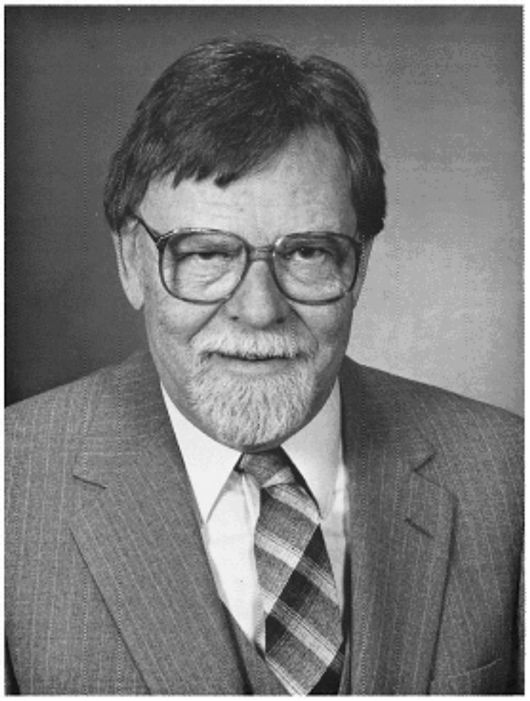
In G. Lindzey (Ed.), *A history of psychology in autobiography* (Vol. 8, pp. 337-389). Stanford: Stanford University Press, 1989. Contains additional material from first dictation.

#139+

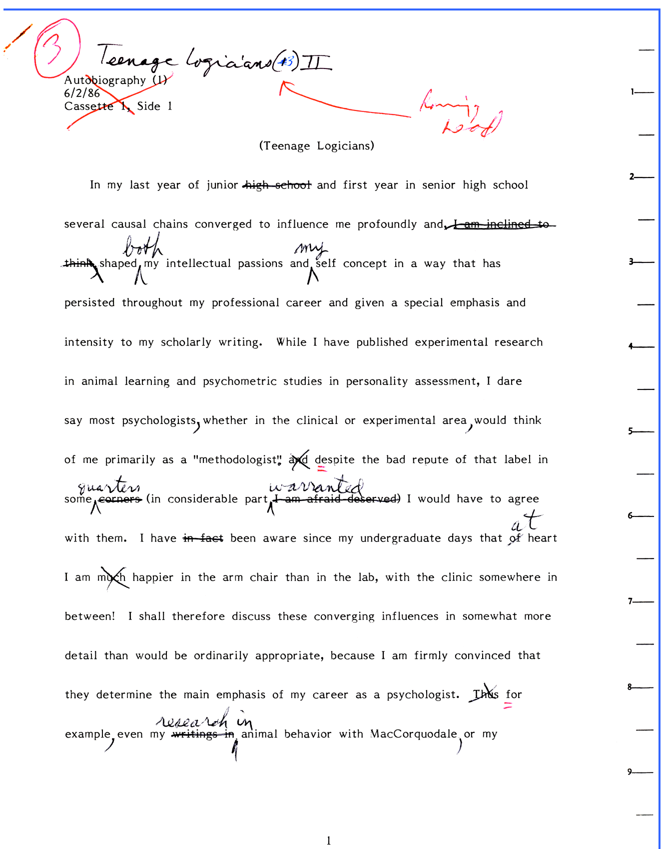


Paul E. Meehl

[In June of 1986, Paul Meehl began dictating his academic autobiography, quickly filling tapes that were distributed among secretaries with varying degrees of transcription skill, knowledge of specialized vocabulary, or experience with Meehl's dictation style. When raw transcription began to come back, Paul soon discovered that his usual calibration of tapes dictated to number of typed pages was so far off that he would greatly exceed the page range allotted. He put aside the first draft and started over with new dictation (using earlier-dictated paragraphs when he could).

Parts of that first dictation have been interleaved in the version here. Shaded material in this copy was omitted or edited to shorten the final publication and would have gone through more editing passes before submission. No attempt was made to check it for accuracy, refine grammar, or fill in citations. Readers should remember that less skilled typists might hear words incorrectly, insert punctuation incorrectly, or miss semi-explicit dictated changes (e.g., might type both a first-heard word or phrase and also the voice-implied replacement of it). Whenever there are more details given or suggested conflicts, it is still the published autobiography that should be considered correct and authoritative. This first dictation shows Paul in a more reflective mood and, being less finely edited, is closer to his characteristic speech style. There is overlap with the published copy, but the first version adds a broader range of historical detail that readers may appreciate.

