

FROM EDITORS

We are honored and pleased to publish in the Archives of Psychiatry and Psychotherapy the first two parts of the forthcoming book by Professor Richard D. Chessick, which probably will be completed next year. His memoirs are a testimony of the development of American psychiatry and psychotherapy in the twentieth century and up to the present time, enabling one to understand the direction and complexities of this development. Professor Chessick, currently Professor Emeritus of Psychiatry and Behavioral Sciences at Northwestern University; a physician and philosopher (at one time he was Adjunct Professor of Philosophy at Loyola University in Chicago), who is also former president of the American Society of Psychoanalytic Physicians, is on the editorial board of the American Journal of Psychotherapy for many years and of many other journals such as the American Journal of Psychoanalysis and the Journal of the American Academy of Psychoanalysis. He is Emeritus Training and Supervising Psychoanalyst, Center for Psychoanalytic Study in Chicago, Psychoanalytic Fellow, American Academy of Psychoanalysis and Dynamic Psychiatry, Distinguished Life Fellow, American Psychiatric Association, Emeritus Senior Attending Psychiatrist, Evanston Hospital, Evanston, IL, U.S.A. and is still a practicing psychiatrist and psychoanalyst. The literary form of this publication is an example of the practice of “broad” formula of our quarterly, in which development Professor Chessick was involved from the beginning. In this way we would like to express our appreciation and gratitude for his contribution in the creation of the specific, comprehensive and multi-faceted nature of our quarterly.

Apologia Pro Vita Mea: An Intellectual Odyssey. Part One

By Richard D. Chessick

Summary

This is a narrative in a dialogue form in which the author, now an octogenarian but still in psychoanalytic practice, describes his intellectual evolution from a published laboratory researcher to engagement in the full-time clinical practice of psychoanalysis and psychodynamic psychotherapy. He reviews the development of his ideas through his many publications and offers commentaries on the nature of the origin, environment and content of his thinking at the time each of these were written. In the current article, part one of several projected articles, he covers the period from 1953, when he received his medical and research training and published his first papers, through 1965, when he resigned his positions of Chief of Psychiatry at the Veterans' Research Hospital in Chicago and co-director of the Psychiatry Resident Training Program at the Northwestern University Medical School and entered full-time clinical work while continuing teaching psychodynamic psychotherapy at Northwestern University.

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*“What are the roots that clutch,
what branches
grow
Out of this stony rubbish?”*

T.S. Eliot (1.p69)

Imagine my shocked surprise when George appeared. While walking every day in my neighborhood for exercise, suddenly one morning there he was, walking with me as we used to do in the evenings when I was attending the University of Chicago College under the great and irreplaceable Robert Maynard Hutchins. Later, as a student in the University of Chicago medical school, I was George's protégé in the field of histochemistry, and our discussions continued in the laboratory. George was a genius, world renowned, and he helped me learn techniques to study the histochemistry of the nervous system. I was hoping to find the secret of the mind-brain problem in the histochemical architectonic of the brain. George and I shared interests in philosophy, medicine and science, but he suddenly died of a myocardial infarction at the age of 51, and I was completely on my own after that.

Now George was back again as a spectre and very puzzled as to what I had been doing since I graduated medical school 60 years ago. To him it seemed that I went off in an entirely different and somewhat unsavory direction. I decided, out of respect for George's questions and out of my own narcissistic curiosity, to trace my intellectual odyssey for him over these years. I wanted to establish the enduring aspects of my travels and try to identify the causes of the turning points. I invited George to my home for a cup of strong coffee and rummaged in the dusty file cabinets until I found the first publications that we wrote together in 1953, the starting point of my journey which is now almost completed. I was 22 years old then, a third-year medical student with big ideas. We then resumed our walk.

By the time I was finishing the fourth year of medical school I had achieved a minor reputation in the field of histochemistry, publishing seven papers with the help of George. Before graduation the dean of students called in each student and asked what specialty he or she – there were only three women in our class – was intending to pursue. When I said I was going into psychiatry the dean, a biochemist, almost had apoplexy on the spot. His face turned bright red and he started shouting at me as to how could I do such a thing. He insisted I was throwing away a brilliant research future and essentially threw me out of his office. I was only 23 years old at the time, one of the youngest medi-

cal students who had ever attended the University of Chicago and I was very frightened by this outburst, but nothing serious happened. The reward that was given for each graduating class to the student who had done the best research was given to someone else since there was no point in their professorial minds in encouraging me to go forward in research! Psychiatry was considered unscientific nonsense and psychiatrists were considered viewed beneath contempt, as they still are in many places.

1953-1954

On our walk George and I recalled old times and discussed my seven publications in his field. "The best thing to do is to begin at the beginning," said George. "Remember, Richard, we started out in 1953 by writing two papers together as I was teaching you the discipline which I myself invented. In it we [2] offered some histochemical studies of the inhibition of esterases. This was a highly technical paper, most of it Gomori (who had the ideas) and very little Chesick (who probably did all the work). It led to the conclusion that histochemical studies of esterases with various substrates and inhibitors reveal a lack of sharp definition between various types of the enzyme. Thus the substrate preferences and inhibition effects overlap in such an irregular and unpredictable fashion as to defy attempts at classification.

With this initial paper under my belt I was able to get a grant from the National Foundation for Infantile Paralysis and be appointed as their fellow. Now I could carry my interests more specifically into esterases and phosphatases of the brain, which was the second and last paper I published with George [3]. It was possible to demonstrate that the distribution of esterases and alkaline phosphatases in the brains of dogs and rats could be traced by selective staining of certain fiber tracts and other structures on the basis of their enzymatic activity. These were my learning experiences in the field, completed before I graduated from medical school.

My first solo papers in histochemistry [4,5] were also about the histochemical distribution of esterases, demonstrating the existence of a spectrum consisting of a group of more or less spe-

cialized enzymes acting on certain specific esters of carboxylic acids with a variable amount of overlap depending on the substrate and species used.

"But 1954, your senior year, was still entirely taken up with histochemistry in my laboratory whenever you had free time," said George. "That is correct," I replied. My three solo 1954 publications [6,7,8] are all in that same field. In my first paper that year I was able to demonstrate that coma induced by pentobarbital insulin, or anoxia, does not affect the distribution of concentration of acid phosphatase, alkaline phosphatases, phosphamidase, 5-nucleotides, α -naphthyl esterase, and naphthyl AS esterase in a rat brain. The purpose of this paper was to contradict a previous publication by Swank & Cammermeyer [9] claiming that there was such a shift in the acid phosphatase concentration and to indicate that the Soulairac & Desclaux [10] finding that alkaline phosphatase activity is markedly decreased in coma could not be confirmed. I did not carry on that debate, as I was about to leave the laboratory.

My [7] final research paper in the field of histochemistry, "The histochemical specificity of cholinesterases" attempted to demonstrate that none of the biological classifications or typing of cholinesterases can be defended and that the cholinesterases of various species and tissues should therefore be viewed as a family of enzymes forming a spectrum with multiple overlapping and differences with respect to physical properties, effective inhibitors and substrate specificities.

I closed off my career in histochemistry with a paper [8] delivered at the senior scientific session at the University of Chicago School of Medicine, entitled "The histochemistry of the nervous system: a new approach to experimental neurology and neuropathology." After reviewing some of the fundamentals of histochemistry I pointed out that the parenchyma of the nervous system was found to have its own distinctive enzymatic architecture, and peculiar histochemical esterase staining properties, differing in various species and in particular localizations in the nervous system. For example, five types of neurons, which were all morphologically similar, could be demonstrated to be different by the use of histochemical techniques. These techniques offer new

methods in neuropathological investigation in that changes on the histochemical level in situ as well as the classical morphological changes can now be followed in naturally occurring disease and under experimental conditions.

Around the time of my graduation I set out the following statement of my future plans in my diary: "The University of Chicago has been very kind to me. I have enjoyed the liberal attitude of the College, I have benefited from the specialized knowledge of the Division of Biological Sciences, and I have learned a profession in the Medical School. During this latter period, I was able to do considerable research work mainly through the aid of a number of my professors, all of whom encouraged and aided my work in a variety of ways. However, my wish to become a psychiatrist stems from both emotional and intellectual grounds. My primary interest since college days has been in the field of the humanities – the problems and aspirations of people throughout the various stages of their growth. This basic interest in human life has led me into diversified study, coursing through such areas as philosophy, the arts, and the sciences. Fortunately I have been able to achieve considerable satisfaction from pursuits in all these disciplines. For example, I am sure that no other medical student at the University of Chicago during four years of school spent his lunch break studying the poetry of Ezra Pound, whose poetic genius and pathological personality have fascinated me to this day.

In order to grasp the etiological factors behind the various manifestations of human behavior, however, I was soon led into the study of medicine. Now I have completed the first stage of this study and would like to turn directly to my major interest. This is psychiatry, as conceived by Adolph Meyer: For him psychiatry has no dogma to offer. It builds on a willingness to learn from experience. It strives to develop a science of man under the sign of experience and creative experiment. On achieving proficiency in the field of psychiatry, I will be able to get both the intellectual satisfaction of advancing my understanding of human life, and the emotional satisfaction of using this knowledge to better the lot of humanity."

But, in this imagined residency application, I wrote that, "If accepted at the University of Chi-

ago, I would like to continue my research work into the histochemical workings of the central nervous system if this is permitted during my residency. If not, then later. I believe that the eventual solution of the mind-brain problem lies in the investigation of the electric and chemical changes that occur on a cellular level concomitant with the mental changes. My viewpoint is directly opposed to that of Sherrington, in that I believe mental phenomena, like all others, to be completely explainable on the basis of physical and chemical phenomena. With my previous research experience and after completion of the excellent residency program at the University of Chicago, I hope to be well equipped to continue research and begin the practice of clinical medicine in the field of psychiatry, much in keeping with the present policy of the staff of the department of medicine at the University of Chicago."

