richard Wonka
Gerald Nemeth	David Gottlieb	Ralph Obler
Mark Orfirer	Simon Horenstein	Dom Rendinell
Caroline Hays	Richard Migel	Edward White

These are all I can remember from classes or other administrative contact. And they were all pretty damned good. Only four failed to make the grade as their graduations in the late 50's through the early 60's took place.

Most of the earlier graduates rapidly developed wholly analytic practices as soon as we were given the approval by the Education Committee to conduct unsupervised analysis. Being classically oriented we took into treatment only

those patients with the primary indication for analysis — the return of the repression or the return of the repressed. All other patients we referred out to those willing to do psychotherapy — usually candidates in early phases of training.

There was no dearth of patients. In fact with each new patient we managed to "flush a covey" of four or five healthy neurotics to refer to colleagues. Jack Vatz, David Brunswick, Romi Greenson, Albert Held, Ivan McGuire and Larry Friedman got me off to a fast start.

The growing city seemed to have an endlessly expanding need for new analysts. Fees were not so great as to preclude the possibility that a patient could be seen five times per week. Through the 50's average fees seemed to move from the $15 per hour range up to $30 to $35 per hour by 1960. We were happy with that, it was good work, and an excellent time for us.

The careful selection of patients saw a relatively high percentage of good results, and this generated more new patients. Patients were figuratively beating down our doors confidently expecting a "cure."

About 1938 Charles Tidd came here from Menninger's. In 1946 Frederick Hacker, Ernst Lewy, Martin Grotjahn, Mark Stone, Jerry Shiell and Albert Kandelin began the "immigration" from Topeka. It continued through the late 40's and early 50's with:

Adio Freeman	Abe Gottesman	Henry Luster
Mel McDowell	Milton Wexler	Henry Spitzer
Maurice Walsh	Henry Lihn	Robert Hans Jokl
Allen Enelow	Charles Furniss	Gerald Aronson
Rudolf Ekstein	Maimon Leavitt	Seymour Friedman
Elliott Foxman	James Mott	Ed Feldman
Irving Kravetz	Ruth Barnard	

The rather special and talented I. Arthur Marshall joined the Southern California group. I don't know why he did this, but I always regretted it.

Menningers seemed to swell our ranks and more than made up for the loss in numbers that our slow leak to the other group created. They were a little cliquish, but that didn't last too long — they needed time to adjust to the big city and then they became integrated into our organization.

Jokl, Ekstein, and quite a little later, Lihn, became training analysts. In the late 60's many others followed suit. Menningers affected us significantly through the influence of David Rapaport, Merton Gill, and Robert Knight on our early and subsequent leaders. Karl Menninger himself was here frequently and impressive he was. Everyone from Topeka has a "Dr. Karl" story.

The people from Menningers made real contributions to our school — most of them good. But they were trained in a large psychiatric hospital and treated many disturbed and borderline patients with modified analytic techniques. I

believe their influence on many of our people may have tempted them to violate Freud's warnings about trying to analyze the narcissistic neuroses. Our cure rate must have dipped and the character of the patients may have changed.

Personally, I held the line and was able to maintain a full analytic practice and now and then to "effect a cure." It was like shooting fish in a barrel if you picked rich, healthy, neurotics to analyze. Except for a rare selection misjudgment, I was never blown out the little end of the horn.

The 50's were characterized by huge expansion in the number of candidates — many of exceptional quality who are today's leaders not just because they are of an age when we would expect this, but because of the fact that they are the inheritors of our best classical psychoanalytic tradition.

The Institute made strides in the 50's, the Greenson curriculum revision, the Van Der Heide/McGuire/Evans curriculum were well worked out. Jack Vatz, Henry Lihn and I seemed to be on every committee or the Board of Trustees. If we weren't teaching we were in committee meetings three or four nights a week.

I taught my first class in 1952. Freud's writings *"Civilization and Its Discontents"* and *"Future of an Illusion."* Did I work preparing that class for Mel Mandel, Bernie Brandchaft, and Ed Price?? They were good! And what fun to learn with them!

The next year — another class with Kenny Rubin, Neal Peterson, Maurice Walsh, Seymour Bird. Later Stoller, Rollman-Branch, Gilman, Ourieff, and Malin. And then another and another, and another, etc., etc. etc. . . .

THE REORGANIZATION

by Maimon Leavitt, M.D.

My remarks will cover the period of the Society and Institute from the early '60's to the early '70's. During this time I was first the Secretary of the Institute, and the Society, then President of the Society from 1965 to 1968, Director of Education for the Society/Institute from 1971 to 1973, and Chairman of the Joint Committee on Mutual Problems of the Society and Institute, as it was first and officially known, from its inception in 1964 to its termination in the beginning of 1967.

I will speak of a time of turmoil and change, anguish and hope. While I may focus on the difficulties, I want to emphasize the constant forces for progress, cooperation and dedication to psychoanalytic education and to our group. The voice of reason is small but persistent, and the power of the libido shall overcome.

In the '50's and '60's the Society and Institute were the central professional focus of nearly all the members. The group was smaller then, and the involvement in the Society and Institute was very intense. It was generally recognized that the training offered was excellent; nearly everyone seemed satisfied with the quality of the training, and this was confirmed by outside sources at the time of the site visit in 1964. The problems at that time did not relate particularly, therefore, to the quality of the training. Rather, the difficulties related to the atmosphere within the Institute and between the Institute and the Society members. The cohesiveness which existed following the earlier split, which has already been discussed, had become fragmented and increasing divisiveness arose. Often this was related to disputes between individuals, and about questions of policy, power, promotions, assignments, and such matters. It was difficult then, and remains so, to determine the nature of the causes of the dissensions at the time. Various evaluations have been made; I am not sure that any of them are really adequate. Much of the difficulty rested with struggles between strong personalities with conflicting viewpoints and goals. By the beginning of the '60's, it was apparent that the organizational structure of the Institute, and in particular of the Training School was a significant contributor to the difficulties. At that time, the Training School was an autonomous organization within the Institute, which was separate from the Society, and there were such problems as the requirement for 2/3s votes for any significant action, secrecy of meetings, excessive tenure in office and total control by Training Analysts.

I would like to quote from the Dean's Report by Dr. Larry Friedman in 1964:

> "Our Education Committee is composed of individuals with various emotional makeups, character structures, with diverse interests, styles of living, commitments, and expectations in personal and professional life. All this combined with strong convictions and dedication to psychoanalysis, equally shared by all, should make up a group in which there are and should be disagreements, debates on all issues which we could all enjoy and should participate in. But this is not the case at all. Every new training analyst is struck by the unhealthy, angry atmosphere of our Education Committee. Some have refused consideration for training analyst, others wish to withdraw on account of it. We spend more time fighting about meaningless procedural matters than discussing training problems. Decisions are made frequently not on the basis of merit, but on emotional grounds and constantly shifting alliances. New ideas are treated with suspicion, questioning not only the value of the proposition, not only one's judgment, but also one's personal integrity.
>
> The Chairman of the Education Committee is put on the defensive and automatically judged guilty unless proven innocent. He is reproached for action or damned for omission. It would be difficult to find four people as different as the Deans who have been Chairmen since 1950, yet the problems confronting them were almost the same."

In a later report, he stated:

> "Continuous membership on the Education Committee for all training analysts, the unjustified and frequently misused 'confidentiality' of its proceedings, restricted communication with the rest of the membership, created a blind, rigid, self-righteous attitude within, mistrust and suspicion from without."

With all this, there was an increasing sense of alienation of the members of the Institute and the Society from the Training School and, in particular, the training analyst group. There were repeated reverberations of conflicts within the Education Committee and reports and evidence of its inability to gain sufficient consensus so as to get its work done. There was a considerable hiatus in the appointment of training analysts because of these conflicts, and while this was temporarily ameliorated by the well-deserved appointment of two new training analysts, the sense of continuing impasse persisted. For all this, there remained the intense involvement in the scientific meetings of the Society, and the general functions of the Society and Institute by the membership at large.

In 1963, the Institute was attempting to implement certain changes in the bylaws to improve the organization, but could not effectively accomplish this. In early 1964, a Committee on Psychoanalytic Practice, chaired by Drs. William Horowitz, and Leonard Rosengarten, with Drs. Sperling, Bird and

Futterman, was appointed by the Society, and in April of that year they submitted a report regarding the difficulties in practice and the disaffection of the membership with the organization. Along with that of the site visit Doctors, in April 1964, the reports emphasized the urgency of evaluating and reforming the organizational situation and improving the atmosphere.

The Subcommittee for the site visit in April, 1964, was impressed with the level of training and commented upon "the high and sophisticated level of psychoanalytic education... the meticulous preparation by the instructors was most impressive and obviously reflected a manifestation of devotion and a sense of responsibility to the students. A positive atmosphere for learning existed." In contrast, however, they noted a destructive relationship which existed among the members of the Institute: "Complaints seem to transcend the commonplace bickering often encountered. The tension and anxiety revealed to us were at a high pitch and did not bode well for the future... A list of descriptive terms used by conferees included autocratic, paranoid, vindictive, dominated by fear, self-seeking, self-centered, etc. . . . Young psychoanalysts do not write papers since they do not dare read a lecture before the Society. To do so would be to invite harsh, destructive, devastating criticism, according to our informants." They also noted that "a group of young graduates are devoting themselves to the study of Melanie Klein and are being encouraged in this endeavor by a training analyst. They are talented young men being allowed to withdraw from the stream of Freudian creativity." They recommended "something must be done to create an atmosphere of trust and a spirit of cooperation, devotion to the ideals of psychoanalysis and participation in the endless search for that goal." These are issues and goals which echo down through time to the present moment.

In late 1963 and early 1964, Dr. Friedman submitted a detailed plan for reorganizing the Training School, but this was not acted upon. The impossibility of effecting any changes led to his resignation in April, 1965. Dr. Lihn became Dean at that point. Dr. Ernst Levy, the earlier Dean, submitted proposals, but these, too, were shelved, as were the suggestions of other training analysts.