—Leslie J. Yonce-Meehl, August 2015]



3 Teenage Logicians (#3) II

~~Autobiography (1)~~
~~6/2/86~~
~~Cassette 1, Side 1~~

Running head

(Teenage Logicians)

In my last year of junior ~~high school~~ and first year in senior high school several causal chains converged to influence me profoundly and~~, I am inclined to think~~ shaped both my intellectual passions and my self concept in a way that has persisted throughout my professional career and given a special emphasis and intensity to my scholarly writing. While I have published experimental research in animal learning and psychometric studies in personality assessment, I dare say most psychologists, whether in the clinical or experimental area, would think of me primarily as a "methodologist". ~~and~~ Despite the bad repute of that label in some quarters ~~corners~~ (in considerable part warranted ~~I am afraid deserved~~) I would have to agree with them. I have ~~in fact~~ been aware since my undergraduate days that at ~~of~~ heart I am ~~much~~ happier in the arm chair than in the lab, with the clinic somewhere in between! I shall therefore discuss these converging influences in somewhat more detail than would be ordinarily appropriate, because I am firmly convinced that they determine the main emphasis of my career as a psychologist. ~~Thus~~ For example, even my research in ~~writings in~~ animal behavior with MacCorquodale, or my

1

This edited page shows Paul's preferred working paper with a line-reference scale on the right, and his use of red ink for "instructions or comments to the typist" versus changes to text (often a fine distinction, but his secretaries seemed to grasp it well enough).

I was born January 3, 1920, in Minneapolis to Otto and Blanche Swedal; the name Meehl is my stepfather's. My ancestry is three-fourths Norwegian and one-fourth Scotch-Irish. In Norway my paternal ancestors were skilled tradesmen and school teachers, the maternal side were peasants. The Scotch-Irish maternal grandfather, whom I never met, was a salesman and a psychopath. My father was a bank clerk who, despite extraordinary intelligence, quit high school to help support a widowed mother and unmarried sister. He was fond and proud of me in a cool way, and I knew it. Fortunately I got his "brain" genes, because he held Admiral Rickover's view that if a man is dumb he might just as well be dead. I identified strongly with him. My mother was affectionate, nurturant, praiseful, but somewhat seductive, which led to sexual problems for me as a young adult.

Child rearing was permissive within firm limits. My parents laid down a few general rules, small in number, which to me seemed perfectly reasonable and which I therefore found easy to obey. The result has been an attitude toward the social group and toward authority figures which I consider healthy and rational, namely, it is foolish to break sensible rules imposed by persons in lawful authority over you; but given that, you are free to do your own thing and pay little attention to what other people expect, want, or approve. I believe that one reason I had such a good time both in high school and in college talking with my teachers is that I was totally devoid of rebelliousness or any chip-on-the-shoulder attitude. I had a mostly happy childhood, although my cyclothymic temperament was troublesome. "Paul, you're getting too excited," I heard often. In school I was an A student—my parents took this for granted—but with occasional "Unsatisfactory" marks in conduct, resulting from hyperactivity and a tendency to talk too much. At the same time, some of my childhood photographs look definitely depressed. I was a leader in my peer group in terms of what games to play and how. Puny and poor in sports, I suffered less from this than usual. As my father assured me, "That stuff doesn't matter, school work does. How many boys grow up to be baseball players?" I was aware of my intellectual superiority by age six or earlier. I liked school and was a "teacher's pet," but that rarely seemed to be resented by the others, for I was regularly elected president of our grade school classes.

I was born January 3, 1920 in Minneapolis of parents who on my father's side were full blooded Norwegians in ethnic origin and my mother half Norwegian and half Scotch-Irish. The name 'Meehl' is German (Americanized from 'Mehl') but this is my stepfather's name which I adopted on entering high school two years after my widowed mother remarried. My childhood name was 'Swedal,' somewhat Americanized from its original form 'Svedal,' pronounced the way my father heard it as a little boy in the home since in Norse the letter v is pronounced as in our letter w. My father's sister, a maiden aunt who played an important role in my childhood and adolescence (she was the only member of my immediate or remote family who had a college degree) retained the original spelling. My father's parents had come to Minnesota from Norway in the early 1890's, and my paternal grandfather, who was a tin smith by trade, contracted an infection and died of "blood poisoning" shortly after my father's younger sister was born I think about 1898. I know almost nothing about the family in Norway except that when my father's sister traveled there after her retirement she was able to corroborate the family story that there were a few peasants but mostly skilled tradesman and a scattering of schoolteachers.