This imaginary letter from my diary was never sent because I decided to go to a more pragmatic medical training program through Cook County Hospital in Chicago, at that time one of the best internships anywhere, to make me into an all-around physician. But it does outline my thinking, George, at the time of graduation. He replied, "I was sorry to see you leave the field of research. I wanted you to become a pathologist and specialize in histochemistry. But I did not say anything at the time because I felt you were really not smart enough to make any substantial contribution to the research field, and you were adamantly against having to do lots of autopsies, which were required for certification in pathology."

Even during all the research and medical school studies I was able to produce my first paper [11] attempting to unite the humanities and the sciences. The influence of my college education under the Hutchins plan, which admitted students from the second year of high school and, after testing, placed me into the second year of college, was beginning to show. I was a great fan of Hutchins, and I will be grateful all my life for this opportunity and a scholarship fund that lasted from the beginning of college to graduation, enabling me to attend. I slept on a couch in the living room of our one bedroom apartment and studied in my parents' bedroom when they were not there, and in the dining room or

on a card table in the living room. Basically the Hutchins approach was a total immersion of the students in the great books of the western world, an approach that is still in existence at St. John's College in Annapolis, Maryland, and in Santa Fe, New Mexico. This is the only college to whom I have ever given a large donation, although I am not an alumnus, and have always regretted that I could not persuade my three children and seven grandchildren to attend there.

All my histochemical research was done while I was a third- and fourth-year medical student but my final publication [11], which appeared in 1953 while I was a student, was obviously from a different part of my brain. It was my first effort to try to approach philosophical problems through neurological studies and it was written before I finished the third year. I was not a very bright person and so all my time was taken up by studies while I was in medical school, leaving very little time available for the humanities. Some students could read a page of the textbook and remember it, but I had to take notes and try to memorize it by going over it again and again. The same was true of foreign languages; I studied Latin, German, French, and am even studying classical Greek up to the present time, but I never achieved proficiency in any of them. Saturday nights were free, and I remember vividly that my parents would go out enjoying their social life, and the apartment became quiet, allowing me to apply what I had learned in medical school to some of the philosophical problems that fascinated me from the beginning of my education and still do to this very day. I was very proud of this paper, as it was my first paper in the field of philosophy that had been accepted by a reputable international philosophical journal, *Philosophy of Science*.

In it, I attempted to suggest a cosmology consisting of external "events" (with apologies for borrowing from Alfred North Whitehead) that impinged as energy quanta on a continually metabolizing, dynamically changing, multi-potential nerve network. In this network the characteristic activity is modification of the configurational state (which has been determined at any given time by the basic hereditary structure and the previous reception of energy quanta) due to the influence of fresh energy quanta on the dynamically active network. These processes

are thought to occur with respect to a time axis which cannot be biologically characterized by any simple mathematical formula, but rather is what I called at the time "Bergsonian" and depends on intrinsic pulsations of the neurons, and the field situation in the network. The reader can observe that I was strongly influenced by Bertrand Russell's philosophy at the time, for example in his book *Our Knowledge of the External World*. George added, "I thought this book was the final answer to the epistemological problem, because in it Russell was sensibly materialistic and very down to earth. But you were not satisfied with it, as most young scientists might have been." "As a matter of fact George," I answered, "Here is what I wrote in my diary at this time:

Beethoven composed the Adagio of the Razumovsky Quartet Op. 18, No. 2 in 1806 from an inspiration that occurred to him on a placid starry night. Let the contemplative, inward, expressive, almost religious mood of this choral structure set the theme of our closing passage. Here is a mingling of magnificence of the eternal with an inward perfection of the spirit. Life can be gracious even in the hydrogen era."

Although this was a speculative paper, it did make some kind of attempt to connect our knowledge of neurophysiology with our understanding of epistemology. As far as I know, it elicited no reaction and I do not know even if it was read by anybody. The relationship between philosophers and scientists has been a rocky one during my professional career, as will be shown in future discussions of psychoanalysis as a science. The point of my paper was that the brain is not a simple passive receptor of impulses, a *tabula rasa*, as certain philosophers – here I think of Bertrand Russell again and the whole Lockean tradition – sometimes do. Our "knowledge" must be conceived from both the peripheral receptors and the field situation already present in the central nervous system environment, and influenced by constantly changing chemical factors impinging on fixed hereditary factors. Thus, both hereditary and environmental factors play a crucial role in our interpretation of reality. Now this, in my opinion, written by a third-year medical student in 1953, was the first recognition of an impulse that moved me from the histochemistry of the nervous system to the field of psychoanalysis. It certainly became clear by the

time I graduated from medical school that the brain was an incredibly complex organ and that there were chemical distributions in the brain that were not evident morphologically, which meant that the study of the brain is infinitely more complicated than it was thought to be. In future work it has been demonstrated by the use of radiographic techniques that this is certainly true but I will not go into that area because that is not where I turned my attention once I left the field of histochemistry, a leaving which caused me great regret but on the whole was probably a wise move in the light of my mediocre abilities and wavering interest. George added, "It was clear to me even at the time that you did not have the mental or emotional abilities to spend your life in the world of the laboratory. You were too arrogant for that, you wanted to tackle the big questions. A worthwhile ambition but why not pursue it as a hobby and focus your efforts on little discoveries?"

Another extremely important influence on me during medical school was the chairman of the department of psychiatry, Dr. Nathaniel Apter, a tall, good-looking and charismatic figure. His lectures on the fundamentals of psychoanalysis, which at that time was generally considered the basic science of psychiatry, opened an entirely new vista for me, the study of depth psychology. Here, I thought, by reading Freud might my interests in the elusive histochemistry of the nervous system and the great psychological and philosophical problems fuse together. Apter became aware of my excitement about both himself as an idealization figure and about Freud's psychoanalysis, and he actually visited George (they were both professors in the same medical school) and even once met my parents by design at a dinner party. Almost nothing was told to me of the conversations at these parties, which indicates it could not have been very complimentary to me.

But watching Apter in action trying to understand schizophrenic and neurotic patients was like opening a whole new understanding of human mentation, both normal and pathological, and gave me the desire to become a psychoanalyst. He was doing experimental work with schizophrenics in a state mental hospital in Manteno, Illinois, and allowed students to come with him. It was my first experience with a ward full

of schizophrenic patients before the days of psychopharmacology kept them under zombie-like control, and offered me first-hand demonstrations of the various forms of schizophrenia, a disease no more understood today, when psychoanalysis is out of fashion, than it was then. I spent many hours after that sitting and trying to do therapy with schizophrenic patients who taught me endlessly about the depth of the human mind and its efforts to repair itself.

"I don't remember Apter very well," said George. "He was more interested in understanding us – your parents and myself – in order to understand you. But he never let on what he thought about all of us. In those days, when everything in psychoanalysis was Oedipal, I am sure he was impressed by your father's lameness, but he said nothing. He resigned or was removed from his position soon after you graduated and was replaced by someone interested exclusively in psychopharmacology research. From what I understand, he retired to Florida and amazingly to me became an expert on the biology of snails." Apter is now deceased. Under the influence of his teachings about psychoanalysis I decided that when I could I would eventually leave laboratory research forever, and, even with a significant membership in the highly respected Sigma Xi Research Society, I embarked on a professional lifetime of continuous, ongoing exploration of the psyche of others and inevitably and necessarily concomitantly an exploration of my own psyche.

1955-1958

As stated, I chose a very pragmatic internship at Cook County Hospital in Chicago. The internship at Cook County was probably the best in the United States because it gave complete latitude to one's learning every kind of general medical technique. It was a highly coveted position and the work was excruciatingly difficult, with heavy responsibilities and long hours. Even then I traded some of my clinical assignments for psychiatry assignments so I spent a couple of months in the psychiatry emergency ward and a couple of months in the emergency room itself as well as doing the usual medical, and surgical and obstetric rotations. I was so busy that there

was no time to write anything and I had begun a marriage which was supremely successful – that is the only way I can put it – and has lasted already 62 years, thanks to the extraordinary love and dedication of my wife. Without her support and encouragement I could not have continued in the direction that I went in the medical field and I am infinitely grateful to her for this. By the time I finished the year of internship I was a capable general medical physician.

At that time psychoanalysis was considered the fundamental science of psychiatry and the prize local residencies were all associated with the Institute for Psychoanalysis in Chicago. I applied there for a residency and was accepted and assigned to the University of Illinois Neuropsychiatric Institute, which I preferred. I was also assigned a training psychoanalyst, Dr. Albrecht Meyer. By the time I was in residency for a few months I began my training psychoanalysis, which lasted about eight years, four times weekly. Although the analysis seems to have been successfully concluded, it began under a cloud, which troubles me to this day. Dr. Meyer asked to see some pictures of my childhood and family so I went to our family album, took out the best ones, and loaned them to him. He lost them! What was I to do? This is the problem with a so-called training psychoanalysis. If the candidate reacts to this enactment with deserved hostility or even walks out of the treatment, his or her entire career is in jeopardy. Kernberg and others have written about this problem, and the so-called training analysis should probably be adjusted so that the training analyst should not be able to report to the faculty about anything that goes on; the patient's privacy must be preserved just as in an ordinary psychoanalysis. This is easy to say but probably impossible to maintain since the psychoanalytic community is a small one and there is lots of gossip.