In early summer, 1964, several of us younger faculty, at a social occasion, began to talk about the ongoing difficulties. We dediced to continue meeting on the subject, and to enlarge our group, and by the Fall, felt that such a study group should become an official committee of the Society and the Institute. Accordingly, we urged the case, and with the increasing frustration in the Education Committe, the Joint Committee on Mutual Problems of the Society and Institute was appointed in the Fall of 1964, by the Institute and Society, and began its formal work. The previous Study Committee of the Education Committee was constituted as a subcommittee of the Joint Committee chaired by Drs. Rosengarten and Ourieff to specifically examine reorganization of the Training School. Then began a very arduous effort by the Committee to evaluate the then current organization, the nature of the problems that might

be related to organization, administration, policies, and so forth, to study the organization of other societies and institutes, publicatioans on institute problems and recommendations, and an attempt was made to get maximum input from our members, our then existing Board of Trustees, and the fine legal experts who were then available to us from the Trustees. Many members served long hours on this Committee. I would mention Drs. Atkins, Brandchaft, Brunswick, Fenichel, Futterman, Horowitz, Leavitt, Leventhal, Lewy, Ourieff, Rosengarten, Rosow, Sperling, Van Der Heide, Walsh, and, as consultants, Dr. Rangell and Messieurs Marvin Freeman, Louis Licht, and John Piggott. It was hoped that by gaining maximum participation by all members, the Committee not only would be able to come up with proposals that would answer as many of the purposes and needs of the organization and individuals as possible, but that also the very process of arriving at such a conclusion would itself help to create a positive atmosphere in the organizations, by bringing together all interested persons who would be heard and who would have participatory input. It took over two years to finish this work with scores of meetings in my and others homes, and there were certain attendant drawbacks to this lengthy process. Certain things such as appointments which were already long-postponed were put on hold for an additional length of time, and in some respects, the functions of the Education Committee were further hampered by the wish to await the outsome of the Committee's deliberations.

But we were not just creating a by-laws, but also an atmosphere and concensus that would permit a united response. We constantly reported back to the members to maintain their involvement. In 1966, Dr. Lihn, Dean, resigned, feeling his proposals for change were being ignored.

In one such report, I stated: It has been the view of some that, other than technical changes, organizational revisions would accomplish little; it was the atmosphere which needed change, however that was to be effected. But organizational changes can be effective if they themselves foster a climate of change and improvement, if they reduce some of the heirarchical sources of conflict, if they force a reassessment by individuals of their roles, if they reflect the wish of the membership and help define the areas of conflict. Such changes should not be merely expedient to deal with some passing phase or particular office or individual. But the nature of these difficulties we suffer indicate they might be diminished by a different organizational structure.

The new bylaws which created a single organization were adopted at the beginning of 1967 by the Institute with Dr. Vatz as Director, and the Society with myself as President, by a favorable vote of 90%, unheard of in our organizations about anything, even adjournments at midnight. It was this concensus that convinced the Education Committee to relinquish its control and accede to the plan, as well as the merits of the plan itself.

These bylaws, with which you are all fairly familiar by now, were an attempt to reorganize the training school and preserve its autonomy, and yet establish

an organic relationship with the Society members as a whole. All within the limits of what was politically possible at the time. It was an attempt to improve the administrative structure and simplify the complexity (even though it still maintained a fairly complex organization), to improve the sense of participation by the members, improve the efficiency and capacity to function of the Education Committee, now known as the Coordinating Council, to give the faculty a greater voice in the operation of the training school, thereby lessening the dominating and conflictual control by the training analysts, and in general to create a certain degree of participatory democracy, along with an autonomous training school. The confidentiality of the training analysis was regarded as crucial. The Chairman of the Committee on Institutes, Dr. Sylvan Keisor, wrote of the heroic job we were doing.

The basic organizational premise we have started with is of a single organization rather than two. The reasons for this include efficiency, decrease of polarization, conflict, and communication difficulty, increased membership responsibility, and common sense. Within this organization, the Training School is responsible to a democratically elected representative Board, yet preserves a large degree of functional self-direction so as to operate effectively and not be overly responsive to political vagaries in the educational function.

The merits and disadvantages of this reorganization are still being discussed. Many subsequent changes were made because of perceived limitations in the original proposal, but the general principles are still maintained.

I, along with others, have wondered whether the changes that were made might have proven to have been more effective, if it had been possible to operate under them without the pressures and turmoil consequent upon the simultaneous development of strong groupings of different theoretical orientation, which took on political importance, and which, in its own way, continued some of the prior conflictual situations in a new arena. In our organizations in the 50's, there had developed a study group involved with the object relations school; subsequently, as already noted, a number of these individuals, plus others, became involved in the work of Melanie Klein. This group, while a minority of the membership, held very strong views and demanded to be heard, recognized, and to participate in the intellectual and educational life of the Society and Institute. Very strong feelings were aroused on all sides about the theoretical issues, the tactics, integrity, and so on, of all parties. It was a very turbulent time, very disruptive to the organizations and, most particularly, to the education of the candidates. The changes in the organization brought about by the reorganization were an effort to open up the organizations and the intellectual life to fresh and divergent viewpoints, and this very change then allowed the developing Kleinian theoretical and political grouping an opportunity for expression and participation that, under the previous organization, would probably not have been possible. Under the previous arrangements, it is open to question as to whether such

movements might have taken on the strength that they did or, if they had, would they have led to as much conflict? Would they have been kept outside the Institute, or would they still have led to the disruptions that did take place? Perhaps it was a fluke in timing, perhaps it was inevitable, but this development made it very difficult to evaluate what could have been the effectiveness of the reorganization had it not occurred. On the other hand, it is possible that it did allow the flexibility to survive this period. These were very difficult times. The antagonisms became as intense as before the split. Serious threats of legal suits, hamstrung action, talk of splits, and intellectual mayhem filled the air. My own view is that we were surviving the difficulties and, in time, the heat of the conflict would have diminished as the revolutionary fervor played itself out. In truth, this did occur, but extended turmoil first prevailed, and considerable further efforts were required to temper the situation.

In February, 1973, we had another site visit. I, for one, felt that we had made some very progressive changes in our organizational structure and training school which would be of interest to others, and incidentally, a number of which have since been adopted by other institutes. I also felt that we were attempting to deal with the theoretical disputes as effectively as possible in the circumstances. Accordingly, as Director at the time, the open participation of all members of all persuasions, in the site visit, was encouraged. The site visit report was devastating. The Subcommittee accepted every complaint as indicative of widespread disaffection. Some of us felt that it was an exaggerated, biased report, reflecting, in part, the prior prejudices of the site visitors and itself a political statement. Others felt it was an accurate report, reflecting the very real disruption going on locally. The principle focus of the report this time was on the quality of eduction, and here the committee felt that it had suffered mightily since the previous site visit. Some of this they ascribed to elements of the re-organization of which they disapproved, particularly the changes in the Education Committee, some to the Kleinian group and its having any role in the educational process, and some to the weakening of educational functions because of the ongoing conflict.

While in many respects this destructive report did give us all pause, and led to self-examination and efforts to try to improve the more obvious deficiencies, but our internal conflicts still took some time before they were played out. The report certainly was useful in confronting us all with the ill consequences for training of the conflicts persisting amongst the faculty and clinical associates. Even though I personally feel that the committee's distaste for our philosophy and policy implementation distorted their view of the quality of our education, nevertheless, the report served the useful purpose of forcing us to a sober reconsideration of our situation. One of the principle matters that concerned the committee was that of quality control, particularly in regard to training analyst appointments; but also, in regard to acceptance and progression of clinical associates, selection of cases and supervision. The

training analyst appointment was a matter that much concerned the Joint Committee in its deliberations, and it was at times difficult to separate it out from political considerations. There is not time to go into this matter at length now, but it is a continuing concern. There are those who feel that the quality and selection of training analysts, for example, is a very important matter, and that there are effective ways to accomplish this. There are others who felt that this is really an extremely difficult task, if possible at all, and serves as a rationalization for appointment based upon very different considerations, such as long familiarity with colleagues, as well as conformism, orthodoxy, cronyism, and politics in general. I raise this because it is a matter of ongoing consideration, and one which will always be with us in regard to both our faculty and students.

Following upon the site committee report, there was a great deal of effort to deal with the problems of education, as well as to try to further resolve the divisive aspects of the Society and Institute. A liaison committee was appointed by the American to work with us, and this, indeed, proved a most helpful contribution. Subsequent administrations worked very hard with these matters and you will hear more about it, perhaps particularly the work of the committee which Mel Mandel managed so effectively. Also, to anticipate just a bit, after further struggles and a brief, abortive consideration of another split, the Kleinian issue simmered down to the point of becoming a non-issue. To a considerable extent, this was due to the rising interest in the ideas of Heinz Kohut, which now captured the interest of many of those previously concerned with Melanie Klein, thereby thinning the ranks and diffusing the interests. This new theoretical orientation did not take on a strong political coloration like its precessor, and accordingly, the atmosphere has become much less charged. The very focus on aggression and projection in the Kleinian conceptions may have had much to do with the aggressions aroused, while the Kohut viewpoint encourages a very different quality of response. The defused investment in the Klein controversy may have encouraged a more relaxed attitude about the threat of divergent theories in general. The issues we face remain those of quality education, both graduate and post-graduate, continued scientific interest, and service to our members and the community. The basic goals of psychoanalysis have not changed, nor has our commitment to them.

Another development I should mention, which reflected the positive forces at work, was the extensive work with the Southern California Institute in establishing the San Diego Psychoanalytic Institute. Likewise there were moves at that time to consider rapprochement with the sister organization.

As I look back on the decade under review, difficult as it was, in many ways "it was the best of times and the worst of times." Like the era of this quotation, there was turmoil, distress, personal anguish, danger to societal integrity, but it was a time of ferment, dedication, optimism, new ideas, revolutionary fervor, the passions of committed life. Along with conflict went cooperative

effort to common goals. I calculated once that well over 4,000 man hours went into the work of the joint committee. There was hopefulness in 1967, along with doubt. The rapidity of change was brought home to me, when two months after the reorganization I heard myself referred to as an "A.K.", whereas only five months earlier the appellation had been "young turk." I must not let this occasion pass without also paying tribute to our spouses, who even more than usual were tolerant of our excesses and supportive of our efforts.