It is interesting that one of the first things my paternal grandmother, one Bertha Svedal, did getting off the boat from Norway, was to buy a Norse-English dictionary which she studied assiduously. I remember her fluent English with good vocabulary and surprisingly little trace of a Norwegian accent except for the two things that are the last to go in persons of Scandinavian extraction: a slight "lilt" which I am good at imitating but do not know how to describe, and a tendency to use a soft s where in English it should be hard. Her attitude was that "We had come to a new country and we should speak their language," so that although once in a while you heard some Norwegian, such as "uffda!" if she dropped something in the kitchen, in general the use of Norwegian was discouraged and about the only time I heard it was when she or my aunt Signe would read first in Norwegian and then in English the gospel for Christmas eve. So far as I recall my father did not have a trace of Scandinavian accent, although like me he had a talent for imitation of speech patterns and was fond of putting on his Norwegian yokel act, sometimes to the irritation of my mother. My mother was born in 1892. My maternal grandmother was a second generation Norwegian who had spoken very little English as a child, her parents having come here from Norway in the 1870's and they were farmers in Wisconsin. My great grandfather on that side was a bearded Norse patriarch who disapproved of me on our rare visits to the farm in Amery, Wisconsin, because I talked quite freely and expressed my views and he thought children should be seen and not heard. My maternal grandmother (Bergina Norgaard) came to the city as a young girl. They were extremely poor at the farm and she began to work "in service" as what we now call a babysitter and so on when she was around nine years old, and as a result never went past the third grade in a rural school in which Norwegian was the language. She married a Scotch-Irishman named Duncan who so far as I can make out by the description of him by her and others who knew him during the short period they stayed married, was a textbook psychopath although at the time of their divorce he had never been in any serious trouble with the law. It is interesting how one can make the diagnosis confidently from people's account of him. For example, everybody said he was charming and fun to be with but irresponsible. You couldn't trust him to keep his word, yet as my grandmother said "He was so charming the women fell all over him and no matter what he did you would forgive him for it." I never got the details of his psychopathic behavior that led to the divorce except that from bits and pieces I gathered that he chased women, gambled his paycheck, and while not alcoholic was given to overdrinking at least by my grandmother's somewhat pietistic standard. She divorced him shortly after my mother was born, so my mother had no memories of him. Some of the paradoxes of my personality that my friends and relatives have observed I am inclined to attribute to having received some of his psychopathic genes along with a considerable dose of anxiety prone genes from my mother's side of the family.

My mother graduated from high school and worked briefly as a typist and secretary before she married. My father, despite his extremely high native intelligence and cathexis of intellectual and aesthetic activity, was a high school dropout. I was given to understand that while he got good marks in school he quit because the family was in need of money. It was already apparent that his younger sister was not interested in boys or likely to be very attractive to them, so it was decided that he should go to work so they would have money for his sister to get a college education, which she did and became a secondary school teacher. My father went to work as office boy in a bank and by the time I was old enough to be conscious of what my father did for a living he had risen to the slightly prestigious title of "chief clerk" in one of the big Minneapolis banks. He saw service overseas in World War I but was not in combat as the Armistice was declared very shortly after his arrival in France.

Although he talked very little about his youth, I was interested to learn (after his death, from my Aunt Signe) that as a high school student and for a while after he quit high school he was associated with a small group which he described as very bright and verbal boys whose favorite pastime rather than sports or hobbies was heavy arguments about politics, religion, adult occupations and the like. This makes me think of my quasi-leadership role in a similar group of teenage males described below. I, of course, have no psychometrics on him, but my father was extremely intelligent and it was obvious to me even as a little boy that his friends, most of whom were coworkers in the Northwestern Bank but some of whom he had carried over from his childhood, viewed him as a super-brain. He was reputed to be a brilliant bridge player and poker player. He painted and made chalk drawings as a hobby, he took all the special in-service training courses available at the bank, and he knew enough about architecture and architectural drawing to have been asked to prepare the plans when the bank moved to build a new building. He also designed the house we built when I was eight years old, saving a good deal of money by not having an architect do it.

He was highly motivated to achieve and the symbol of achievement was money, a fact which had catastrophic consequences as we shall see later. I was confident that he was fond of me as I was of him and I identified very strongly with him in my speech, mannerisms and activities. In those days, people worked a half-day on Saturday mornings and one of my good times was to accompany him down to the bank where I would be ensconced at one of the unused desks with my green eye shade like the other clerks and do make believe work. Paper, bound books, sharp pencils, erasers and office equipment generally have a positive valence for me, so that when I go into a store that sells such things I react as many people do in a candy store or bakery, I would like to buy everything in sight whether I need it or not. The interaction of capacity genes with socially acquired values is not something psychologists know much about, so I don't know how much of this kind of thing is responsible for my being at the 98th percentile (of the female norms!) on the Minnesota Clerical Ability Test. I have a hard time being tolerant of people who make frequent mistakes in copying or proofreading since I, even at my present age, am close to infallible at such activities.