The first year of residency was relatively easy because I already had so much psychiatry experience at Cook County Hospital and I was able to find time to produce a paper on a subject that has puzzled me up to the present, the problem of time. In this brief paper [12], I tried to look at the problem of time by integrating philosophy, neurophysiology and psychiatry, in the same spirit as my earlier paper mentioned above [11]. I got no farther than the work of St. Augustine. I ar-

gued, as he did, that there can be no such thing as “absolute time,” that time depends on creation and before that there was no time. Augustine was actually ahead of Newton, who tried to distinguish “absolute time” from “relative time.” Time, I thought, was something subjectively known but not explicable in a scientific “thing language.” It is something subjectively understood but only indirectly or partially explicable in public language. Another view found in Augustine as well as in modern present day science is that there is only the atomic present or “here and now” in time for us. The past exists in the present only as memory traces distorted by the subjective personality of the individual. I contended that this argument avoids the play on words and confusion, as I called it at that stage in my development, of Bergson’s [13] rather mystical view of time. For time is measurable in scientific terms only when projected by the intellect into some kind of succession, usually in space.

This is the first publication in which I mentioned Freud. I referred to the Interpretation of Dreams [14] where he suggests that the psychic system has a direction and that excitations traverse the system in a certain temporal order. Organization in time of conscious awareness seems to be based on this ordered flow of psychic processes. When the excitations in the system follow what Freud calls a “retrogressive course,” which is neurophysiologically equivalent to the situation of regression that occurs in conditions of sleep, psychosis and so on, one gets the kaleidoscopic mingling of past, present and future appearing in the conscious. Of course this does not tell us anything really about the “nature of time” and its connection to the parameters of space. I did not go that far in my thinking and had I done so, I would have had to acquire a great deal of mathematics such as Riemannian geometry, a subject that I have always loved but never had the time to acquire to my satisfaction. One of the saddest moments in my intellectual life occurred when I was asked by my college mathematics professor to participate with him in solving a number theory problem and thus earn graduation with honors in mathematics. Because the competition was so intense to get into medical school I had to spend day and night trying to earn the best grades in my required premedi-

cal courses – some of which did not at all interest me – so I was forced to turn him down. I remember the very sad scene in his little office vividly to this day. I have always pursued mathematics as a hobby even now and in my (15, p308-310) textbook tried to present a mathematical version of the factors interacting between therapist and patient during the process of intensive psychotherapy (pp. 308-310). There was no response to this from reviewers or readers, so I sadly did not pursue it.

The following year, I [16] published another paper involving the subject of time, “The sense of reality, time, and creative inspiration” in the *American Imago*, my first publication in a psychoanalytic journal. This was my first effort to apply psychoanalysis to some of the problems that I have mentioned above and it also introduced into my writings my lifelong fascination with Ezra Pound, whom I considered probably the greatest poet of the century, although his work was terribly flawed, mostly ruined, by his psychotic perturbations. My paper wanders in many directions on the subject, even mentioning Ezra Pound’s *Cantos* as an epic of timelessness, an attempt to arrest the passage of time. The *Cantos* are an attempt to capture his sense of world time, trying to get the world under control in terms of an organization of history. This is a common tendency in both poets and patients, to personify time as some sort of controlling omnipotent being that must be mastered by one magical process or another.

George interjected, “Don’t you think you are trying to do that here in producing these memoirs? Many old men like you come up with memoirs. I did not get a chance as I was cut down by a myocardial infarction in the midst of my creative work. You are lucky that by the time you developed heart disease, cardiac surgery, which, you remember, was being already experimented with on dogs while you worked in my laboratory, now was available for you.”

I let that pass in order not to get into a digression about old age and death, and continued with reviewing my first investigations into “time.” I began this 1957 paper [6] by pointing out that our ultimate perception of reality is ruled to a tremendous extent by the “configurational state” of the nervous system or the equivalent emotional state of the organism at the time

of the perception. This indicates that the problem of our knowledge of reality is somehow related to problems of metapsychology, what I call a psychological-physiological inference of reality "out there." I turned to issues of ego psychology, as they were described for instance by Federn (17) and others, to try to understand the sense of reality as having to do with the cathexis of the ego as one goes through a number of stages of development. Even the sense of time seemed to be bound up with ego psychology and again can be connected, as Federn argues, with object and ego cathexis as they fluctuate. Of course this is all based on psychoanalytic ego psychology, which was the predominant point of view of psychoanalysis in America at that time. I believe it represents the approach with the highest potential for understanding patients and human mentation in general, and I think it is a tragedy that it is no longer allowed to serve as a basis for psychoanalytic thinking in many places today and has been replaced by so-called two-person psychology. I will discuss this later on in these memoirs.

Kant tells us that time is a form of perception a priori and our notion of time is a conviction superimposed on our perceptions by the very mechanism of our mental functioning and comes from within. The alternative view is that there is something like a noumenal time, a time "out there" or "time-in-itself" behind our phenomenological notion of time, that is, there is some outside time which we learn about through our perceptions and from which we then develop our notion of time. Freud's point of view about time is essentially Kantian. For Freud as for Kant, time and space are necessary forms of thought and, "Our abstract idea of time seems to be wholly derived from the method of working of the system Pcpt.-Cs. and to correspond to a perception on its own part of that method of working" (18, p28).

This paper also contains my first thoughts on the creative process that many years later led to my [19] book *Emotional Illness and Creativity*. At this point, in the 1957 paper my [16] thinking was immersed in ego psychology. I regarded creativity in one sense as a form of self-feeding exercise, in which the creator projects impulses from the id, elaborates and orders them, and then enjoys his own work through incorpo-

rating the material which he or she has projected or which, in the terms of Bion, the artist has created all by himself or herself as alpha elements from beta elements.

"This sounds like autobiographical narcissism to me," said George. I answered that even today, in those artists I have treated, this hypothesis seems to be basically valid. In the same paper [16] I argued that artists can be classified into three groups. One group seems driven by creative genius from their biological furnace, another group suffers from an incipient schizophrenia, causing "temporary regressions and exhaustion of ego libido," and a third group creates through a neurotic, anxiety driven process, utilizing a constitutional ability to shift levels of cathexis in the production of art in their attempt at mastery of the anxiety. In my 60 years of clinical practice I have worked with artists from all three of these groups. However, no details or case reports were given in this paper and, as if it did not cover enough topics, I closed it with a paragraph on so-called free will, which I conceived of as an intuitive conviction of a healthy ego, a sign of ego strength. I contrasted this with a pessimistic sense of determinism that reflects the ego's depressing subservience and weakness dealing with internal and external reality.

George asked, "Did you really think that in one paper you could tackle all these problems that have been debated for centuries?" I said these papers reflected my dawning amateur excitement about the capacity of psychoanalysis to investigate century-old philosophical issues, an excitement that began with listening to Apter's lectures in medical school. For me, Freud was and still is a source of the capacity to understand many thorny intellectual and emotional problems. I could even sense Freud's excitement as he wrote page after page of applying his discoveries to all sorts of social and individual issues. Sometimes he was wrong, but the methodology of his work truly represents a new science, a new mode of exploration.

Since these papers were written during my residency in psychiatry, it is no surprise that I [20] published with others a brief paper discussing the psychiatric ward administrator. We pointed out that unless specific attention is paid to ward administrative policy, there will be damage to the therapeutic atmosphere of the ward because

the policy will develop along the lines of least resistance and primarily for the benefit of the staff. One of my co-authors was Dr. Francis Gerty, the chairman of the psychiatry department, who was exceptionally nice to me. He took pity on what it was like with my new wife to live on \$100 per month and found me a job along with another resident (now deceased) to consult as his assistant to the Texas Medical Association on the mental health services in Texas. The results were published in a very nice pamphlet and he gave me a copy with a nice personal inscription. Although Gerty was a very well known administrative psychiatrist and high on the political ladder in the American Psychiatric Association, that sort of thing did not interest me but the payment certainly did. In fact, it was so welcome that I still retain an autographed picture of Dr. Gerty standing on a shelf in my office.

The residency programs at the University of Illinois were of many specialties but one could always identify psychiatric residents who, you remember, were assigned there by the Chicago Institute for Psychoanalysis and who were all in training psychoanalyses and hoping to be accepted to start courses. In the cafeteria and at parties our group was conspicuous by never wearing white coats, a kind of self-imposed segregation. On consultations I was asked by more than one patient, "You are the doctor? Where is your white coat?" Our group was more open, partied more intensively, and had more fun. But it was obvious that the doctors in the other specialties had no respect for us and regarded us as not being doctors, which to them was demonstrated by the lack of white coats and which generated a lot of hostility. Our attempt was to be "psychoanalytic" and avoid the authoritarian doctor image that predominated in the 1950s in order to remain neutral with patients and let them develop the transference out of their own psyche. Today, of course, that authoritarian doctor image has almost completely vanished in our culture. I have been to doctors who not only did not wear lab coats or neckties, they did not even wear socks! All this has come about in my country as doctors have become "health providers," surviving in an entirely different ambience around the medical profession. Whether that is for the good of patients or not is a matter of debate but the entire doctor-patient relation-

ship is different than it was in all specialties, and women are becoming predominant in the medical profession.

I served as the Chief Resident at the University of Illinois Neuropsychiatric Institute and around that time the famous neurosurgeon Percival Bailey gave a savage speech at the meeting of the American Psychiatric Association attacking Freud and psychoanalysis [21]. I did not know it then, but it was the beginning of the demise of psychoanalysis in American psychiatry. I was present at this speech, which was given in Chicago, and shocked at the hostility and sardonic tone employed. It was my first experience of highly regarded physicians even in our field of neurology and psychiatry expressing their hatred and distrust of Freud's work and of psychoanalysis in general. Bailey expressed bewilderment and doubt at the end of his long experience, beginning with an interest in psychiatry 50 years before and returning to that same field 50 years later, concerning the so-called revolution in psychiatric thought over that period. This was in response to psychoanalysis having become the basic science of psychiatry, which he considered a matter of faith and not science. He even quoted segments from Freud as if he were giving a religious speech and quoting the Bible.