I trust the passions and comittment to psychoanalysis are still within us and do not require destructive conflict for their arousal and intensity. This we need today when we have new challenges confronting psychoanalysis as well as the age-old ones.

THEORETICAL HISTORY: 1955 TO THE PRESENT

by Morton Shane, M.D.

It is my pleasure to have this opportunity to review with you the theoretical developments of our Institute over the past thirty years on this 40th anniversary of its founding. As it happens, the American Psychoanalytic Association just celebrated a birthday, too — its 75th. Charles Brenner, as the plenary speaker for that occasion, spoke of the history of psychoanalysis from the point of view of a participant observer. I refer to his comments here because many of what he identified as important trends in American psychoanalysis were linked, in one way or another, with our local scene. Brenner noted that once the period of heroic contributions by Freud was over, the mainstream of psychoanalysis then became profoundly influenced by three major contributors: Anna Freud, Heinz Hartmann, and, most pertinent to our discussion here, Otto Fenichel, whose work on technique was translated from the German in 1941 by Dave Brunswick. Counterposed to this mainstream in the 40's and 50's, and appearing to the participant/observer at that time as if it would actually carry the future of psychoanalytic ideas, was psychosomatic medicine, spearheaded by Franz Alexander, who also has personally influenced the Los Angeles community. Meanwhile, on the international scene, Melanie Klein, along with others of the British school of object relations, competed for center stae. Then, turning to the present, Brenner called attention to more current controversies, concluding his remarks with the question: What will it be like on the 100th anniversary; which theoretical model will stand the test of time? Will it be the model of structural conflict of mainstream analysis, or self psychology, or object relations, or infant research and the developmentalists? I bring up Brenner's address today because it demonstrates that the theoretical history of our Institute reflects the major trends on the national, and international, scene. Many influential contributors throughout the world have played a part in our local history — a history characterized by the existence of a lively, passionate, and increasingly competitive marketplace of ideas which have alternately challenged and contributed to the ever widening mainstream of psychoanalytic thinking.

Undoubtedly, the most dramatic aspect of our history over the past thirty years has been the competition between the British object relations school and proponents of mainstream psychoanalysis, the most dramatic because (as you have heard from Mike Leavitt) it came close to tearing us apart once again as an Institute.

powerful and controversial among them. It should be noted that in other parts of the country, particularly the East, Klein had been dismissed as an aberration of the English and the South Americans, and, in our Institute, those who fought against her ideas were identified with this mainstream view. On the other hand, those in our Institute who became involved with these ideas devoted themselves enthusiastically to their study and elaboration. Early in this development, one group, formed for this purpose, presented their work to the Society with a reception that gave the members of the study group pause. Severe criticism met their efforts to take Klein's work, in particular, that seriously. At least one member of that group responded by concluding that the Institute was not ready to officially receive the ideas of the British school. Having journeyed to England to learn firsthand from followers of Melanie Klein and others, he invited Herbert Rosenfeld to present his ideas in Los Angeles, but not under the aegis of the Institute. Instead, Rosenfeld's trip was privately subscribed to. This initiated a procession of visitors from England, some official and some unofficial, such as Winnicott, Betty Joseph, Margaret Little, Bion, Hanna Segal, and Guntrip.

The groundswell of intense fervor flourished as a movement half inside and half outside the Institute, met by an equally intense mainstream opposition. I think an important part of the scientific differences between the British school and the mainstream related to questions about who is analyzable and, in particular, what constitutes analysis. Fairbairn and Winnicott state overtly that interpretation is secondary to the object relationship experience. Klein does not make such a distinction, but the severe pathology of those whom she treated led her critics to believe that interpretations were received as the experience of being understood, with the content, and resultant insight, being far secondary. Mainstream analysis, on the other hand, viewed interpretation and insight as the key mutative process, with the relationship a more or less distant second. Furthermore, and perhaps most salient, is the fact that the British school shifted the focus of importance in pathogenesis from the oedipal phase and triadic conflicts as central, to a focus on the preoedipal phase as the seat of pathology, with Fairbairn and Winnicott stressing dyadic deficiencies, and Klein stressing very early dyadic and triadic conflicts, some of which are based on inborn, innate phantasies.

Fortunately, this highly visible controversy between British school adherents and the mainstream did not freeze either group into a locked position. Each, I believe, eventually learned from the other, and each made significant theoretical progress within its own ranks. In the mainstream, Romi Greenson, from the late 50's on, was busy refining ideas of technique and pathogenesis that culminated in his monumental book on technique. He was in the forefront of postulating preoedipal relationships, especially that between the mother and the child, as important in their own right and also important in the contribution they make to oedipal development and oedipal pathology, elaborating this position in a number of significant papers. Furthermore, Greenson's original concepts, the working alliance and the real

relationship, brought object relation aspects into the foreground of the analytic situation. Finally, he was instrumental in bringing the ideas of Margaret Mahler, and Mahler herself, into our scientific community.

Leo Rangell, in a truly prodigious output of original contributions and scientific participation, has furthered mainstream psychoanalysis and sharpened the differences between the mainstream and competing frameworks. He has defended on many occasions his contention that the Freudian framework is both superordinate and flexible enough to accommodate all of the alternative frameworks, viewing each of them as comprising but a partial aspect of the grander totality of mainstream theory. To name but a few of his specific contributions, he focuses on anxiety as a key affect in pathogenesis, advancing the integration of Freud's two disparate theories on anxiety. Rangell views anxiety as the key affect in pathogenesis and conflict formation, differing with Brenner over the pathogenic role of depressive affect. He also elaborates his useful image of the wheel, with its hub, radial spokes, and interconnections, to delineate the analytic process in its totality.

Rudy Ekstein, coming to our Institute in the late 50's, did landmark work on childhood psychosis and the use of metaphor in understanding the interpretative process in child analysis. In a sense he was similar to McGuire, who introduced the ideas of Fairbairn and Klein to the community, in that he was receptive to new ideas but respectful of classical contributions, and is notable as a psychoanalytic scholar, and like McGuire dislikes true believers of any ilk.

Finally, I will name just one more person from our Institute who has made, and continues to make, clear, original advances in mainstream analysis. Bob Stoller's groundbreaking work in gender identity, a term he himself introduced into psychoanalysis, advanced the general understanding of female and male development well beyond Freud's original contributions. Stoller questioned Freud's understanding of femininity and put forward primary femininity as a central concept in female development, destroying once and for all the notion of the little girl as but an envious variant of the little boy. His work inspired the therapy and analysis of children with gender identity problems and in addition elaborated the significance of Margaret Mahler's contributions, particularly the concept of symbiosis anxiety and the unending symbiosis of the transsexual. Stoller also put forward some original and controversial ideas about sexual excitement.

What I have just reviewed are the most obvious contributors to the mainstream from our Institute. I could easily name ten more, and I have no doubt that you could, too, but I want to turn now to those who have furthered the tributary of the English school in our Institute. Wilfred Bion has emerged most clearly as an outstanding post-Kleinian contributor, publishing many original works, among them his germinal contribution to the understanding of groups; the development of an organization of human thought structured

by his grid; and the providing of a fresh metaphor — the analyst as container. In addition, Bion enabled many who came into contact with him personally as analysand or supervisee to move beyond the generic Kleinian views to more expansive and inclusive positions. Jim Grotstein has been foremost in advancing Bion's ideas, both in our community and in the literature. In addition, Grotstein has been a spokesperson for modern Kleinian thought. Finally, he has made significant contributions in integrating Klein, Bion, Winnicott, and Kohut, at the same time advancing his own dual track thesis regarding development and pathogenesis.

Albert Mason, too, has written in the Kleinian framework, but at the same time analyzes oedipal level neurotic conflict in a more classical fashion, illustrating the benefit that can accrue from an openness to competing frameworks. From outside our Institute, on the national and international scenes, I can think of two important influences in the development of scientific ideas in our Institute. Kernberg represents a creative Americanization of Kleinian ideas, especially in the understanding and treatment of borderline pathology. He has been a subtle force for integration of Klein and mainstream analysis. Another salient example of the creative use of Kleinian thinking in the mainstream is provided by a frequent visitor to our Institute from England, Joseph Sandler. He is steeped in classical analysis, has been a close collaborator of Anna Freud's at Hampstead, and yet finds concepts from Klein, most particularly projective identification, useful in conceptualizing what he has come to call actualization in the countertransference. He presented this particular idea to our Society about nine years ago. It was understood and accepted by our members, all of whom were by then sophisticated in this regard, a hard-won sophistication, to be sure.

To return to people from our own Institute, undoubtedly the most venturesome of those who had been attracted to ideas of the British school, and the one most instrumental in introducing them to our community, is Bernie Brandchaft who subsequently, through an analysis with Bion and through experiencing the phenomenon of negative therapeutic reaction in several of his patients treated with a Kleinian model, reformulated his ideas of a therapeutic approach along the lines of Heinz Kohut, but more of that later.

I want to turn now to another important development in our theoretical history, one less dramatic, I suppose, but still of profound significance. That is the role of child analysis in our community. It is interesting to note that while Melanie Klein was an extremely important child analyst, the differences between her views and those of the mainstream child analyst, Anna Freud, were never joined in controversy within our Institute's program. Rather, Anna Freud's views predominated. All of our significant early teachers were Hampstead trained, with Marie Briehl in the Southern California Institute trained in Vienna by Anna Freud. Klein's contributions were viewed from a distance with a combination of respect and serious question.

The story of child analysis in our Institute begins with Hannah Fenichel, who, though herself not a child analyst, was familiar through her training in Europe and her personal contact with the Bornsteins, with child analytic theory and technique, and thus supervised in the early 50's our pioneering child analytic candidates, among them Heiman van Dam, and Roc Motto. She was joined in 1951 by Gretta Ruben, who came from Hampstead, and who became the Institute's dean of child analysts, followed by Miriam Williams. Anna Freud was initially, and remained, the chief influence, her visit to Los Angeles in 1959 solidifying her hold on the child community. Her work, *The Ego and the Mechanisms of Defense,* highly significant to all of analysis, as Brenner noted in his plenary address, was particularly useful to the understanding of child analytic theory and technique, for example, the concepts of denial in fantasy, identification with the aggressor, and ascetism in adolescence. Later, in the early 60's, her contributions of Lines of Development and the Developmental Profile became significant research tools forwarded by Chris Heinicke with the help of many of his child analytic colleagues. Among Heinicke's many research publications is the only significant study that exists on a comparison betwen once-a-week and four-times-a-week therapeutic intervention. As had been predicted by Hartmann and Kris in 1946, child analysis has brought to adult analysis many insights in regard to environmental influences, and the crucial role of the new object in the analytic situation.