While my father was at about average level for a middle class male in display of affection, and was evidently proud of the fact that I got good marks in school all along the way, he tended to take the attitude that <u>of course</u> I was bright and <u>naturally</u> I would do well, rather than to be markedly forthcoming in positive verbal reinforcement. This gave me some trouble as a graduate student and young faculty member since my advisor Hathaway took very much the same attitude toward his doctoral candidates. My mother was an unusually warm and affectionate person and as I dimly knew even before my psychoanalysis she was somewhat sexually seductive toward me. I was an only child, an object of a great deal of attention and affection from grandparents and my maiden aunt, especially because I was prematurely born as a result of my mother having to have surgery for acute appendicitis during her pregnancy, and there was serious doubt for several days after my birth whether I would live. When I hear people talk about children being "spoiled" I have trouble deciding whether I was spoiled or not. On the one hand I got what I would now view as probably an unhealthy amount of attention paid to me; on the other hand there were some pretty definite expectations and they were enforced. The difficulty of teasing apart what is transmitted temperamentally by genes and what is transmitted by precept, example, identification, and the reward/punishment schedule sometimes strikes me as almost undoable, although perhaps further development in measuring devices and the techniques of path analysis will enable us to unscramble such influences.

As a boy I was expected to keep my clothes and room neat and my hands and face clean and to have good table manners. It was taken as a matter of course that I would get A's in school and that I would "stay out of trouble" by not participating in neighborhood activities that would "bring the cops," and that I would address adults in a seemly fashion and promptly obey firm commands. I was suppose to be home after school unless I went to some other boy's house, in which case I was expected to say where I was. If somebody asked me to stay over for a meal I would call up to that effect, the point being that they wanted to know where I was, but neither my father nor my mother was particularly controlling once these basic regulations were obeyed. I think the reason that some boys don't call up when they're somewhere else is that they know if they ask if they can stay over at Mrs. Jones' house they will be told to "come home this instant" and not be given any reason for it. Perhaps the best way I could describe my parents' childrearing practices were that they were relatively permissive and not authoritarian for upwardly mobile middle class parents of the 1920's. What they did was to lay down a few general rules, small in number and to me, at the time, perfectly reasonable, which I therefore found easy to obey. The result has been an attitude towards the social group and towards authority figures which I consider healthy and rational, namely, it is foolish to break sensible rules imposed by persons in lawful authority over you; but so long as you do that, you should feel free to do your own thing and pay little attention to what other people expect, want, or approve. I believe that one reason I had such a good time both in high school and in college talking with my teachers is that I was totally devoid of any element of rebelliousness or chip on the shoulder. When I had a scholarly conversation with Paterson or Hathaway or Skinner in which we got into a big argument, I am sure that one reason they relished such conversations with student Meehl is that they knew I was not interested in playing silly games by "showing up the professor" or "winning the argument".

The neighborhood we lived in was a mixture of white collar and blue collar, the latter slightly predominating, and our house was about typical in size and quality with the others. But because my father's job as chief clerk in a bank was somewhat prestigeful in that neighborhood, and because as an only child I had more and better toys than most kids, I did have a mild feeling of being somehow "better" than the others. My personal style was such that this (hidden?) attitude was not resented and in the various group activities like cops and robbers or cowboys and Indians, I was generally the one that decided who was going to be whom, what area we would cover, what the "rules of the game" would be, etc. For some reason the way in which I went about this alpha baboon dominance did not bother anybody. So that while I was a somewhat puny boy and not good at sports, I did not suffer very much of the usual damaged self image young males have because of this deficiency, since I maintained an ascendancy in the game playing, and very much in the classroom where I was regularly the brightest kid in the class. I recall one time, when we were arranging how we would go about our Tom Mix scenario for the day, my mother calling me in and, without making a big fuss over it, telling me gently "Paul, you know you should try not to boss the other children around so much, they won't like you." This did not faze me but led me to tell her that while I understood why she said that, it didn't seem that it bothered them and that the reason it didn't bother them was that they had come to know I had more interesting ideas of what to do than they did. The age at which a child of superior IQ first becomes conscious of this difference has fascinated me in my psychotherapy practice where I have had patients of very superior intelligence who insist that it was not until sometime in late high school or even the first year in college when they became consciously aware of their intellectual superiority. I have always found this hard to believe but it seems to be true. I think I already knew by first grade that I was better at school tasks than almost anybody else around me. Perhaps my awareness of it came from identification with my father. Despite his lack of college education (or perhaps partly because of it), he tended to make a big thing about brains—about what people knew, what kinds of books they read, how well they reasoned and the like. In my father's value system, as Admiral Rickover was once quoted, "If a man is dumb he might just as well be dead."

I remember one specific event which highlighted the point and also tells something about my parents' values.