In response to this, I [22] wrote that psychoanalysis should not be based on faith but on clinical experience from dealing in depth with individual patients. The test of psychoanalytic principles comes from whether they are useful in understanding patients when sitting in the interview room alone with the patient. These principles are based on clinical experience, not on faith and I claimed, as I do today, that Freud's "our science" was indeed a science. I insisted that the psychiatric resident must have a personal psychoanalysis and be immersed in the participant observation process with many patients to get a full grasp of the power of psychoanalytic understanding of human mentation and behavior.

I am still making that defense about 60 years later in many of my publications, even including my [23] latest one. Those who defend psychoanalysis in the field of psychiatry today are now a dwindling minority, but it is possible that circumstances will change. I will discuss this in considerable detail later. But now George said, "The case is still not proven for psychoanalysis.

It is a long way from the laboratory of Brücke where Freud worked on eels, and the brain tumor pathological studies that made Bailey famous, to the tenets of psychoanalysis."

In these publications, produced mainly when I was in my 20s, one can already observe some of the main themes of interest in my intellectual odyssey emerging. A few excerpts from my diaries, which now have grown to about 60 composition books, might be interesting in the light of what I have written so far; keep in mind this is the writing of a very young man.

Unpublished diaries April 3, 1955: "This period of my life cannot be viewed as anything more than a state of dynamic equilibrium between my natural tendencies and rational beliefs on the one hand, and the mass of pressures that move me from without. It leaves me with also a sort of feeling of suspension and a sense of awe at the power of the forces that toss one about in the world like a cork bubbling on the ocean surface.

October 15, 1955: I will devote all my spare time still to the manuscript – it is the hub of my intellectual life, around the periphery of which everything else is arranged. I feel possessed of genuine vital force only while I work on it – it is a genuine sublimation [my children were not born until 1958]."

George interrupted, "This was the time you wrote how I had become a shadow of the man. It was due to Serpasil, given to me for my high blood pressure, and you were embarking on one of your ill-fated manuscripts, one that you said you would dedicate to me. Clearly you knew that I was in trouble."

"I did George, but there was no talking to you about it. As I look back it seems like a death wish on your part stirred up by the automobile accident that left your elderly mother in a disintegrating state."

"Well, let us not get into what you call my Oedipal problem and psychosomatic disintegration – I know you published more about that later. Go on with the quotes from your diary in 1955, the year of your internship when you were supposed to be so busy," said George, obviously annoyed.

I continue with the diary:

"The situation now in the world is the loss of respect for the individual. What does philoso-

phy have to offer? What is it now? What could it be? What medicine has been and what could it be. How it could focus on the personality and study human living problems. What does literature have to offer? How it can focus on the human personality and study contemporary human living problems?

The basic premise of philosophy is the Socratic spirit of inquiry, the principle that one may achieve virtue through knowledge and a hierarchy of values, as in Plato. The emphasis on reason and habit by Aristotle and the individual-centered ethics of Aristotle – the key culmination of Greek ethical thought and the center stone of our argument. Spinoza as presenting the best metaphysical basis for mind and brain as attributes of the human substance. For Ezra Pound, the revolution in literature opening a whole new intellectual world, with attention to culture as the highest end and founded on the criticism of language. An individual-centered ethics with culture as the highest goal.

February 10, 1956: The needs and passions of man stem from the peculiar conditions of his existence. Those needs which he shares with the animals such as hunger, thirst, the need for sleep, and sexual satisfaction are important, being rooted in the inner chemistry of the body, and they can become all-powerful when they remain unsatisfied. But even their complete satisfaction is not a sufficient condition for sanity and mental health. These depend on the satisfaction of those needs and passions which are specifically human, and which stem from conditions of the human situation: the need for relatedness, transcendence, rootedness, the need for a sense of identity and for a frame of orientation and devotion.

Mental health is characterized by the ability to love and to create, by the emergence from incestuous ties to clan and soil, by a sense of identity based on one's experience of self as the subject and agent of one's powers, by the grasp of reality inside and outside of ourselves, that is, by the development of objectivity and reason. Ethics in the meaning of the Judeo-Christian tradition is inseparable from reason. Ethical behavior is based on the faculty of making value judgments and the basis of reason. It means deciding between good and evil, and the ability to act upon a decision. The use of reason presuppos-

es the presence of self and so does ethical judgment and action. Furthermore, ethics, whether it is that of monotheistic religion or that of secular humanism, is based on the principle that no institution and no thing is higher than any human individual; that the aim of life is to unfold man's love and reason and that any other human activity has to be subordinated to this end.

September 22, 1956: My Psychoanalysis begins! Paul Valery said, 'Fame is a species of ailment which one catches as a result of cohabiting with one's thoughts.'

May 20, 1957: From Aristotle: 'Our task is to study coming to be and passing away. We are to distinguish the causes, and to state the definitions of these processes considered in general as changes is predicable uniformly of all the things that come to be and pass away by nature.'

December 28, 1957: Projected book: Art and the Life of Man, 1. Philosophical basis of the problem; 2. Socratic spirit and focus on the individual; 3. On the springs of human action; 4. On the springs of creativity; 5. Examples from the ballet; 6. Drug addiction and the artist; 7. Foundation of modern humanism."

George interrupted, "What are the examples from the ballet you are talking about? What has this got to do with all your amateur philosophical musings?" I replied that throughout my three years of residency I made every effort to obtain as many and as large a variety of psychodynamic psychotherapy cases that I could, and I chased supervisors all over the city to get supervision of my treatments. I somehow knew I would spend the rest of my professional life doing this sort of thing and wanted to be as good as possible at it. I even remember the kindness of Dr. Apter, who agreed to meet with me for half an hour or so at 6:30 AM at Michael Reese Hospital before he started seeing his own patients at 7 AM. We watched the sun come up together. The only other sun-come-up experience I had was at Cook County Hospital on Saturday nights. The emergency room on that night was known as the butcher shop, because it was flooded with people enduring knife and gun wounds. Interns on the surgery service scrubbed in about 7 PM in the operating room and finished one surgery case after another until the sun came up and the day shift appeared at 7 AM.

Specifically George, here is what my diary thoughts were after doing intensive psychotherapy with a couple of ballet dancers, who could only afford treatment by a resident at the University of Illinois clinic:

"May 20, 1957: Ballet:

1. Non-verbal communication obviously
2. At ballet, the dancer must work hard and learn gradually, not by insight.
3. Men – emphasis on the genitals, lack of masculinity, very tight cutting and painful supporter "to avoid bouncing," I am told.
4. Taboo on sexual discussion – sexual and seductive aspects, even sexual seductive choreography romanticized.
5. Patient: "You dance for yourself and those who watch". Ballet as a silent repetition of the primal scene with the passive observer turned to the active dancer?
6. Patient's Oedipal worship of the male choreographer, her father was a gym teacher, identifies choreographer with me in slips of the tongue.
7. Excitement before and breathlessness after the performance suggests an orgiastic nature, like the Maenads.
8. Looking = an infantile sex theory. The "Naked Ladies" game.
9. Haskell: "Her face and her body are the instrument on which she plays."

George asked at this point why I did not work any of this up into a publication. I felt that I did not have enough cases and was not yet qualified to go deeper into the psyche using psychoanalysis, nor was I sufficiently familiar with the literature, for example Conyn's (24) *Three Centuries of Ballet* and works by Haskell (25,26) and Amberg (27), which were on my library shelf but not carefully studied.

The incisive, kind and thoughtful response of the famous professor David Rapaport (whose works have been collected by Gill [28]) to my (16) article on the sense of reality, time and creative inspiration mentioned above, which I had the arrogance to send him, asking for his comments:

"Dear Dr. Chessick, Thank you for sending me the reprint. I read it with interest. You asked for my comments.

1. I find it enjoyable to run into a psychiatrist who has interest in the Cantos and a few other such things.

2. The problems you touch on are real problems.

3. You tried too much as already your title shows. This never works: not even when one is not a beginner.

4. In regard to reality sense, for example, you did not find the most important papers: Hartman 1939, Freud 1911, Hartman, 1957.

5. You can't very well mix psychoanalytic theory with Sechehaye who is a descriptive, Odier who is confusing, and Sullivan who has a theory of his own.

6. It is unclear how you use the concept metapsychology.

If you ask for advice, pinpoint one problem, exhaust its psychoanalytic implications and if you want to push further gather other implications and use them but only if you can within your own compass define their relation to psychoanalytic theory. (By the way if you use any other theory as focus then the same rule holds). Otherwise you get a *mélange*.

Sincerely yours, David Rappaport"

Hearing me read this incisive and very kind letter, George now sounded very exasperated with me, grumbling, "When he told you that you missed the main references, that should have been a warning to you that your work was amateur, superficial, and a poor scientific paper. You never would have let such a paper emerge from our histochemistry laboratory – we would have lost our reputation and all our grant money!" "George," I answered, "You asked what I have been doing since your death and I am reporting it as honestly as I can. I am not proud of it."

I come now to the central figure in my residency experience, Franz Alexander. He is too well known for me to have to describe him here (see Chessick [29] for details). He came every week from the Chicago Institute for Psychoanalysis, of which he was the founder and head, to discuss a case presentation by a resident, and almost invariably he would find an Oedipus complex at the core of the disorder. He did some of his principal research on psychosomatic disorders at the site of my residency, and he was one of the few eminent psychoanalysts who were very encour-

aging to me and showed confidence in my future. This was consistent with his founding with others of the American Academy of Psychoanalysis, which encouraged questioning and debate about the principles and practice of psychoanalysis, in contrast to the very conservative American Psychoanalytic Association. Today our Academy is restricted to physicians practicing psychoanalysis and psychodynamic psychotherapy, but the American Psychoanalytic Association has been flooded with psychologists and social workers after a nasty lawsuit forced it to take on non-medical members. But it is true that Freud (30) would have much encouraged this change and indeed thought medical training was a hindrance to becoming a psychoanalyst.