In the late 60's and early 70's, using the salient contributions of Erik Erikson on the life cycle, and Margaret Mahler and her coworkers on separation individuation theory, a developmental orientation and approach was formulated, leading to further integration of adult and child analysis. The mainstream model was broadened by the developmentalists to focus on preoedipal development, as well as development throughout the life cycle; an understanding and treatment approach to borderline pathology; and an appreciation for the role of object relations experience throughout life and as a mutative force in the analytic situation. This point of view, which includes infant research, was furthered by contributors in our Institute. Infant observation, a burgeoning field pioneered by Spitz, was advanced locally by Justin Call and his coworkers, enriching our child and adult programs. Thus, child analysis and its informing theories grew and developed in our community in a relatively noncontroversial atmosphere. The much earlier controversy between Anna Freud and Melanie Klein had been more or less resolved in the minds of child analysts through an integration of Anna Freud's and Klein's ideas. The introductory phase, touted by Anna Freud and distained by Melanie Klein, wherein the analyst tries to become for the child a useful person in his or her life in an effort to entice the child into a therapeutic alliance, was resolved by Berta Bornstein who counseled interpreting the child's early defensive withdrawal from the analytic situation rather than trying to get around it in a manipulative ploy. Thus, Klein's view that child analysis should in every way be similar to adult analysis was accepted as more

right than wrong. On the other hand, Klein's method of deep interpretation, which ignored systematic working with and respect for the child's defenses, was not accepted. Kleinian child analysis in pure form was never introduced into the curriculum of our Institute. It has, however, been put forward successfully in this community by some analysts at Reiss-Davis, which orientation continues to the present in a form modified by the insights of Bion.

In the early 70's, as the theoretical controversies in Los Angeles between the British school and the mainstream were reaching their political zenith, another frame of reference was being developed in Chicago which would prove to be important to Los Angeles in its own right, and was to provide what might be considered a leavening influence on the dichotomy resulting from the diverging mainstream and Kleinian viewpoints. The psychology of the self, which began in 1966, developing out of Kohut's original 1959 paper, is addressed to the treatment of what Kohut first called narcissistic personality disorders. In 1971, Kohut's *Analysis of the Self* was published, causing quite a stir throughout the psychoanalytic world. A definitive psychoanalytic treatment was outlined for various narcissistic personality types, specific transferences and countertransferences were delineated, and all of this was kept more or less within the confines of mainstream psychoanalytic theory.

To review very briefly, the theory of treatment had specified that certain types of transferences will unfold in analysis if they are permitted to do so without interference. Such mirroring and idealizing transferences are reputed to be difficult for the analyst to bear without attention to inevitable countertransference dysphorias, such as the pain of being ignored, experienced in the mirror transference, and the discomfort and embarrassment of being idealized, experienced in the idealized parent imago transference. Destructive aggression is viewed as emerging in the form of narcissistic rage reactive to empathic failures. Isolated drives are seen as breakdown products of a fragmenting self, again reactive to empathic failure. A central Kohutian thesis is the focus on a subjective stance wherein the analyst attempts to view the patient's productions almost exclusively from within the patient's subjective world.

Kohut's 1977 book, elaborating the ideas proposed in 1971, expands self psychology into a general approach for understanding normal development and an enlarged sector of psychopathology. This expansion of theory relegates conflict-generated neurosis and neurotic character to a smaller sector of the patient population for whom mainstream analysis is still seen as the appropriate treatment, but the larger share of pathology is conceptualized as best treated by self psychological understanding using the concept of a superordinate self. One appeal of Kohut's theory is that patients who had been considered too narcissistic to be analyzed by conventional analytic approaches might be dealt with interpretively, and though borderline and

psychotic patients are considered by Kohut to be unanalyzable, they are at least understood within this same framework, and treatable using a therapeutic approach.

Los Angeles first became interested in self psychology when representatives of this Chicago school visited us in the 70's, and by 1979 Kohut led a conference at UCLA, sponsored by a few of his friends and friendly critics, and a burst of enthusiasm followed which has been sustained, more or less, to the present. It seems to me that the attraction for Los Angeles of this newer theory is that it allows for the ever-sought-after widening scope of analysis, while at the same time it provides avenues, for those who seek them, of sustained integration within the mainstream. Further, I would suggest that for our Institute in particular, the similarity of Kohut to Fairbairn might have made Kohut appealing to those who had studied Fairbairn closely and admired him. My brief review of Fairbairn and Kohut indicates likenesses. In fact, parallels between the two theorists are many, but I will add just two more: Both insist on the object-environment being essential to sustain the individual, Fairbairn, along with Winnicott, describing the individual as developing only in the sustaining environment of object relations, and Kohut saying that the self cannot exist without sustaining selfobjects, any more than a person can exist without oxygen. And Fairbairn, like Kohut, views destructive aggression as reactive rather than innate.

There must have been a comfortable familiarity also on the part of those who had studied Winnicott when they came upon the work of Kohut. Winnicott speaks of the true self and false self in relation to deficiencies and inadequacies in the environment, just as Kohut postulates pathology arising from inadequately empathic parents. Also the idea of insight as secondary, and the relationship as the chief mutative factor in psychoanalytic cure, central to Kohut's theory (especially evident in his remarks on termination), should have been familiar to those who studied the English school. And, in fact, many of our analysts who had been attracted to, or at least familiar with, that school found themselves enthusiastic about the works of Kohut and his followers. In particular, Bernie Brandchaft, central to the object relations movement, became a very significant leader in the self psychology movement. Thus, in 1979, Brandchaft delivered his paper on negative therapeutic reactions and the beneficial results of a self psychological approach. His teaching and supervising activities expanded, and he began to write important original papers on self-psychologically oriented approaches to the understanding and treatment of borderline patients. Many other members of our Institute have become enthusiastic students of self psychology, some going in the direction of separating themselves from the mainstream, and others in the direction of attempting to integrate self psychology with the mainstream. Finally, the large number of active critics of self-psychological ideas in our Institute create the now familiar environment of intellectual excitement and tension that characterizes our particular psychoanalytic free enterprise system.

And so we stand today on our 40th anniversary, sophisticated and worldly wise, anything but parochial, theoreticians of modern psychoanalysis. We have amongst us some of the best and the wisest, not only those whom I have mentioned, but many I have not, for I have not included any of those who are known to us and highly valued for their ease and mastery of the clinical situation or their considerable skill and wisdom in teaching. And while there is this prodigious variety of theories propounded among us, there is one thing all of our members have in common, and that is a serious and vital interest in and dedication to the practice of psychoanalysis, not a watered-down version, but the full strength, heady stuff that drives us at times to a kind of embarrassing frenzy and excess. When tempered, however, our love of analysis, our search for better ways, our impatience with dogma, yields a sustaining vitality that should carry us into an unknown, difficult, but undoubtedly creative and exciting future.

POLITICAL HISTORY OF LAPSI

by Melvin Mandel, M.D.

The reorganization of 1965-67 was designed to contend with problems of the 1950's, and I think it succeeded. But there was trouble on the horizon, noticed en passant at the time, but too vague to attract major attention. At almost one moment, the old problem was effectively dealt with while the new problem was preparing for its entry on stage.

Kleinian psychology was first presented to our Society at a scientific meeting in 1959. Shortly thereafter a succession of English Kleinian psychoanalysts were invited for stays. They held seminars, conducted supervisory sessions, addressed various local psychoanalytic entities and some settled among us. Many followers were recruited from among the psychoanalysts, psychiatrists and para-professionals of the area. The beginnings of broad scale training appeared. Since this activity was organized privately, that is outside the official programs of our Institute, the outcome was that a privately sponsored organization was brought into being. It had all the ambition of a formal training body, but could not hope to accredit graduates at the American unless it could somehow make use of an accredited institute; ours was the most natural and logical training school to cultivate for that purpose.

As clinical associates who were under the influence of Kleinian psychoanalysts entered our Institute, seminars and supervision were thrown into confusion and chaos. It was virtually impossible to teach because each camp operated out of a theoretical and clinical base essentially incompatible with the other's. No theoretical or clinical supposition could be made to begin a line of reasoning, without an a priori attack. Imagine a biological society divided between firm believers in creation and evolution theory. One could no more expect intelligent discussion between that pairing than between Kleinians and classicists.

So our Institute became paralyzed. Once again training analysts could not be appointed. Both Faculty and Society were divided into fighting camps. We had overcome the problems of the 50's only to fall victim to the 60's. As we entered the 70's, the paralysis was becoming total, and we caught the attention of the American, which now became a player in the game.

In 1973 we were reviewed by the Committee on Institutes of the Board on Professional Standards as part of their rotating oversight of the constituent institutes. Much has been said about that visit, mostly unprovable. Some members of the Site Visit Committee may have prejudged us; others may have held such conservative views that our experiments in democracy were unacceptable, and these concerns became the focal points of simplistic explanations.

They did observe our paralysis, however. They could not miss seeing and feeling the tension and conflict in our classes, in our Society, and among our Faculty. They judged our teaching to be inadequate, for good reason, and informed us that unless we were capable of correcting our problems we could not pass muster. Clearly we were threatened with loss of accreditation by the American, and it was our task to reform. They offered us vague guidelines in writing, and other more specific goals were reported to have been delivered verbally. The watchword thereafter was that our first priority was to re-evaluate our training analysts, and root out the "incompetents." The terms "Kleinians, Fairbairnians, and object relations analysts" also crept into the priority listing, either in that report or shortly thereafter in other documents.