My parental home was wholly without racial or religious prejudice, although one grandmother admitted to a slight preference for Scandinavians. My mother's *n Nurturance* and liberal Methodism led her to view such prejudices as unloving, while my father considered them stupid, ignorant, and irrational. At age seven I had an experience that, with a succinct but powerful assist from my father, gave me insight into some unsavory features of the human condition, at least in its "herd" aspect. The families in our neighborhood were mostly Scandinavian, with a few Irish, Polish, and German, but there was one Jewish family with whose son, a boy my age, I had fallen into conversation on the way to school and we became friends. We argued about the Tunney-Dempsey long count, about politics (I was a Republican, as of course every bank clerk was in the 1920's, and he was a Democrat), and he was the first boy that I knew who "didn't believe in Jesus." I had a spotty exposure to a tepid Methodist Sunday school, and I found this theological divergence interesting to argue about. One day I saw a crowd of boys gathered on the playground, and when I got up close there was a boy confronting my Jewish friend who was simply standing, looking frightened, with a bloody nose and his glasses awry. The other boy kept poking him in the shoulder, daring him to fight, which the Jewish boy obviously did not want to do. I was frightened, puzzled, and angry, and I was ashamed of myself because I wanted to do something to protect him, but I feared the group who were standing around eagerly looking for more blood and calling out "Hit him again," "Sock the dirty Jew," and the like. I must emphasize that this school had mostly children from the middle and lower-middle class; we were not in a slum nor a violent crime-ridden area, and yet here was my friend being subjected to this treatment, as far as I could discern solely because he was Jewish. Furthermore, it seemed that *all* of the group were eager for the fight, that I was a 1-in-20 deviate!

I was troubled by this episode and my own timidity (although in retrospect I realized that if I had got into the act all that would have happened was that there would have been two bloody noses instead of one). I asked my father that evening why they did this, "What is the matter with them?" He put down his newspaper, looked at me somewhat skeptically, and said, "Paul, you mean you haven't figured out what's the matter with them yet?" I said no, I hadn't. He spoke three words, he tapped his head and said simply, "Dumb—no brains," and went back to his reading. This episode, and my father's three word diagnosis, "Dumb—no brains," carried more impact than my routine school experience that I could think, read, and speak better than my peers. I reflected on this for several days and my conclusion was yes, they were dumb. In fact, they were not just a little dumb—most of them barely had the wit to tie their own shoes or come in out of the rain. I have lived over half a century since then and have changed my views on a variety of subjects, but I must confess that I have never had occasion to revise that judgment. Needless to say, this expectation that, statistically speaking, most of the people you meet will be fair to middling stupid is one that you cannot afford to let be manifest if you want to win a popularity contest. Of course, a frenzied egalitarian could say that I have substituted an elitism of intellect for the more common snobberies of race, family, or money, a point I cheerfully concede.

Given a father with whom I could identify, a very affectionate mother and a good deal of attention and affection from grandparents and the maiden aunt, having had little trouble with health, and being highly competent at what Freud calls "the child's work" (i.e., school), I was a pretty happy child. So far as could be dredged up on the couch my recollection that I was, by and large, a happy child was not refuted by any strong memories to the contrary. However, I have reason to believe that my cyclothymic genes were manifest to some extent even then. One admonition I remember hearing frequently was "Paul, you're getting too excited." On the other side, I have a couple of photographs of me at ages five through seven or eight which I think any clinician would confidently characterize as looking depressed. Assuming the photographer knew his business—and having a picture taken of me was not the sort of thing on which my mother would have stinted, the kind of professionals she went to or the number of child pictures to take—I must conclude that I was depression prone as a child which I still am as an adult. However, despite this early experience of cyclothymic tendencies, I am convinced that my childhood was at least as happy as the average and my real opinion is [that it was] considerably happier than most. I felt loved and accepted, I had nothing to worry about, I had a good deal of freedom in deciding how I would spend my time, and I was not only good but unusually good at the main thing that I had been led to suppose was the "serious business of life," doing well in school. The only trouble on the academic front was something that appeared when we moved to the new neighborhood and to a different school at age eight, where for a couple of years my report card would have all A's in subject matter but "Unsatisfactory" in conduct. This unsatisfactory in conduct, which was very troublesome to my parents and puzzling to them and to the teachers, did not come from delinquency nor from uncooperative rebellious attitudes but simply and solely from a tendency to talk to my neighbor or show other signs of what I would now call my hypomanic tendencies. My childhood experiences in the family group, in the neighborhood, and in the classroom—despite this one phase of being too excitable—led me to an expectation that I would be good at things, and that people would like me and pay attention to me, which over the years has been fairly consistently the case. Once in a great while I find someone who seems to dislike me as a person, but this is so rare that when it happens I am not threatened by it but puzzled, my reflex response being, "I wonder what's the matter with him, that he doesn't like me?"