At the time, I do not think Alexander fully grasped that the winds were blowing psychoanalysis out of psychiatry. He (31) wanted the total integration of psychoanalysis into psychiatry and a blurring of the boundaries of psychoanalysis and psychodynamic psychotherapy. He leaned heavily on Freud's famous definition that every psychoanalytic therapy is based on concepts of transference and resistance. He refined this by suggesting that a "corrective emotional experience" via the transference might be therapeutic. Such manipulation of the transference was considered a horrifying idea to the orthodox psychoanalytic powers that prevailed and Alexander was always kind of suspect after he published that. His [32] work on psychosomatic disorders was allegedly discredited, although I have always found his outlines of the types of personalities that tend to get certain types of psychosomatic disorders to be clinically useful starting points in my thinking.

Perhaps inspired by Alexander's psychosomatic approach, with my good friend and fellow resident Dick Bolin, we published a brief clinical paper on patients with psychomotor seizures [33]. We maintained that such patients had a traumatic early history, a struggle with unresolved conflicts around dependency needs, easily aroused rage and destructive impulses that were poorly integrated, lability of affects, an impoverishment of the ego from excessive repression, strong tendencies to deny and project, and a resulting psychological and physiological rigidity manifest in their everyday thinking and feeling. This was the first of my many papers

on patients with aspects that today would be labeled borderline, and I will discuss them when they became central to my career in part two of these memoirs.

1958-1960

While a resident, I did a great deal of teaching in order to pay for my training psychoanalysis and as part of my contribution to the University of Illinois Neuropsychiatric Institute. It was during my residency in 1957 that you died of a coronary at the age of 51, George, about the same age as my children are now, at the prime of life. I realized I was on my own as far as research was concerned. However, a surprise awaited me.

George said, "As a ghost I did not forget about you Richard, and such a plethora and *mélange* of publications now ensued that I wondered where you were coming from. And I am still your ghost, not yet an ancestor, and I still wonder where you are coming from!"

I was required to put in two years of military service when I finished my residency. I chose the United States Public Health Service and was assigned to their hospital in Lexington Kentucky. This "hospital" consisted of a federal prison for incarcerated drug addicts and a small side building housing psychotic patients who had broken down during their duty with the Coast Guard. I was appointed director of residency training and spent a lot of time teaching residents who were United States Public Health Service doctors training to become psychiatrists and assigned to work at this hospital. I also had ample time to take on a number of apparently hopeful cases among the imprisoned drug addicts for intensive psychodynamic psychotherapy and I continued with them for the two years that I was in Lexington and with a few of them I even continued after I left.

I also was lucky enough to make the acquaintance of Dr. Abraham Wikler, a physician who was doing a lot of psychopharmacologic research in what was called the addiction research center in the hospital and who was a professor in the department of psychiatry at the University of Kentucky College of Medicine. Wikler was a broadly educated man; rumor had it that he

endured a very bad experience in his personal psychoanalysis and decided to devote his life to psychopharmacology. I never had the temerity to ask him about this and we never discussed psychoanalysis, as it was clear he wanted nothing to do with it and thought it completely unscientific. He was a very interesting, pleasant, and unassuming person who approached recidivism in addicts as a problem of conditioning. He was an authority on all varieties of addictive drugs including some new ones at that time, especially LSD. With my early histochemical research experience I was able to work with him on several projects. I also enjoyed his interest in Spinoza and other philosophical topics but we did not publish on those things, we just had many discussions. So here I had in a sense a new mentor but, although we were on more equal terms than in the relationship I had with you, George, there was a certain distance with Abe that I think was the result of his discomfort with my main interest, psychoanalysis and psychodynamic psychiatry. That is a topic we did not discuss.

In retrospect I used my two years in Lexington very well and even included preparing myself for certification by the American Board of Psychiatry and Neurology, which I did pass the winter that I left Lexington. I also flew back and forth from Lexington to Chicago every weekend to continue my training psychoanalysis. It was a very busy time for me indeed, and also I became a father for the first time. I was delighted to be a father and from that time on I deliberately curtailed my practice and research to provide time with my children, much to the annoyance of my mother-in-law, who expected all doctors to be busy and rich.

I should not move on without mentioning my acquaintance in Lexington with Erwin Straus. A phenomenologist and a neurologist refugee from Germany, he taught at Black Mountain College and then moved to the Veterans' Hospital in Lexington. I brought my residents over there once a week to hear him give a patient demonstration. He taught those few psychiatrists who would listen to him to approach patients in a new way, complementing the standard medical approach of clinical neurology and psychiatric DSM with a much more holistic way of understanding, concentrating on the patient's conscious experiencing, a phenomenological approach. He had the

ability to take the most burned out, chronic, neurological patients and make their illness become clear and interesting by his approach, a sort of Charcot in Kentucky. From him I learned the elements of phenomenology in psychiatry and neurology and became acquainted with the usefulness of the phenomenological approach in working with psychotherapy and even psychoanalytic patients. It was to some extent a precursor of the emphasis on empathy by the self psychologists of a later time. Strauss had an uncanny ability to make every case a teaching case and simultaneously a demonstration of how a sensitive phenomenologist could elicit from any patient material a sense of vivid immediacy. He remained rather aloof, because I think it must have been terrible for him to have to flee his position as a highly respected phenomenological psychiatrist, with many publications, on the staff of the University of Berlin and end up first in Black Mountain College and then the Veterans Hospital in Lexington, Kentucky, located in a Midwestern small city with a hot, muggy climate and no cultural advantage at all. I did not forget his fine contribution to our residency program, offered without remuneration. Years later, at the Symposium on the 100th year of his birth in Heidelberg, I participated in discussion of his seminal ideas [34].

A flurry of publications from my work at Lexington resulted over the next few years. Of course because of the usual delay of publications after they are accepted in journals these all appeared in the 1960s. The first was on what I (35) called the pharmacogenic orgasm in the drug addict. Summing up my two years of experience with a substantial number of drug addicts in psychodynamic psychotherapy, I contended that the psychic mechanisms involved in the act of drug ingestion began very early in life with a threatened or actual loss of primal love. (36) I maintained that the damaged ego of the drug addict reacts to this loss with panic and regresses to an oral stage. Concomitant with this regression is the loss of secondary process thinking and a resomatization of reactions. The urge for passive object love is felt as a physical craving for a "fix." The process of injecting the drug is equivalent to the introjection of the ambivalently loved mother and results in the satisfaction of a primal love longing, where the breast

is placed in the mouth and satiation after feeding occurs. I felt that this incorporation of the drug leads to the pharmacogenic orgasm, a phenomenon consisting of a physiologic reduction of sexual and aggressive drives, a possible epileptic-like central nervous system discharge in the alimentary region, and a state of intrapsychic destruction of and fusion with the mother where she is tucked safely inside of the patient. This satiation of passive longing temporarily relieves primal love yearning and permits the patient to engage in a primal sleep, so warding off for the time being the threatened loss of primal love by denial, fantasy and a magic act. Over the years I have encountered a number of patients who, employing similar dynamics, compulsively force oral sex on women, using her as what self psychologists would call a selfobject, and reduce panic and depression in that manner. I think this dynamic may also play a role in compulsive rapists, although here the aggressive component overshadows everything else and is enacted to an extreme extent, sometimes ending with a murder.

A second paper [37] was that describing the asthmatic narcotic addict. The influence of Franz Alexander can be found here. We predicted that since many authors support the theory that asthmatic attacks frequently develop as a reaction to separation from the mother or mothering one, and there were other similarities in personalities, object relations and the extensive use of introjection in the fantasy life of both drug addicts and patients with depression, one would predict a higher prevalence of bronchial asthma in narcotic addicts. Our data confirmed this expectation. We concluded that the shifting between the following three states would be understandable: the narcotic addict is often depressed when he or she is not addicted, the asthmatic is often depressed when he or she has no asthma, and the drug addict loses the asthma and depression when he or she becomes addicted. The co-authors on this paper were two of the fine residents whom I was training and the general who was in charge of the entire base – one would dare not exclude him.

While in Lexington I gave a brief discussion to the Kentucky State Medical Association in which I (38) pointed out that cerebral structures have an overwhelming influence on when and how a

patient does or does not present himself or herself to the physician with the complaint of abdominal pain. I warned my fellow physicians that in making the differential diagnosis of abdominal pain it is important to keep in mind what the patient is trying to communicate by this complaint. I urged careful listening to the patient, which might save a lot of money and confusion and avoid a lot of unnecessary procedures. The influence of Straus and phenomenology was showing.

The serious research that I did that hooked together my studies at Lexington and my early studies in histochemistry, was published in two major articles [39,40]. "These were really classic papers," said George, "and the kind of research I expected from you, not like the alimentary orgasm and asthma stuff." These were some of the first studies on LSD, which at that time had just begun to appear on the scene. But I soon grew tired of this sort of tedious research; I am sure Freud must have felt the same way when he was dissecting eels in the famous Ernst Brücke's laboratory. I (41) tried to imagine how this kind of research fitted into psychiatry, whose primary goal was to change the mind of the patient. I reviewed some of our research on LSD and Wikler's (42) discussion of what constituted good psychiatric research, but I wrote that the giving of large non-physiological doses of chemicals in the hope of affecting one specific circuit or area or system in the brain is compared to looking for a needle in a haystack with a steam shovel. I pointed out that my histochemical studies demonstrated that there are enzymatic cerebral architectonics, which in many cases do not even follow the presently delineated anatomical structures, and for all we know these may be all important in brain functioning. I argued that the known biochemistry and physiology of the brain contribute little of much clinical use at present. The mind, I thought at that point under the influence of reading Vaihinger [43], is an "as-if" abstraction from the data of the doctor-patient relationship.