After the site visit committee completes a visit, it writes a report which is mainly directed to the institute which was the subject of the visit. The report is intended to be a stimulus to self improvement, through correction of deficiencies, and to serve as an alert to prospective difficulties, the beginnings of which may be more apparent to outside observers. In addition, the report becomes available to the Committee on Institutes of the Board on Professional Standards of the American Psychoanalytic Association. In 1973 the relationships between that committee and many of the institutes were hardly cordial. The atmosphere has now markedly improved, in large measure as an outgrowth of the COI's experiences with us, as I shall later explain. In any case, such a report serves as an evaluative function also, so that accreditation or potential disaccreditation could be based upon the findings of a site visit committee.

Local institutes develop a mechanism for responding to the initial report, and the institutes' responses are then considered by the site visit committees in formulating their final reports. Our Coordinating Council (Education Committee) organized a Site Visit Review Committee, chaired by Dr. Leavitt, to consider the initial report of the 1973 site visit, and to recommend actions which our Faculty could undertake in order to correct the deficiencies noted. The effort to comply with the recommendations of the preliminary report became the mission of the Faculty for the next 3 years. This effort tore apart the fabric of our organization.

Reading the minutes of the faculty meetings, and where applicable the minutes of the Society, tells the story of our odyssey. It is appropriate to call it an "odyssey." It is a story of trial and tribulation, of incestuous rage, fratricide and parricide as well as filicide. There is not much love in it, only thwarted ambition, threatened power, and civil war. Like the American Civil War, it left the participants exhausted, and worst of all, those who were children caught in the strife of the parents have suffered, as children always do, in ways that can mark them for life.

The American seemed convinced of one thing, namely that in order to rid ourselves of our problem the first order of business was to re-evaluate our training analysts.

Here, of course, was the rub. It was clear to almost everyone that the severe problems we encountered were the result of scholastic rivalry, but since it was difficult to uproot a group who operated from a psychoanalytic theory embraced by large numbers of psychoanalysts all over the world, the rationale for the re-evaluation process was expressed as the need to weed out "incompetent" training analysts. No doubt there were a number of us, Kleinian, classic, and otherwise oriented who might have been considered incompetent by a jury of peers, but clearly many understood the problem to be the result of our divisions rather than our incompetence.

In 1973, 1974 and 1975 the Faculty met monthly, sometimes weekly, when we thought we might be on the verge of possible solutions. The reports of the meetings, mostly written by Drs. Rollman-Branch and Ourieff, are remarkable documents. A broad representation of our Faculty consistently entered the discussions. Ideas were put forth, debated, put to vote. Various devices were attempted; everything failed. We debated about going to analysts outside our area for evaluation, then attempted to find some of our own analysts who would be accepted by 2/3 of the faculty, hoping they might begin the process of appointing new training analysts. It was felt their combined efforts might have a ripple effect, thereby reaffirming enough training analysts to move our training programs along. Dr. Greenson received the required numbers of votes of confidence in his psychoanalytic persona, and Dr. Leavitt came close, or perhaps succeeded, but since these two did not provide a large enough base, the effort failed and was halted.

Throughout all our efforts to comply with the understood demands of the American there was always the uncertainty of how such a goal could be obtained, even if we could devise a technique for beginning a re-evaluation process. For the question remained: on what basis would the re-evaluation of our training analysts be conducted? Where would the question of Kleinian affiliation come into the equation, how would it be applied?

As the debate continued and attempts at resolution failed, the question of teaching Klein became more critical. One of the leaders in Kleinian education was removed from an assigned seminar, and feces met a strong air stream. The faculty meetings now openly questioned the appropriateness of Kleinians teaching in the Institute — while accepting the right of Kleinian participation in the Society.

When the threat to training analyst status on the grounds of Kleinian orientation was spoken of, letters from an attorney representing Kleinian interests arrived in our various offices before scheduled meetings. They made it clear that if the attorney's clients were to be removed from training analyst status on the grounds of theoretical affiliation, we would be facing a significant suit. The arguments presented held much legal logic and performed their intended purpose, which was to give our officers and Faculty cause for consideration.

Our Institute officers sought support from the Faculty in the form of assurance that should matters come to legalities, they would not be forced to finance their defense out-of-pocket. In other words, that the Institute would assume the financial burden of legal defense. When the issue was presented, the Faculty supported its officers.

But since we are a Society/Institute, with no separately funded institute, the Society holds the purse strings. For indemnification to be a reality, the Society members must vote their support. The issue was presented to a meeting of all the members, and the results this time were different. Society members expressed a desire for further negotiation prior to approval of indemnification. The hope of the Institute Faculty for a forthright end to its divisiveness was thereby thwarted.

As 1975 was ending it was clear that almost three years of negotiating had come to a standstill. All our efforts to develop acceptable machinery which would get the Institute moving had produced no solution, and the attempt to ram through a possible forced solution was stymied by the legal threats. We were adrift without sail, motor or rudder.

Around the time of the mid-winter meeting of 1975 the Chairman of the Board on Professional Standards, Dr. Weinshel, finally put the machinery of the American into motion. He appointed a committee named the "Ad Hoc Committee on Los Angeles" to join our local scene. Joan Fleming was Chair, and the other members were selected by virtue of their various experiences in other similar situations. Some were educational experts, others were experienced leaders in successful institutes, and at least one had been a candidate when his institute became disaccredited. He was expected to oversee the successful transition of our candidates to other training situations, much as he had experienced help from the American 25 years earlier. Clearly the American was prepared for the worst. But Joan Fleming hoped for the best, and was prepared to work hard to realize her hopes.

In January 1976, the 200th year of our Declaration of Independence, a group of 17 analysts of classic persuasion decided to declare theirs. In utter frustration, no longer hopeful of resolving the issues by their own efforts, they petitioned the American for the right to begin the process of establishing a new institute. It was of some significance that the incumbent Director and Assistant Director of the Institute were among the signers; they had not resigned their posts, so they were in the ambiguous position of pleading for a new institute, rival to the existing one, while they headed that existing institution.

A few weeks earlier, Dr. Rosengarten and I had concluded a series of meetings. We had not been identifiable among the leaders of the disputing groups, and both of us had been officers in the past. I had been President of the Society about 5 years earlier, and Dr. Rosengarten had been Director of Education about the same time. It was our judgment that we might be in

position to offer our services to the Institute should all hope for any other solution fade; that we would then attempt to serve as a bridge between the divided groups; that we would organize a committee representing all parties to the dispute; that we would become investigators into the details of the divisions and issue a report to the Faculty following that investigation; and that for this plan to be attempted it would have to be approved by a very large majority of the Faculty, probably a 2/3 vote.

We had decided on this plan, but waited for the proper moment for presentation. When the action of the 17 analysts became known, it was clear that the critical moment had arrived. As in the Chinese ideogram, it was a crossroad of potential disaster and great opportunity. In a day or two there were defections from the request to begin a new institute; soon another letter went to the American asking that the action be delayed. But the crisis had arrived.

At a meeting of the Faculty on February 18, 1976, reports of events were delivered. Various proposals were heard, and among them Dr. Rosengarten put forward our proposal. The matter was discussed, and finally brought to a vote. Because there now was no workable alternative, our proposal was approved by almost precisely the 2/3 vote which we had declared necessary if we were to implement the plan. So the Committee on the Unification of the Institute (CUI) was born.

Significantly, that very week we received word that the Ad Hoc Committee was fully organized and would arrive in Los Angeles shortly. The Faculty meeting of the 18th, with that information in hand, voted that CUI should negotiate with the Ad Hoc Committee on an equal footing with the Institute's elected officers. So the way was prepared for the next step.

The Ad Hoc Committee arrived within a week or so. They set up meetings with all comers. Clinical Associates, Society members and Faculty members were all invited to meet with the Committee in confidence. They could come individually or in groups, but the Committee made clear they wanted to hear from as many people as possible.

I don't know what transpired when others met with the Ad Hoc Committee, but I do know what CUI's exchange was like. CUI consisted of Leonard Rosengarten, Gerald Aronson, Neal Peterson, Robert Rodman, Donald Siegel, and myself. As we began a very tense meeting, Dr. Rosengarten asked the Ad Hoc Committee members who they were and why they were here. Talk about being wary!

Through the years of our troubles the American always lurked in the background, a vague shadow taking form primarily through the site visit process. In searching for solutions, from time to time individual members raised the possibility of our turning to the American for some form of assistance. The aura cast by the shadow of the American tended to be so conservative, however, that the large majority of our members regularly

analytically, that is that at the moment the analyst finally catches the futility of the patient, it often happens that the patient has turned a corner and is now capable of renewed progress.

We returned to Los Angeles. CUI concluded its report, and presented it to a Faculty meeting preceding the annual Spring meeting in Baltimore. That report pictured our problems as CUI saw them, suggested changes and modifications in procedures and regulations. The faculty discussed the report and by a 70% majority overwhelmingly voted to accept it and the proposals embedded within. This action was seen as significant by the Ad Hoc Committee and the Committee on Institutes. They viewed the report and the Faculty's response to it as an insightful work by our Faculty, and it gave hope. I recommend that the reader review CUI's report, it is still instructive.

By May 5, 1976, at the Baltimore meeting, the Ad Hoc Committee was ready with a remarkable document. It began with the expressed hope that The American Psychoanalytic Association can learn "from the depressing Los Angeles saga." It then presented an abbreviated review of the history of our troubles, focusing on the most recent events. They noted that in spite of our inability to resolve major areas of conflict, improvements could be seen in the efforts of the Administration to improve teaching, and in our tutorial and advisory systems for Clinical Associates. The report made the very important point that some hitherto vital issues, such as rethinking our combined Society/Institute governing format, and especially the issue of immediate reevaluation of all training analysts "would have to be postponed until the educational values (of the Institute) could be better defined." In other words, the priority task which the 1973 site visit report imposed upon us, the pursuit of which had torn us apart for three years, was set aside until educational issues could be first evaluated. One wonders how different matters might have been had the 1973 Site Visit Committee been blessed with greater wisdom.