In 1931 my father, who had embezzled money to play the stock market, committed suicide. Taunts by classmates showed me human cruelty, and doubtless this (plus reading history) is why my view of mankind is closer to that of Freud and Luther than of Rogers and Rousseau. My mother began having frightening "heart attacks," and life seemed precarious indeed. At age twelve or thirteen I chanced upon Karl Menninger's *The Human Mind,* which was a healing Damascus experience. "Why, these fellows have it all figured out, the workings of the mind follow scientific laws, it's like my chemistry set! My mother isn't going to die of heart failure, she's a young widow with anxiety neurosis." I decided overnight to become a psychotherapist. My father's sister, a secondary school teacher, lent me some psychology books, and I devoured Woodworth's introductory text, Angell's *Psychology,* and Starch's *Educational Psychology,* my first exposure to statistics. Freud's *Introductory Lectures* were counterbalanced by the behaviorism of George A. Dorsey's *Why We Behave Like Human Beings.* These books were lifesavers, and I have never doubted the efficacy of bibliotherapy for the right people at the right time.

In 1931 when I was eleven years old there occurred a family tragedy that I believe damaged my psyche but indirectly had a beneficial effect by its impact on my vocational choice. Being a good student, enjoying studying and being achievement oriented, I had thought I might want to be a physician or a lawyer. My verbal fluency and enjoyment of argumentation led people in the family circle and others to encourage the latter. My father had, in connection with his bank work, purchased a six volume set of "commercial law for businessmen" which I read in its entirety (I think when I was around ten years old) and found it fascinating. I think it correct to say that my vocational aim in 1931 was to be a lawyer with being a physician as a second possibility, although it was obvious that my relatives did not think I was cut out for the latter, I believe because they perceived me as too excitable, and not as sympathetic to people as they thought a physician had to be to enjoy his work. Because of his high abilities and conscientious work habits, my father sometimes functioned as a member of a bank examiner crew. I don't know whether he was technically considered a bank examiner himself, but in any case he made several trips and had technical competence in that area. Being under a good deal of financial pressure from the too expensive new house he built in 1928, he began playing the stock market on margin and succumbed to the temptation to embezzle some money—it was around $2500 and he could have borrowed money from sympathetic friends if they had learned of his plight. After the big crash and further decline of the market in the early 30's he had no way to replace it. His technical skills enabled him cleverly to conceal this defalcation but he was under chronic pressure fearing discovery and, of course, feeling terribly guilty. To embezzle money is about the worst conceivable thing a bank clerk can do. A friend called him one afternoon in early January 1931 to tell him that his clever juggling of the books had been discovered. That night he wrote a note to his superior explaining how to unscramble the mess in the bookkeeping, and another note to my mother; and late that night drove the car into the garage and left the motor running. This was a ghastly, traumatic event for all of us. Within a few months after his death my mother began to have what she called "heart attacks" in which she had palpitations, tachycardia, dyspnea and thought that she was going to die. The family received assurance from her physician that she had a "functional" heart disorder but he never said what 'functional' meant, and none of us understood it as reassurance, although I suppose that's how he intended it. That one clumsy episode has made me acutely aware of the necessity for explicitly clarifying diagnoses to patients and relatives and I think I'm unusually careful about making sure that people understand my communications in such matters as a psychotherapist. If I were at a friend's home in the evening she would sometime call up and I would rush home because she was either having or expecting to have one of her "heart attacks." Needless to say, the threat of a second object loss under these circumstances to an early adolescent boy with more than the usual Oedipal problem was a source of chronic tension, and for a while my relations with the peer group, and my schoolwork, deteriorated from its previously high level. One day when I was at the home of a friend looking through his father's books, I came across Karl Menninger's The Human Mind which this friend's father, a civil engineer who was a secretary of the Minneapolis park board, had received as a Book of the Month Club selection a year or two earlier. I have recently reread Menninger in preparing to write this autobiography, and I told my class in philosophical psychology that I would still recommend it, despite its being essentially a popular book rather than a scholarly text, for reading by clinical psychology students since it still seems to me one of the nicest jobs of explaining psychodynamics that I have ever read. For me as a troubled twelve or thirteen year old it was kind of Damascus experience, and it had an immediate and—I must insist to skeptics—a permanent therapeutic effect. From that one episode in my own life and its profound influence on my subsequent career I have never doubted the power of bibliotherapy for certain people under certain circumstances. Although like most professionals in the field I have some disdain for the poor quality in the self-help books, many of which combine shabby psychologizing with even shabbier ethics, I am somewhat more tolerant of such books than most psychotherapists. Without having any hard data, I am quite confident that at least some subset of troubled persons, perhaps a small minority but not negligible, receives significant and permanent benefit from reading them. It also gave me a conviction that has doubtless affected my psychotherapeutic practice in a variety of ways, namely, that we should not underestimate the importance of n Cognizance in at least some kinds of patients. Having a set of plausible, explanatory concepts to "hang your symptoms on" is inherently gratifying and anxiety reducing, at least to many people and not only to theory oriented intelligentsia. People want to understand what is happening to them, and being cognitively disrupted even at an impersonal and elementary perceptual level (e.g., the Adelbert Ames setups which make some college students anxious despite no objective threat of danger or social rejection) is threatening. I don't infer from this that all explanations, including ones that are totally without objective validity, are equally efficacious—my psychoanalytic background is too strong for me to say that—but any explanation of puzzling personal phenomena of mind or body, if not too far fetched, has a soothing effect upon the mind!