My 1961 paper grew out of the turning point in psychiatry, which was now starting to accelerate into full swing at the time, that eventuated in an almost complete split between psychiatrists today, who mainly prescribe drugs, and a mélange of psychologists, social workers and so-

called "Psy-Ds" who administer all varieties of psychotherapy. I attempted to explain this split on the basis of a false separation of the mind and body, since the mind, I thought then, could be viewed as an "as-if" abstraction from the doctor-patient relationship and was wholly dependent on the brain, and the body surely could be influenced by the mind, as anyone in love or in a rage knows. I thought "good research" could rectify this false dichotomy and gave an example from Wikler's [42] admirable work. Little did I know what was coming in the field of psychiatry! By 1980 the DSM-III from the American Psychiatric Association destroyed the commanding position of psychoanalysis in psychiatry. "I hope you will tell me more about this later," said George, "Since I know the reason you went into psychiatry as a specialty was because the commanding position in it was psychoanalysis." "Of course I will, George," I replied, "It was a shock and I am not sure I am over it yet!"

For the time being, however, I returned to my narrative of the early 1960s. Utilizing my Lexington experience, I then proceeded to publish a series of clinical papers on addiction. In a study with colleagues of the alcoholic narcotic addict (44) we were able to demonstrate that the shift from oral alcohol and barbiturate addiction to intravenous opiate addiction represented an additional regressive step when the patient was unable to gain stability through previous defenses, including alcohol, and it warded off deep depression or even paranoid psychosis. I (45) wrote about the problem of tobacco habituation, suggesting that patients habituated to cigarette smoking were seeking a psychopharmacological state of arousal to protect them against the deep fear that they will be destroyed, ruined or exposed if they let their ego defenses down. I added that tobacco habituation can be understood as an ego operation, which Rado [46] called a drug miracle brought in by the ego itself, which through artificial means keeps the ego functioning at a high level of alertness. I warned physicians who abruptly withdraw patients from tobacco to be aware that some of these patients will be unable to stabilize emotionally and have to be carefully observed.

Near the end of my time at Lexington I [47-50] came up with the idea of "periodic hyperingestion" that I first described in a long let-

ter to the Journal of Obesity. I think this is one of my most important ideas and I have seen it demonstrated over and over again in my clinical practice. These are patients who to the despair of their physicians and the panic of their families consume large quantities of substances or sometimes combinations of substances including opiates, barbiturates, marijuana, methyprylon, meprobamate, hydroxyzine, mescaline, alcohol, amphetamines, and food. At other times there was a certain amount of abstinence, sometimes almost complete. What would happen was that certain physical and psychic symptoms such as a plethora of various aches and pains, insomnia, anxiety attacks, twitching, depression, and so on would periodically build up and be followed by an explosion of hyperingestion in which the patient would even be functionally paralyzed. Much of the patient's energy was concentrated on a compulsive stuffing in of the various substances, for example, "I went to the grocery store and bought a whole cake and ate it all at one time." Periodic hyperingestion lasts from a day or two to several weeks and then abruptly stops, usually leaving withdrawal symptoms depending on the substance ingested. In the prodromal period patients have a great deal of difficulty in conceptualizing and verbalizing what is troubling them. They are just driven to hyperingest, sometimes even against their will and resolutions.

These phenomena seem clearly to be rooted in an early phase of development in which striving for security and other satisfaction is not yet differentiated, that is to say a basic disturbance in the primary mother-child unit experienced at the early stages of ego development on a somatic affectual level. The psychosomatic apparatus has been conditioned in the direction of tense and rigid preparation against ever-present danger. These patients are left in a state of "affect-hunger," which forces them toward the high cathexis of a set of primitive self-healing and cannibalistic introjection fantasies. So periodic hyperingestion is an attempt at self-therapy through a frantic reaching out process, a combination of the chemical effect of the ingested substance, and the hidden fantasy gratification behind the act of ingestion in itself is used for relief of the severe state of "affect-hunger."

The dynamics are similar to the dynamics of the alimentary orgasm described above. The patient is struggling with the desire to take in an ambivalently loved source of supply, originally the mother or mothering one. On the one hand, there is a great wish to fill up the longing and emptiness by ingesting the mother, and on the other hand, there is a tremendous fear of being destroyed by the malignant and hated part of the introject. The problem is solved by a fantasy operation of splitting the mother into a good and bad part, and then by compulsive stuffing with the good part and projecting the bad part out into the world, which is seen as composed of persecuting or annihilating objects. Indeed, the hyperingestion often leads to people making fun of the patient's obesity, to parents, clergy and physicians criticizing them for lack of morality and self-control and even, in the case of hyperingesting drugs, incarceration.

George said, "How on earth are you going to establish the truth of this kind of hypothesis?" I answered, "The attempt at formulation of deep fantasy life of these patients is very difficult, but it does represent what has been common to all the patients that I have studied intensively. My hope is that other therapists who are appropriately trained will contribute their experience on this subject." George went on, "This is a long way from the studies that you did with Abe Wikler! It seems that the scientific demonstration of these hypotheses is simply impossible. Perhaps you would have done better to have gone back to histochemistry. After all, you were highly trained in scientific method and laboratory research."

Letting this rhetorical comment pass, I went on with my narrative in a way that George was not very happy with and described [51] a case of the periodic hyperingestion of aspirin, warning physicians not to confuse the mental symptoms of salicylism with the original psychic illness. In this case, periodic hyperingestion of aspirin took place during certain depressed states. The prodromal syndrome, as I described it above, here resembled what Ostow [52] called the syndrome of ego impoverishment. Elsewhere I [53] pointed out that a huge variety of substances and activities may serve the same purpose depending on what that purpose is for any given patient. I tried to group these substances. The first

group, such as the opiates, produce a dreamy or fantasy state and help to deny reality, in addition to which they provide a form of substitute pleasure. These substances encourage passive ingestion and produce a fantasy world conducive to escape. The pleasure they produce has been called by Rado [46] a pharmacogenic orgasm and by myself an alimentary orgasm as described above.

The second group of substances have a stimulating effect on the central nervous system. They are sympathomimetic amines, cocaine, mescaline, LSD, and sometimes cigarettes, coffee and tea. These are related to denial of depressed feelings and can even be related to the denial of the onset of schizophrenia or of annoying or frightening schizophrenic symptoms. Many patients who are addicted to amphetamines and cocaine turn out to be basically paranoid and the so-called toxic effects of periodic hyperingestion of these substances, which can produce frank paranoid delusions and ideas of reference, actually emerge from the basic underlying personality.

A third group of substances are the hypnotic, sedative and tranquilizing drugs. The ingestion of these substances is also basically for denial, but unlike the first group they do not produce a dreamy or fantasy state, they just put the patient into a somnolent mood or even directly to sleep. Patients with a lot of deep or hidden anxiety often have recourse to these, which make them feel much better. Some patients ingest hypnotic and sedative drugs and at other times, cerebral stimulants, producing phases similar to a bipolar disorder. At the bottom of this are unsatisfied oral cravings and so, for example, the patient who denies depression and affect hunger by ingesting amphetamines during the day and then taking barbiturates at night is artificially undergoing, in a self-induced and sometimes iatrogenically induced way, the same sort of bipolar psychic variation. Often drugs obtained from the physician are utilized for this purpose instead of developing an addiction to food or alcohol.

A fourth category of substances, characterized by alcohol, are those in which different doses of the substances produce differing types of the three basic effects outlined above. For example, small amounts of alcohol stimulate the ego and relax the superego whereas larger amounts can

produce a dreamy state in which the superego is dissolved and the ego functions poorly, while further ingestion of alcohol produces sleep. What brings the patient to treatment is the increasing dependence on the addictive substance as a way of solving his or her life problems, interferes with normal personality functioning, and causes biological damage. But it is because these drugs work to relieve the intrapsychic disaster, and they work quickly and at whatever dose the patient wants, that patients who have been detoxified in various hospitals go home to the same life situation, begin experiencing prodromal symptoms, and very soon are engaging in periodic hyperingestion again.

"I am not very impressed," said George, "This kind of descriptive Kraepelinian approach, resembling what psychiatrists are doing with DSM-5 now, is not research and offers nothing in the way of helping these damaged individuals. What do you have to offer? Your psychodynamic therapy has a poor record with addicts, alcoholics, and overeaters." "Of course you are correct George," I replied, "But sometimes it helps to have a more precise understanding and evaluation of phenomena before effective therapy can emerge. We are still waiting for this, but I can tell you that incarceration is not an effective therapy."

My [54] final writings on addiction per se presented an appeal for a better understanding of the addictions, whether to food, alcohol, drugs, or whatever, as emotional disorders representing defective ego functioning and an attempt to cover it up rather than a crime or a moral issue. By placing this problem in the hands of the law, the medical profession has become intimidated and frightened to innovate with, approach, or even to have anything to do with addicts. I did not object to the criminalization of the sale of addictive substances but I felt that patients who possessed substances and were addicted needed more than incarceration. So I was then and I still am on the side of those who feel the huge governmental structure of agents chasing down people using drugs could be dispensed with to a great extent and the money that would be saved, which is a prodigious amount, could be spent on the rehabilitation and research into understanding of patients with these self-destructive personality disorders. So I have always tried to

characterize these disorders as medical problems rather than criminal acts. The sale and dispensation of addicting drugs is, of course, definitely a crime against humanity.