The Ad Hoc Committee at first felt that the April workshop in Denver indicated it would be necessary for "The American to encourage the Los Angeles group to request provisional status." Their minds were changed by a number of informative items brought to their attention following the workshop. First, our Clinical Associates decided to form study groups of their own, modeled on their experiences at the Denver workshop, for which they expressed much appreciation. Above all, they were no longer consumed by helplessness. And, the report of CUI and its acceptance by so large a majority of our Faculty, the fact that it was "self-confronting" and presented many of the observations and evaluations which the Ad Hoc Committee made, were uplifting to the Ad Hoc Committee. They declared that CUI's report "should become part of the archives of the accrediting committees of the Board and available to future site visiting committees."

As a result of this sequence of events, the Ad Hoc Committee now recommended that a final decision about our Institute be deferred until the

December, 1976 meeting; that a moratorium on admissions to candidacy and graduations from our training program be instituted "until the educational process is on a sounder basis"; that the Los Angeles Institute should utilize the consultative relationship with the American (read: the Ad Hoc Committee) "to assist in the development of an educational philosophy... for studying group process and educational goals and procedures."

Beyond these recommendations pertaining to our Institute, the Ad Hoc Committee also had 3 recommendations for action by the Board on Professional Standards itself:

1. That the Board should study problems encountered in implementing a consultative relationship where provisional status is envisioned. (I would assume this says that when Ad Hoc Committee was organized the American envisioned we would have to be placed on 'provisional status', and this has been essentially corroborated in personal conversations with members of the Ad Hoc Committee in recent years.)

2. Permission to distribute this report "to the appropriate participants." In the past, such reports were apparently too hot to be openly distributed.

3. The Board should evolve modes of clarification of the Freud-Klein controversy and related issues.

As a result of these recommendations the Committee on Institutes set in motion its current system of having a consultative/liaison subcommittee meet with institute representatives at each national meeting. In this way there is an early warning system in place should problems arise. But far more importantly, the orientation of the Committee on Institutes thereby changed from an accrediting, evaluating, and potentially prosecuting relationship to the institutes, to an advising, consulting, and assisting representative of the American. In part because of the reparative work of the Ad Hoc Committee, compared to the previous ineptness of the COI in troubled situations, the relationships between the American and the local institutes have undergone a revolutionary change.

Following Baltimore, our Faculty came together for its annual meeting. The report of the Ad Hoc Committee was read, the Director and Assistant Director resigned, new officers were elected, and a number of changes in our rules and regulations were voted into place. A momentum was begun, culminating in the 1980 site visit. The Ad Hoc Committee, and the Consultative Committee which followed, made regular visits to Los Angeles, during which seminars and workshops in the various phases of institute function were arranged. We reviewed admissions procedures, curriculum development, factors in evaluating progression of clinical associates, and elements of faculty development and progression.

At the mid-winter meeting of December, 1976, in New York, our moratorium was lifted, so we could once again develop classes and graduate candidates. In its final report, delivered to the Board on Professional Standards at this meeting, the Ad Hoc Committee noted that they had been urged to "take over" our Institute, and instead they developed a working alliance with us; they could have entered into our adversarial atmosphere but "refused the assignment." They did accept the role of psychoanalytic educators. Finally they noted the benefits of involving candidates in educational matters.

Thereafter, our Faculty became heavily invested in ongoing groups dealing with issues of psychoanalytic education. The workshops were composed of mixes of our members whereby Faculty members of different persuasions worked side by side with the "other" side. The groups met regularly, reports emerged, and the group process had an effect of its own.

Drs. Bird (Assistant Director), Malin (Chairman of Candidates Evaluation Committee), and Shane (Chairman of Faculty Committee) formed a committed group of leaders whose drive led to revision of a number of the operating procedures of the Institute. Seminars for supervising analysts were organized and supervised by Dr. Goldberg. A series of seminars were conducted in collaboration with the Advisory/Liaison Committee of the Committee on Institutes. We were grateful for the interest and expertise of Jim McLaughlin, Maury Friend, both from the older Ad Hoc Committee, and Jay Shorr and Doris Hunter of the COI. We owe much to all the volunteer consultants who contributed their time and energy out of conviction that psychoanalysis is worth their investment in it, that psychoanalysis is meaningful and worthwhile, and that we would be worthy of their efforts.

So we continued on through 1977, 1978, and 1979. At some point it became known that a number of Kleinian advocates had become students of Kohut's Self Psychology. The eroding political strife, whatever remained of it by that time, was over.

The 1980 Site Visit Committee of the Committee on Institutes conducted our next 7th year review in an atmosphere altogether different from the 1973 visit. Their report was said to be one of the best evaluations of a functioning institute, a tribute to the efforts and abilities of our Clinical Associates and Faculty. As well, it speaks of their spirit.

Since then, Dr. Bird has become Director of the Institute, Dr. Shane the Assistant Director, and a variety of Chairpersons and members have served on our committees. We have had a steady flow of new Clinical Associates, and our most recent classes have been filled with students who have a degree of enthusiasm for psychoanalysis which reminds us of much earlier days.

Compared to the Kleinian difficulty, our current tension between classical and Kohutian advocates creates no loss of sleep. There have been a number of vigorous exchanges, and hopefully there are more to come. Living in a world

of ideas, as we psychoanalysts do, we must welcome new findings, subject them to scrutiny, debate them, and filter them through our core of knowledge. Problems arise when this process is thwarted for any reason; they become compounded when adherents undergo a process of politicization. Educational processes then fade into the background, and Institute troubles take center stage.

Is it possible that the wheel has turned full, and the emphasis on organic factors is causing thoughtful young psychiatrists to again contemplate the mind and inner world of homo sapiens?

observation. And I would go on to say that an education in sophisticated medical statistics was not part of the training of most of the psychoanalysts who, let's say led the profession in the early 40's and 50's. People like Kubie, people trained in the early . . . in the 20's and 30's, for them, for the most part, medical epidemiology and statistics were not important. This model changed dramatically after the second world war, it seems to me. The medical model has become one in which double blind research is important, in which the case history recedes as a mode of valid evidence. And I think this has played an important role in what to some is the persuasiveness, or lack of persuasiveness of psychoanalysis, which has in many ways not moved beyond the case history model for the verification of its data. Anyway, these are all just attempts of trying to understand why disagreements occur in psychoanalysis, why it's very hard to resolve them, and why they'll probably be a continuing function of the discourse within analytic communities themselves.

There's a second problem, and that's the very nature of psychoanalytic rhetoric as it's been practiced in the past. The standard reading from 1905 on is to attribute pathology to the dissident. And I'm afraid it's a continuing practice. It's less so now, much less so I'm happy to say. It was used by Freud against Jung, by Jung against Freud, by Freud against Adler and vice-versa, and one could go on through the history of psychoanalytic polemics and there it is. The imputation of pathology to whoever disagrees. And it happens today, in a much more muted, I'm happy to say, a more muted way.

The third problem plaguing this problem of verification, in addition to the problem of generalization and statistics, is the problem of institute research. Most of the major research in psychoanalysis has been conducted, it seems to me, in hospital settings — not in universities or institutes, because their very nature as teaching institutions have not really been terribly natural. Sociologically, research doesn't tend to take root in them for various reasons; it's very hard to do. And also, if any of the studies of psychoanalytic practice are correct, beginning with Glover's in 1941 or 42, psychoanalysis is not monolithic. Analysts are very individualistic; what they do in their practices seems to vary from one to another. It is not, in short, a monolithic discipline, although it certainly has made conscientious attempts to be, in certain very positive ways. Nevertheless it cannot be said to be a totally unified discipline, in that analyst "A" is doing the same thing as analyst "B." And this creates its own problems for research. As you know, the fact finding committee under Hamburg in 1957 could never come up with an agreement on the definition of recovery, some problems of diagnosis, and this is again a central and crucial matter. And it's become even more crucial now when hostile outside forces begin to attack psychoanalysis and place it on the basis of any other psychotherapy, and simply say that you can't demonstrate efficacy, there's no cost benefit analysis possible. And this is again an unresolved problem and it's been unresolved since the mid 1950's. I'm not trying to be critical, I'm just trying to sort of bring up problems that have seemed to have plagued the

history of psychoanalysis over long periods of time. They are not new. They go back a very, very long time.

There are two more problems, and then I'll just try to say how Los Angeles illustrates some of these. First, the problem of belonging to one of the helping professions. Medicine traditionally has treated the nervous and mentally ill, and psychoanalysis is heir to that tradition. Although, as you know, in Europe this position was slightly changed, nevertheless when Sigfried Bernfelt writes back to Anna Freud from San Francisco in 1939 and says, "it may surprise you that only a few people in San Francisco know that psychoanalysis is really related to medicine, that it's an important part of medical practice. We know that in Europe; the American, at least in California, doesn't seem to know it!" But despite the importance of lay analysis in the European sense, I think it can be said that what gave psychoanalysis its scientific status for many people who are outside the profession was its very close alliance with medicine. And this was furthered dramatically during World War II by the fact that in all of the major armed services, the psychiatric services were for the most part directed by psychoanalysts. It's quite surprising. The Navy, the Army, the Air Force. To a lesser extent, the Navy. But certainly in the Air Force and the Army. The major formulators of psychiatric policy were psychoanalysts. And World War II solidified the identification of medicine, psychiatry and psychoanalysis that had begun before the war in what was a real drive for professionalization and respectability.

But, anyway the problem of belonging to one of the helping medical professions is that when medical definitions of science change, then constituent members of that particular professional discipline have problems as those changing definitions affect their status.

The other problem with psychoanalysis and its close relation to medicine is then seeing laymen as rivals, as unscientific rivals, people outside the pale of the clinical experience which confers scientific status on those who have it. And I think there is a kicker which is that the psychologists, of course, have been the ones who have been most insistent on careful statistical studies. In fact, the first psychotherapy research was done not by psychoanalysts or psychotherapists, but by psychologists. There had been collections of statistics that go back as early as the 1920's but quite systematic research was the province of the psychologists, not the psychotherapists or analysts.

Well, given this status as part of a medical helping profession, the problem of professionalization became absolutely crucial. Controlling access, controlling training, controlling the passing on of knowledge has been one of the major causes of all the fundamental splits. Lacan, Klein-England, almost Klein in Los Angeles, and one could go on. This is crucial because just the simple issue of what people are going to be taught to practice a given profession takes on an absolutely critical significance in the training situation in which most psychoanalytic institutes find themselves. You are not going to train people in a theory in which you don't believe, which you believe is wrong-headed.