In any case, my reaction to the Menninger book was immediate, intense and pervasive. Not knowing any better at the time, I found it easy to accept practically everything in the book. It was apparent to me where my mother fit in Menninger's list of "personalities", namely, she was the "neurotic type", and she was an affectionally and sexually deprived young widow under financial pressure who was suffering what were perfectly obvious episodes of anxiety-attack. She wasn't going to die of heart failure, and the mysterious phrase "functional heart disturbance" was obviously to be translated into this harmless significance. I changed my vocational goal overnight from that of lawyer to psychiatrist. (At the time, I did not know there was such a thing as a clinical psychologist, and Menninger's treatment of the psychologist's contribution in that book is negligible.) However, the combination of anxiety reduction and intellectual excitement from Menninger's book led me to poke around in other books in our house and to ask my Aunt Signe if she had any books about psychology or psychiatry. As a graduate of the school of education she had taken psychology courses, so it turned out she owned a copy of Woodworth's first edition, James Rowland Angell's Psychology, and Daniel Starch's Educational Psychology. My reaction to Menninger's book might be phrased something like this: "This stuff is terrific, the mind and its effects upon the body do not have to be mysterious, fearsome, unpredictable, baffling things. These fellows have it all worked out in scientific detail, they know how the mind works, they know how to repair it when it's busted. Why it's practically like my Gilbert chemistry set, it's all scientifically understood!"

I was, however, saved from becoming a dogmatic doctrinaire Freudian because these other non-Freudian psychology books my aunt gave me looked at things in a different way although at that time I did not view them in any important sense as being incompatible with the Freudian view of personality and neuroses. There was also an immediate tie-in between Starch's educational psychology book (which I found almost as fascinating as I did Menninger although it didn't deal with emotions and neuroses) and some books by Wiggam that I came upon of within a year or so of reading the Menninger. Albert Edward Wiggam, in his Marks of a Clear Mind, poked a good deal of fun at politicians and one of the things he pointed out was that as a newspaper man he had listened to hundreds of politicians defend their views or propose solutions to various problems, and he had never once heard a single politician, even a bright, educated and honest one, refer to the probable error of his opinions. One of the marks of the scientist, said Wiggam, is that he attaches a probable error to his results. In Starch's Educational Psychology there was a presentation of elementary statistics, means, sigmas, correlations and the like, as well as probable errors. So the idea that the psyche could be understood and to some extent measured found a natural affinity with the "scientific" value orientation of the little group of teenage logicians discussed in the next section. It is tempting to trace the psychodynamic and social origins of my most impactful single publication, Clinical Versus Statistical Prediction (1954), to the confluence of the bibliotherapeutic effect of Menninger's book with its psychodynamic orientation, and the concepts of statistics and mental measurement from this other set of influences. Another book that fell into my hands about the same time, I think a Christmas gift from my Aunt Signe, who (having been disturbed by the transitory decline in my academic performance in junior high, before I was inspired in ninth grade by a certain science teacher) wanted to encourage my new psychology interest and gave me the anthropologist George A. Dorsey's, Why We Behave Like Human Beings, which took a non-Freudian behavioral approach and which also was the first book I ever read which explained about amino acids.

At age sixteen I suffered a second object loss when my mother (who had remarried when I was fourteen) died of ether pneumonia after surgery for a brain tumor. Her physician, an internist of high reputation, had diagnosed her as having Meniere's disease and must have never rechecked her neurologically (even eye-grounds!) while her condition steadily worsened over a year's time. The neurologist we finally called in (against her will) observed a definite choked disk and correctly localized the tumor. This episode of gross medical bungling permanently immunized me from the childlike faith in physicians' omniscience that one finds among most persons, including educated ones. It has also helped me to avoid dogmatism about my own diagnostic inferences, to which I am tempted by my self-concept as a naturally gifted and well-trained clinician. After her death I lived briefly with my stepfather, then for a year with a neighbor family (so I could finish high school), and after that with my maternal grandparents who lived conveniently close to the university.