In the years that have passed, however, no progress has been made of any substantial nature in the direction that I and many others have suggested. I will not speculate on why that progress has not occurred. The progress I was hoping for I [54] compared to the progress that has been made in the treatment of witches, who at one time were considered to be evil persons that ought to be destroyed, but then finally, with the work of Charcot and Freud, became seen as suffering from hysteria, suitable for attention by the medical profession. This has not happened with addicts, and one of the reasons might be that there is a huge apparatus dedicated to chasing and incarcerating addicts in the United States, which would be decimated if the whole subject became medicalized. A great many people who are making a living from the pursuit of addicts and the prosecution of them would find their income seriously affected. Add to this the continuing fears and hysteria in the public stirred up by repeated announcements of the dangers of alcohol and narcotic addiction and of crimes committed by alcoholic and narcotic addicts makes it hard to sway public opinion in the proper direction. The worst victims of this calamity are those who suffer chronic pain and need opiates for relief, who have to go through all sorts of bureaucratic contortions to obtain them, all the while regarded with suspicion.

It is so complicated and difficult to obtain the necessary medications because of all these increasingly stringent legal regulations that patients with chronic pain are even afraid to travel on vacation from one state to another because of the difficulty in getting a refill of their prescriptions if they run out of pills! Some doctors are influenced by this also; for example, they will let a hospitalized patient in considerable pain suffer rather than increase their dose of narcotics. This cruelty seems to be a function of the personality of the doctor, but also of fear of getting in trouble with the so-called authorities who monitor the distribution of these medications. Some pharmacies will not carry strong narcotic pain medications even if they are given a legal physician's prescription to fill because they

just don't want to be bothered with all the federal and state investigations that would constantly harass them. The loser is the person with legitimate pain and suffering.

George said, "Well Richard, now you are into sociology, rather than research. I don't have any opinion on this matter but I am sorry to see you stray away from the laboratory into these murky regions where no proper examination of hypotheses can take place and no clarity can be established." "George, you will be happy," I said, "Because my next step, again rather surprisingly, was into more of the same kind of research that we did together!"

1960-1965

When I returned from required military service my children were 2 and 5 years old and another one was on the way to be born, so I was faced with the immediate demands of supporting a family. I decided to go into the private practice of psychiatry and applied for a position at the Michael Reese hospital, which had the best psychoanalytically oriented staff in the Chicago area. I went for an interview with the famous Dr. Roy Grinker and managed during the interview to light my pipe using several matches, which then produced a fire in his ashtray. I will never forget that embarrassing moment for the rest of my life. In spite of it he did accept me on the staff, but only a couple of months later there appeared in the mailbox of all the psychiatry staff members a demand for at least a \$3,000 contribution to the hospital building fund which I could certainly not meet. So I took a job as the Chief of Psychiatry at the Veterans' Administration Research Hospital, an elegant looking building which was located on the lakefront near the Chicago loop. At the same time I was appointed co-chairman of the psychiatric residency training program at Northwestern University. My colleague as co-chair was the famous Dr. Jules Masserman, whose picture appeared in many psychiatry textbooks based on his excellent research work making animals neurotic.

But I felt more and more estranged from research in spite of having just accepted a research position – "Maybe that's because you didn't have any good ideas for research," interjected George

rather testily – but I felt the only basically correct approaches to life were either the doing of good for other human beings or pursuit of knowledge for its own sake. The former was less satisfactory to intellectual thirst and hard to do in such a materialistic age, but as a physician there were many opportunities. The latter sounded more egocentric and perhaps more suited to the nature and function of man, as Aristotle would say. Of course, research can certainly lead to doing well for innumerable humans and it can be characterized as the pursuit of knowledge for its own sake, so again my discomfort seemed to be at working intensively on a very little or even remote problem rather than extensively on an immediate big problem. Freud seems to have felt that way. If he would have worked intensively at his discovery of the use of cocaine he would have become famous, but at the time he felt fame was more possible in working with patients like those demonstrated by Charcot. At any rate, in my new job I directed a rather small group of psychiatrists caring for veterans and a few researchers who were already there, including a physicist and a Ph.D. psychologist busily engaged in an unusual research project. I will describe only the research that was done while I was there and in which I took part, much of it an extension of what I had learned from Wikler at Lexington.

We [55] explored the biochemical mechanism of amphetamine toxicity in isolated and aggregated mice. It is known that aggregation of mice significantly enhances their toxicity to amphetamine and we tried to demonstrate that this is due to the norepinephrine depleting action of amphetamine. In three short papers (56-58) we were able to show that since the intensity or duration of biological action of circulating or locally released norepinephrine is limited to a large extent by what are known as “recapturing mechanisms,” interference with these mechanisms by drugs or other procedures may result in increased and prolonged pharmacological effects of exogenously or endogenously released norepinephrine. For example, inactivation of liberated norepinephrine by recapturing mechanisms is prevented by cocaine or amphetamine, resulting in prolonged and intense action of norepinephrine on tissue, leading to tissue damage or even death. We found that cocaine can cause

a small but significant decrease in the concentration of noradrenaline in the brain. This is important when compared with the size of the noradrenaline pool proposed to be involved in physiological nerve activity.

In contrast to amphetamine, the effect of cocaine seems to be identical under conditions of aggregation or isolation of mice. So there must be a different mechanism for the increased lethality of amphetamine and cocaine under sensory stimulation, or possibly the noradrenaline depleting action of the cocaine-induced convulsions obscures the differences between the isolated and aggregated mice. Decapitation of the mice showed that the norepinephrine level was protected by the instant freezing from the usual drop in brain norepinephrine after death. “Well, that’s more like it,” said George. “It’s good to see that there are some similarities in the research you did at that time to what you did in my histochemical laboratory.”

“You will like the next two studies even more,” I replied, “Since they (59,60) are almost modeled after the work of Wikler.” In the first paper we studied 30 species of tropical fish for their reaction to LSD-25. Eleven of these species had repeatable behavioral changes. It was possible to conclude that LSD-25 does not act on tropical fish mainly like a respiratory enzyme poison such as sodium azide or potassium cyanide. In the second paper we showed that pre-treatment with tryptamine, tryptophan and DOPA had no effect on the LSD reaction in the species of tropical fish we studied. We were able to conclude from this work that the effects of LSD-25 common to all the species were signs of non-specific distress. This includes the characteristic vibrating behavior and swimming against the glass that we saw. Although that in a sense was the end of my research on tropical fish, I have maintained a tank of tropical fish in my office as a memento to these experiments even up to today.

But I was getting increasingly concerned with problems in pharmacologic research and the poor general level of experimentation. With the psychologist McFarland, we [61] insisted that certain statistical and experimental design techniques had to be meticulously followed for the results of such research to be trusted. We listed a whole host of interfering variables in this kind of research, even in traditional double-blind re-

search protocols. For example, the side-effects of the drug may alert the patient to the fact he or she is getting a drug and not a placebo, or the color or the weight or the texture of the drug may alert the experimenter as to whether or not it is a placebo. We reviewed the principles of experimental design and of statistical evaluation of psychopharmacological research and warned physicians to look for these in evaluating reports in the literature. "This is very good work," said George, "But has it influenced psychopharmacological research?" "No," I replied, "There are a number of very poor studies in the literature – often sponsored by pharmacology corporations – and unfortunately the so-called meta-analyses of all studies on a given medication often mix together the good ones and the bad ones, making these meta-analyses unreliable even though they are excellent dissertation topics."

Several of our group at the VA Research Hospital [62] worked together to investigate the effect of morphine, chlorpromazine, pentobarbital and placebo on the "anxiety" associated with anticipation and experience of a painful interpersonal situation. "Anxiety" was measured by a large variety of autonomic nervous system indicators that were employed. Twenty-five paid medical student volunteer subjects in good health were tested. The painful interpersonal situation was a stress interview by a psychiatrist in an area of the subject's previously determined emotional difficulties that was previously delineated by the use of a battery of psychological tests and two hours of psychiatric interviews. On the day of the actual test we added to the subject's anticipation and the stress of the interview by doing the entire experiment in a hostile, threatening ambiance, warning them that the interview would be taped, judged by other students, and reported to their professors. The drugs were administered intramuscularly in a double blind random design over two days using different drugs, followed by a debriefing session.

We discovered that in the interview and experimental setting, when the subjects did not know what drug they were going to get, the results showed distinctively different physiological effects from drugs that would be expected to cause signs of decreased anxiety, such as decreased body temperature and heart rate, as compared with placebo. The interviews were ac-

companied by a subjective report of increased anxiety even though morphine and chlorpromazine are widely used for the relief of anxiety. The only conclusion possible, which was verified by interviewing the subjects after the experiment, is that in our experimental setting any physiologic effects of the drugs were perceived as a threat by the subject and this had a greater anxiety producing effect than the expected tranquilizing effect of the drug on the central nervous system. We concluded our experiment demonstrated that the setting in which a tranquilizing drug is administered can work in a contrary way to the known pharmacological action of the drug to an overriding extent.