And there is every reason why tremendous emotion should be involved in this particular kind of problem. It's an emotion laden problem. Horney had very similar problems in New York, as you know. She was a popular Training Analyst, but there were members of the institute faculty who thought she was departing much too far from Freud's ideas, and that you could not turn out candidates who'd only heard from Horney. That it was under-cutting the whole theoretical structure of psychoanalysis. And there were attempts to tighten up on her seminars, at which point she simply departed and formed her own institute. But again, part of this is again the problem that there's no agreed upon way in which to come to some kind of conclusion about how important were her ideas of self and ego psychology in that period; how did they differ from Hartmann's; what was their importance; how did they affect therapy. What happened was the reiteration of accepted theory within the New York Institute by some very powerful people. And this is what in effect, I think happend, and why she left. Her ideas now seem tame, a lot of them . . . some of them anyway are common coin. They're no longer heretical as they once were. And, in fact, they've been assimilated into much of the mainstream of psychoanalysis, including work by feminists. So, it's an interesting history, very similar things have gone on let's say with many of Adler's ideas, some of Jung's. Jung was the first great self psychologist. You would not know that reading Kohut's book. There's not a single mention of Jung's self psychology. Propounded in all of its rather elaborate mythology in the early 1920's. But whatever you think of it, there it was. And it was, indeed a self psychology.

Anyway, the problem then of access to this helping profession, the kind of knowledge should be passed on, the problem of being within a tradition of medicine helping itself, and the fundamental problem of verification are, I think, the roots of what under-lie much of the controversy within analytic societies. And I don't know how it can be otherwise. On the other hand, when I heard you today, I thought to myself this is an example of Kuhn's normal science. After the crises of past, people resume their clinical work, they exchange ideas, they don't disagree over fundamentals, they carry on their clinical work in constructive ways, they are able to integrate new ideas. And I thought to myself, something's wrong with my model, I have to rethink this conflict model and emphasize not only the fact that there's these unresolved problems, but there are ways of collegially resolving them. I don't know what those ways are. I'm even more baffled after hearing you this morning as to how those problems were solved: by attrition?, powerful personalities disappearing from the scene at a given point, or were they solved by cross-fertilization and discussion? It's not very clear to me. But in a sense, Kuhn's scientific community working in a pattern of normal science seemed very much to be what you were describing in the post 1973 period. It was fascinating to hear that.

Let me just say a couple of things. Let's just take the lay analysis question. Through a historical accident there were no powerful medical psychoanalysts in California. None. And the first people who came, the first training analyst,

Brunswick, was a layman. Simmel was never a licensed medical doctor. Neither was Fenichel . . . nor was Bernfelt in San Francisco. The most powerful intellect, the most important people, the Europeans who really were most intimately involved with the development of psychoanalysis in Europe, were all laymen. And it created a terrible situation because California was full of fake healers. Mrs. Wilshire was mentioned earlier. I've gone through the Yellow Pages of the San Francisco phone book that Bernfeld refers to, and it's not quite high colonics in psychoanalysis, but the Yellow Pages have about five or six psychoanalysts who also treat other things. Dr. so-and-so in a downtown . . . what looks like a medical insurance practice; Dr. X, another psychoanalyst — none of whom had been trained. These are medical doctors, not to speak of the laymen. The 1920's were full of popularization. There was a man who purported to be a Freudian analyst on the fringes of Greenwich Village who died sometime in the mid 1920's who used to paint his toenails and go to cocktail parties in a Roman toga in New York. He would talk to the Metropolitan Opera Company Women's Club about psychoanalysis. There were a couple of Jungians running around. There were a lot of people on the fringe of respectability. Some of them were pretty outrageous. If you look through the ads of the 1920's there are lots of them for mail order psychoanalysis done by some one who purports to have a Ph.D. in psychology. You sent away for this course. So what I'm saying is that there's a tradition of the notorious layman, the notorious lay analyst, and this is particularly strong in California for all kinds of indigenous and peculiar reasons. And the Montgomery episode is one, Hugo Staub's extremely difficult problems in Santa Barbara which you perhaps know about. He was one of Alexander's collaborators, a lay analyst in Berlin. He was set up to be head of a criminology institute which a wealthy patron had established in Santa Barbara, and he ran off with the Judge of the Superior Court's wife — something of that sort. I believe this is buried in one of the oral histories — I don't think I can write about it because I don't know how to corroborate it. But, anyway, this does not give a good odor to lay analysts in this period when analysis itself is struggling for professionalization, for professional recognition, for an entry under the umbrella of hard medicine and hard psychiatry — by hard, I mean, disciplined, well-trained. In *Fortune* Magazine in 1935, there was a big, wonderful, fascinating article. I think it's by Dwight McDonald of all people, who later wrote for the *New Yorker* on mental hospitals. Menningers, Austen Riggs, Hartford Retreat and Phipps Clinic are the four examples of the best mental hospitals in the United States. The interesting thing is that Menningers is called the center of psychoanalytic training and psychoanalytic psychiatry in America. And the article goes on to point out that the best training standards, the highest training standards, are maintained by the psychoanalysts. And that psychoanalysis has the toughest rules for training, the toughest requirements for entry, and that's all it says. But that's terribly important. It's an important image, an important image appearing in an important publication at a crucial time. And all I'm saying is that the whole Los Angeles scene with its amorphousness, its lack of someone

"10/40" CELEBRATION

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I think we've had a fascinating morning and I'd like to structure my remarks around three main problems that have been posed. First is the life cycle metaphor: this is a birthday, number 40. Is this mid-life crisis where a group of people look back and say, "How did I get here? Am I going to be stuck in this rat race for the rest of my life? Or can we have renewed vigor and youth?" And so on. I'd like to say I think that the biological metaphors don't apply to institutions any more than they do to nations or people. British historian, Arnold Toinbe at one point typified those peoples and said these are vestigial ancient peoples and they'll never have a renewed life, and so on. And we've seen that isn't true but in the little off-print by Albert Kandelin from the *Bulletin of the Menninger Clinic* that you have in your folder here, you'll see where Ernst Simmel is writing back to Topeka, we are in adolescence and please, you know that's a stormy time and help us out. That was his approach but I think leading to the other problems that were touched on today. Creativity in institutions is one of the big problems that's tackled in different ways. Then we had an exposition in situ with a great deal of pain and a lot, I think, of crisis management. This was Leonard and Mike Leavitt and certainly Mel Mandel telling us about their labors in crisis management, and I think there are things to be learned going probably both ways — from groups down to individual dynamics and the other way around.

I'd like to go through some of the presentations and give you some reactions that will be, I hope, stimulating for the ensuing discussion.

Leonard, I was impressed with the enthusiasm — the excitement in this town about psychoanalysis, in 1946. There was a great deal of idealization. In 1946 if people wanted to learn analysis, the Institute was the only place they could go. If they wanted to get a good lecture on analysis, this is where they had to go. If they wanted to get into analysis or psychoanalytic therapy, they'd have to go to the Institute. And so you stood there talking on the curb for hours afterward. I think part of the picture is that this has been lost. In the whole spectrum of presentations, I don't think it came out that today we face apathy, we face the erosion of energy, we face demoralization. The eagerness to learn and sit there night after night and soak it in is something we'd like to re-capture maybe. I thought there was an excellent demonstration here about the psychoanalytic attitude toward documents. You may know we've had tremendous difficulty with the Freud Archives, and using the documents in research. And then some people got into the Archives and published them and now nobody else can get at them. Leonard emphasized three or four times what we've got here. I can remember being invited by Al Kandelin up to his house, way up at the top of Bel Air, to look at these documents. They were in

files in his bedroom. And I looked out at all that sagebrush and wondered what would happen if one of these California brush fires came along and I said they really should be protected. They're wonderful, they're valuable, this nice collection, and all the labor that went into putting them together, that collection of the split, the interviews with people who aren't here anymore . . . like Hannah Fenichel, Romi Greenson, and so on. *Now* they can't leave the premises. I personally don't think there's anything that loaded in them. I can remember a lot of acrimony in those executive meetings, including one training analyst saying to another, "Why don't you ever send me people for supervision?" And the other man saying, "Well, I don't send them to just anybody." Then the reference to Ernst Lewy's ethics and principles: at one point in a critical vote, he stood aside because he didn't want to vote for himself. So the vote was three to three. It just about went the other way until they went back and said, Ernst, you've got to vote for yourself, this is really important. And his modesty was overcome.

Mike's presentation struck me with a good deal of sadness because it was around those 4,000 man hours. 4,000 man hours put into first aid, into crisis management, into various kinds of group therapy. There's a price, an enormous price which he eluded to in personal terms. Mike's a family man and I multiply that by a dozen or twenty other people involved just as he was. It's also an act of love, obviously. A great act of love for the Institution. But not too many people would be willing to do this. At some point . . . at *many* points the question must have been asked, "Is it worth it?" There's something going on at UCLA that I'm going to talk about in a minute where those numbers of hours come into the picture. You won't find any academics who are willing to put in 4,000 hours on an organizational project. You just won't because time is so precious and it's supposed to go to research and teaching. I thought that Mike's presentation was a formidable synthesis and just a tour de force of all of recent psychoanalytic theory and its disputes. Pursuant to what Leonard said about Romi Greenson and Mort, about his contribution to the real relationship, the working alliance, and what Nathan just said about flakey California — flakey Los Angeles. In my experience this is still the image too often in psychoanalysis nationally. For some years I've belonged to a group called CAPS that meets at Princeton. They'll have a topic to discuss and at one point it was the working alliance. For a full week end we debated and raged about the working alliance and its value. I found myself alone, and at the end a vote was taken, and in this group it was a vote of 14 - 1. The resolution was that the working alliance is of no clinical value, all you need is the transference. And that, quite recently, was the position of these New Yorkers and Philadelphians, Boston, so on, in this particular group. Needless to say, I was the one dissenter. In my experience Romi was a tremendously vital element in this Institute, and personally very generous, warm, volatile. I recall his teaching a dream seminar at the Hawaii meetings. It's too bad that in 1959 you didn't get a chance to present part 2 of that Klein program. Maybe things could have been prophylactically brought out at that time and could have forestalled the crisis of the 1970's.

It seems to me that in your work in crisis management there are certain principles that come out. The first thing that was significant in the account was that at absolutely decisive points you managed to slow things down, you managed to have a chance to go back and reconsider, a kind of cooling off period. Two points I'm thinking of in your account. One, where this law suit was filed. And then there's a decision by the membership to go back and re-negotiate. And in any crisis, and this is on an international level, whenever you get into a time crunch, you have a crisis automatically. And the crisis manager has got to slow things down, just as you would clinically with two people that you might be working with. And second was the letter to the American of the 17 analysts. Then it slows down and it becomes 10, and then they decide not to go ahead. It slowed things down and it kept the Institute together.

There are other principles of crisis management, such as facilitating communication. One side is more interested in theory, in making a theoretical point, another side is more interested in what's being taught, another party's more interested in who's a training analyst and who's on the Faculty and so on. And if you can find asymmetries between the contending sides you can move in a crisis management way and that applies to large groups as well as two people, or an international situation.

It seems to me that the nature of the prevailing socio-economic structure of an area reflects and interacts with its psychoanalytic practice. And that is, in this culture you have what's called the industry. The entertainment industry. I suggest that this reflects in the patient load, in the style of practice, and that it goes back and forth, and analysis has had a tremendous impact also on the kind of humor and the kind of material in the entertainment industry. It's not that there aren't elites in other cities, in other places where there's big analytic practices, for instance Boston or Cleveland or Cincinnati. But there's a difference in style if you're dealing with back-bay Boston and old inherited wealth than this kind of thing here. It seems to me there's something in the nature of the industry that's very fast, that's materialistic, intensely narcissistic, often opportunistic and shallow, that goes back and forth and presents its unique problems and probably affects the nature of practices here.

My own first contact with this Institute was in the study group that met at the home of Ernst Lewy, sometimes at the home of Leonard Rosengarten, in the period of 1966-1970. This was an occasion where a couple of us at UCLA were just beginning our analytic training, Vic Wolfenstein and myself were joined by Alex George who was at Rand at that time, a Woodrow Wilson Scholar, my colleague Faun Brodie from the Department of History, and A. J. Slaybin an English historian. We met with members of this Institute, with Ernst Lewy, Bob Dorn, Ira Carson, Gerry Aronson, Herb Kupper, Al Goldberg, and Leonard. A number of interesting papers came out of this. In 1969 I published a little note on this in the *Journal of the History of the Behavioral Sciences,*

and I see at that time we were working on Woodrow Wilson, that was Alex George's stuff. Theodore Hertzel and Heinrich Himmler were subjects I was interested in and have since been published. Fredrick the Great was a paper of Ernst Lewy's that he first wrote and presented in that group. Winston Churchill, Leon Trotsky, and Malcolm X were all things that Vic Wolfenstein was working on. Joseph Smith and Thomas Jefferson were things that Faun Bodie was working on. And we had, in some cases, guests from UCLA who brought in their material. One was Leonard Thompson who has since moved on to Yale and just recently published a book about South Africa and the origins of apartheid. At that time he gave a paper on the Zulu nation. It's one of the reasons I'm grateful to this Institute, but that was my own experience with the kind of thing that Leonard described. It was voluntary, of course. There was a tremendous amount of interest and enthusiasm, and we stood on the curb till midnight or one o'clock. We stood out in front of Ernst Lewy's house on Manning Avenue. I don't know what the neighbors thought, but sometimes it even got loud and agitated when Ernst and Maria went to bed. I checked in some of Faun Brodie's writings and I see that she, in her books, acknowledged the seminar on leadership.

Well, we have a comparable development at UCLA right now that I want to share with you. Today there are eleven departments at UCLA who have members who are in psychoanalytic training or have completed it, and we meet regularly at the home of Herb Morris, who's Dean of the Humanities. There's the political scientist, Victor Wolfenstein who I've mentioned; in German literature there's Janet Hadda; in English literature, Al Hutter; in History, Bob Dollock, Jimmy Fisher, and Peter Loewenberg; anthropology, Alan Johnson and Ben Kilbourne; sociology, Jeffrey Praeger; in law, Herb Morris, and then we just lost Bill Winslade who has taken his work down to Galveston, University of Texas. Herb Morris is also in the Philosophy Department; the School of Social Work, Jay Cohen; School of Education, Louise Tyler; and of course, in the department of psychiatry, Bob Stoller and Steven Marmer. Then we have a number of people who have been analyzed elsewhere but are not members or affiliated with our Institutes here, such as Elizabeth Mardrake who studies 18th century France. Now that's a big contrast from when I came up for tenure at UCLA in 1971. I was *way* out there, all alone. There wasn't any credit in heaven or anywhere else for being interested in analysis at that time. It had to be strictly on other kinds of credentials and so on. With this group of research psychoanalysts, we are talking whether we are going to continue as we are or going to do something more in a way of a center or an organized campus group. At least one of the assumptions that we've been operating on which probably ought to be re-examined is to keep clear of the Institutes; that you really don't want to get involved in that committee morass. You know, that if you have a program or something you can invite institute people, but anything formal or institutional means you're going to be locked into hours of committee meetings and not get anywhere. That assumption probably ought to be re-

examined and I just mention it here to tell you the state of our thinking. Nobody up there is willing to put in 4,000 hours because there's just too much else going on.

Now I want to close with a consideration of what it takes to make a creative group because I think that's where we are today. I'd had a seminar for a couple of years on creative scientific groups in the 20th century and, one of the nice things about that University setting, you can decide on the question, the research problem, and you can work on it for a couple of years. We have looked, as a graduate seminar, at the Berg-Hallsey at its time of great flowering at the turn of the century when Bleuler was training many, many young analysts, including A. A. Brill and Carl Gustav Jung and Karl Abraham and Ernest Jones and so on. And the Ferme group in Rome, a group of scientists who won many Nobel prizes, and between 1925 and 1938, reshaped theoretical and experimental physics. The Bauhaus group where the masters and the students who were learning architecture and art lived together and shared their parties and so on. Frankfurt School, there are different artistic groups, one of the most interesting is DeBrica, the makers of German expressionist art. In the early exhibits of DeBrica you couldn't tell which artist was which, they didn't sign their paintings. They exhibited together so you didn't know whether you were looking at an Emile Nolda or an Ernst Ludwig Kirschner and so on. Now I think, out of this, I'll distill the research on these creative groups. I think certain analytic concepts are extremely relevant here and it's what's going on today. What I think is happening today is the Los Angeles Institute, having been through its turmoil is secure enough. There's a secure space here where we can be playful. I think that's the secret to institutional creativity, play space in Winnicott's sense. That you have a secure holding environment where what is me and what comes out into not me is considered safe and you can allow fantasy to become cultural creativity. The institutional boundary fuses with interpersonal boundaries, that the people at the Bergholz, for instance, Brill describes at great length how they shared their associations, their dreams at breakfast, and if somebody picked up a fork instead of a knife he'd have given his associations and everybody was very interested in this, and they were discovering the unconscious. There was a sense of excitement about this. This, it seems to me, is what takes place in each of these groups. In the Ferme group, Enrico Ferme when he spilled his idea of beta decay, he did not do it in a seminar room in Rome. Because the group went out skiing and mountain climbing together, it was up in the ValleGardena. Emilio Segrei describes that he had just been skiing and was bruised and battered and very tired, and wanted to lay down on his bed, but Enrico Ferme was there explaining beta decay and he couldn't lay down. This is the idea with which Ferme went on to get the Nobel Prize, but it was on a skiing outing up in the Alps. That's because he felt comfortable enough to play with ideas in that particular setting. As a conceptualization I wanted to share with you something that seems to me to be about analysis, apply to analysis, and . . . just a wonderful stimulating book. It's by Ludwig Fleck, a

Polish immunologist. Well, this little book by this Polish immunologist was published first in German, in Basil in 1935. It's called *The Genesis and Development of a Scientific Fact* and Thomas Kuhn discovered it in the Widner Library when he was developing his thesis. Fleck proposes, and this is on the basis of his own field, he's a practitioner, is that science progresses by esoteric groups who develop their own language and develop their own style of thought. He calls it a thought collective and a distinct thought style. He's talking about the August-VonVosserman group in 1862 in Germany who developed the blood serum test for syphillis. It applies in many, many ways to analysis. The kinds of things are that you have to have an apprenticeship in that particular style. Well, that's what our institutes are about, it seems to me. That's what training analysis is about. He talks about reality and truth and a scientific fact being a network of consequences. And, in fact, I thought this was most exciting to me. He showed that the original experiments of VonVosserman are irreproducible. That the control tests and the negative results are unintelligible and the high positive results were fortuitous, and that his basic assumptions were untenable and his initial experiments were irreproducible, and so on. But they were of great heuristic value. He uses language that certainly reminds me of Freud's metaphor of the telephone, of the analyst tuning in to the unconscious of the analysand as the receiver of a telephone listening to that message. Listen to this, what he says about Vos Vosserman, he says: "It is clear from these confused notes that Vosserman heard the tune that hummed in his mind but was not audible to those not involved. He and his co-workers listened and tuned their sets until these became selective. The melody could then be heard by even unbiased persons who were not involved. If you've got the right set." That's a wonderful series of metaphors that really apply to analysis. He talks about seeing things as a gestalt, and that there are concentric circles of those who use this esoteric language. That is when two immunologists, two real professionals talk to each other, the serums tests are full of grays and complexities. It's not a yes or no. And there's another circle of the practicing physician who wants a yes or no, does my patient have it? I want a positive or negative, and he will get that. Then there's a large concentric circle of the lay public which gets a generalized and popularized idea. It seems to me that this particular model that comes from another branch of medicine and has to do with biology is the one that really fits analysis. That's what it is. It's an esoteric form of communication with its own thought style; with its collective symbols that is in that sense establishing scientific fact.

I think I'll close with a nice thought from Esat Benefin which is when she tells us there are few real answers in life but really what's most important is the questions that we ask, and often these can be most productive.