The final research study with my colleagues that emerged during the time I was the chief of psychiatry in the VA Research Hospital [63] compared the effects of infused epinephrine, norepinephrine and placebo with the effects of induced anxiety, pain and anger on a variety of physiological variables in 18 medical student volunteers. A parallel between the physiological concomitance of affects and of drugs was found for only a few variables. When all of the drug changes and affect changes were compared, no clear patterns of correlation were apparent. The only resemblance of general physiological patterns that could be demonstrated was that between epinephrine and anxiety. It seems from this work that it is not possible to produce anger, anxiety or pain in a "pure" or unique form experimentally, although there is reason to believe that the physiologic response patterns to these affects do not significantly differ. Our experiment cast doubt on the possibility of producing any clear-cut affects in the laboratory, even further complicating the problem of seeking a parallel between drug effects and the effect of laboratory produced affect. I should add that this is the only paper I ever published in the *American Journal of Psychiatry*. George seemed quite concerned at this point. "You had a really nice research team and a number of interesting papers coming out," he said, "Why didn't you stay in the position you had and continue building a really good research organization?"

"George, I ran into a brick wall. I was on the faculty of the Northwestern University medical school and, as I said earlier, I was co-chairman of the residency training program along with Jules

Masserman. I discovered that Masserman, who later gave up his license for abusing female patients after putting them to sleep with barbiturates, had built a coterie of residents who were in analysis with him from our program. So there were some of our residents with him in analysis and some residents not, a situation that led to quite a conflict of interests. I protested and I felt that this was highly unethical and detrimental to the residents. Either he should have taken all of them into analysis, which of course was impossible, or let them have analyses elsewhere since there were plenty of analysts around. It represents one of the great shortcomings of a training program when a candidate is both analyzed and judged by the same person and the situation becomes even more disreputable when some of the candidates are in analysis with the training director and some are not. I think this makes it impossible for candidates to free-associate as they should in a formal psychoanalysis, knowing that they are also being judged for their capability to remain in the program and are in a privileged position with respect to other residents who are not in analysis with the training director! So in 1965, after much protest about this situation, I resigned my position rather than be a party to what I regarded as Masserman's exploitation of the residents, since my protests broke the old adage "If you want to get along, go along."

This was not before I published my usual couple of general psychiatry papers induced by the environment I was engaged in. Milton and I [64], in the spirit of Franz Alexander, discussed the psychosomatic aspects of peptic ulcer. Milton was a resident in internal medicine at the VA Research Hospital and we gave it as a contribution to Northwestern Medical School grand rounds; it was published in the Northwestern University Medical School quarterly bulletin. We said it now appears that there are three parameters which contribute to the development of a peptic ulcer: a physiological parameter, which involves tissue susceptibility, a psychological parameter, which involves a relatively specific psychic conflict, and a social parameter which involves a non-specific environmental situation that requires adaptation to stress. The tissue susceptibility issue has of course since been related to bacterial infection with *Helicobacter pylori*. Our paper concentrated on the psychological param-

eter and was based on Alexander's theory of the ulcer personality. It was more of a review than a contribution to science, but it shows that my interest in psychosomatic medicine was alive and well.

In a paper with Kaplan [65] we attempted to show how liaison psychiatry at the Veterans Administration Research Hospital was an important and effective part of all patient care. "This paper," remarked George, "Seems much like your paper [20] on the psychiatric ward administrator mentioned above, as it is simply a report of your clinical experience, not research. Why do you write such papers?" My answer follows:

The years 1960 to 1965 were ones of great uncertainty and great upheaval for me. Finishing my training psychoanalysis, I was emerging from psychiatry to psychoanalysis just at the time psychoanalysis was beginning to be extruded from psychiatry. My diary of 1960 opened with a quote from James Thurber: "All men should strive to know before they die/ What they are running from, and to, and why." I continued that this is the basic motto of my intellectual life. In my diary on October 7, 1962 I wrote: I believe work in biochemistry and physiology in psychiatry is done sometimes due to pressure from medical disciplines that we should be a "science" and by people who are either unaware of the deep powers of human fantasy or who by reasons of personality are unable to relentlessly pursue the depth of the psyche in order to understand such powers. Such so-called basic science research at this time is carried out often by the fringe people around psychiatry, those with Ph.D.s in experimental psychology, biochemistry, social work and so on who have never sat down for more than five minutes to leisurely talk to a patient without watching the clock, and occasionally by malcontents who have never been able to grasp what psychiatry is all about or are hostile to psychoanalysis, or who want to prove it is all chemistry anyway. Or like my gracious friend Abe Wikler, who wanted to confine it to various forms of conditioning (or what today is called cognitive therapy) and brain chemistry.

If we want to help psychiatric patients, we had better quit being biological scientists and learn to be men of leisure. This brings me to De Grazia's [66] wonderful book. Here is the prop-

er attitude. To tune in to a patient's fantasy life and to be able to understand his or her free associations one must be at leisure, to be on "slow time." Mental illness results out of interpersonal relations imposed on a constitutional and genetic makeup (and much more, but this was my thought at the time). It is the job of research to understand this makeup, but to be able to understand human psychic processes is essential to treat mental illness. Drug therapy is purely for symptomatic relief and that is fine but it does not put out the fire.

De Grazia chooses Aristotle's definition of leisure, contemplative life. He sharply separates this from free time which is nothing but recuperation from work. Leisure for De Grazia is a great rarity these days; and it requires a sacrifice, a state of being in which activity is performed for its own sake or its own end. These are such things as listening to or playing music, quiet contemplation, stamp collecting, chess, learning an ancient language, dining with friends chosen for their own worth, and above all the interaction with loved ones. To have leisure, says De Grazia, one must be free of the clock!

George interrupted here and remarked, "This is too rhetorical. There is nothing in the idea of leisure that precludes setting aside periods especially devoted to contemplation or music or the other arts while working very hard most of the time. Cannot parcels of time be put aside for a person to be alone with himself or herself?"

Here is De Grazia's answer in paraphrase: There are three reasons why people don't take leisure: 1) there is no tradition of it in that particular society, 2) endless television advertising is a force opposed to leisure and the absence of leisure brings on a different tradition ("shop 'till you drop"), 3) leisure is beyond the capacity of most people, says De Grazia. He maintains that culture is something you absorb as a child and many don't have the "stuff" it takes to enjoy leisure, that is, intelligence and temperament to enjoy leisure. For DeGrazia there is the great majority of people and then there are the leisure kind, those who love ideas and imagination, which is both a blessing and a torment. These are the ones who create culture. "I think this is really elitist," interrupted George, who seemed quite irritated by all this, "Leisure is an attitude of mind, an approach to life. Leisure and work

are not mutually exclusive. It is a mistake to connect leisure with detachment; the 'new texture' to our life brought about by leisure is not synonymous with withdrawal. It can only be provided by an inner conviction and determination of how one wishes to live, not by withdrawing and trying to contemplate."

My diary goes on to record De Grazia's claim that the point is to live a life of good quality, for "Design on the world, then, fractionizes your view. Not only that, it unsettles the mind. The crowding of desires, one upon the other, can shake a man's head until it rattles. In the end he has not only bias but confusion to contend with. To be objective, you must be tranquil" [66, p420]. I did not like the neo-Thomistic and regressive solution to 20th-century problems implied here, for, as Freud said, "It is impossible to escape the impression that people commonly use false standards of measurement – that they seek power, success and wealth for themselves and admire them in others, and that they underestimate what is of true value in life. And yet, in making any general judgment of this sort, we are in danger of forgetting how variegated the human world and its mental life are" [67, p64].

I did not withdraw, and from this starting point I set out into a busy full-time private practice with a lot of voluntary teaching of residents and medical students at Northwestern University. I was a popular teacher and won many awards as "teacher of the year" from the residents. Because I was entering rather late after my own residency into the competitive fight for patients, those people who wanted to send me a case at all usually sent me a borderline patient, the type of patient that nobody else wants. I set the stage for receiving these kinds of patients by having published two papers during the time I was at the Veterans' Administration Research Hospital. I presented the first one [68] as a proposed groundwork for research into the study of empathy and love in psychotherapy. This was before I was at all acquainted with the ideas of Kohut. I felt that in the psychotherapy of borderline schizophrenics, as they were called then, empathy would provide an important source of understanding of the patient. If such understanding is communicated back to the patient, I wrote, it leads to two kinds of phenomena. One of these is in the category of insight and provides the sal-

utatory effect of insight in psychotherapy. The other, at least in the psychotherapy of borderline patients, leads to the generation of positive feelings and even affection on the part of the patient, which can be studied and leads to the freeing up of their capacity to love. If it can be successfully accomplished it provides them a motivating force to do therapeutic work as a consequence of the reward for the patient's substantially improved interpersonal relationships.

Earlier I (69) began to be interested in so-called pseudoneurotic or borderline schizophrenic patients as they were called at that time, now all under the rubric of borderline personality disorder. I attempted for the general physician to give an idea of what the borderline patient was like and to help the physician establish at least a tentative diagnosis. I said very little about the treatment or psychodynamics but pointed out that getting to know the patient is mandatory before an accumulation of various signs begins to point to a diagnosis. I reviewed the "microscopic" and "macroscopic" indications of this condition and pointed out that spending some time in the office talking to the patient may save a tremendous amount of physician time later on and save the patient from needless medical and surgical procedures.

George looked dejected at all this. I told him those who are interested in laboratory research will find no more of it in my exposition because I now went further and further into an exploration of psychoanalytic and philosophical problems which were stirred up by my increasing clinical experience. "I am also interested in philosophical problems, especially when they pertain to science," said George, "but it seems to me you were a directionless failure as a research scientist and just fiddled around with whatever came along. If you insisted on psychoanalysis, why didn't you attempt the kind of research that Grunbaum [70] demands?" "George," I replied, "Like so many in the medical professions you do not understand. Freud gave us a new science, as he repeatedly called it. He brought us a new method of investigation and a new opening to the exploration of mental processes. I wanted to immerse myself in it, write about it, teach it, and derive as much clinical experience with it as possible and to use it as much as I possibly could to alleviate the suffering of my patients, so many

of whom had nowhere else to turn for help, often after having tried everything else."

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