In my last year of junior high school several causal chains converged to influence my intellectual passions and my self-concept in a way that has persisted throughout my professional career. While I have published experimental research in animal learning and psychometric studies in personality assessment, I dare say most psychologists think of me primarily as a "methodologist" (cf. Meehl, 1950a, 1959a, 1967a, 1971a, 1972b, 1973a, 1978a, 1986a; Cronbach and Meehl, 1955: MacCorquodale and Meehl, 1948b; Meehl and MacCorquodale, 1951b; Meehl and Rosen, 1955). I am happier in the arm chair than in the lab, with the clinic somewhere in between! I shall therefore discuss these converging influences in some detail.

I shall therefore discuss these converging influences in somewhat more detail than would be ordinarily appropriate, because I am firmly convinced that they determine the main emphasis of my career as a psychologist. For example, even my research in animal behavior with MacCorquodale, or my empirical publications on the MMPI, show a heavier emphasis on methodology of science and even general epistemology than one ordinarily finds in psychologists' papers.

In the ninth grade there was a course called General Science which almost all the intellectually able students took because the teacher was reputed to be so stimulating. This teacher, Victor H. Smith, was of unusually high intelligence and unquestionably had the brains to be a teacher at the college level had he so chosen. He compared his attitude towards junior high teaching with that of a Jesuit priest, the old saw about if you get them when they are young they will never get the Church out of their blood. He looked upon the teaching of general science to adolescents as "already a bit too late" for counteracting the irrational and unscientific ways of thinking that they were exposed to by the peer group, in the home, and in Sunday school. While he had no appreciable interest in day-by-day political happenings (he despised politicians as a genus and used to make sarcastic asides to that effect in his lectures), he was by no means devoid of social welfare drives or values. His view was that while there were certain unavoidable characteristics of the human condition, including natural catastrophes and the inevitability of death, a large portion of all human misery was in principle remediable if people did not think so irrationally and unscientifically about practically everything. This melioristic view of improving society by teaching young people scientific habits of thought was quite common among scientifically trained "emancipated" persons in 1934, but in recent years has fallen into disrepute. I have myself become more pessimistic, as would anybody who lived through the horrors of fascism and Stalinism, not to mention the Great Depression, but I still see more merit in Smith's position than do many contemporary intellectuals (Goebbels and Robert Ley had Ph.D.'s, but they did *not* think scientifically!). …but I must say that I still see more merit in Smith's position than is true of many contemporary intellectuals, including the young. I am not, for instance, impressed with the argument that "science didn't prevent Hitler," since I have no reason whatever to believe that the majority of his passionate followers, including many with advanced degrees, ever learned any habits of thinking scientifically about things in general. It is possible of course to learn to think scientifically about the benzene ring (as did Dr. Robert Ley, a chemist) without having any inducement to generalize it to think scientifically about the human mind or society.

Whatever the merits of Mr. Smith's views on science and society, he managed to convey his passionate commitment in his teaching. He was not one of those teachers who believes that you can teach young people to think without teaching them any facts or principles to think about. Every day's lecture and demonstration was capped by his dictating to us (into what we called our "fact book") anywhere from one-half dozen to one dozen *facts* or *principles,* and we knew that we would be held responsible for learning all of these. Whether he was talking about weather prediction or the way American houses are constructed (he told us that the housing industry was always running at least 100 years behind the times in terms of cost, hygiene, and comfort) or the atomic theory or the effects of drugs, he would almost always add to the purely scientific content some comments about what would happen in the world if people really understood this, took it completely seriously, and applied it in their affairs. Doubtless the rebellious spirit of teenagers found this poking fun at preachers, politicians, journalists, and so on much to their liking even when it was not entirely objective. But he was not a fanatic or a cruel man, and on balance I am inclined to think that mobilizing a little bit of adolescent aggressiveness and intellectual muscle flexing did more good than harm. In any case, the dozen or so male friends that I mostly associated with at the time were all moved and shaped by this man. Among us the word "unscientific" came into everyday use and was one of the worst things you could say about anybody. Of course this theme meshed well with my father's views on stupidity.

In 1934 I read Bertrand Russell's *Our Knowledge of the External World,* my first exposure to epistemology. I cannot explain why it fascinated me, although during my analysis we spent some hours on this question with the usual scopophilic possibility, which I did not reject given my mother's tendency to seductive exhibitionism, but which never really grabbed me at the affective level. My analyst had a lot going for this interpretation (e.g., I respond to literature and music but am blind to the visual arts; as a teenager I was fond of "shadowing" people and was preoccupied with telescopes and a toy periscope with which one could look around corners).

During that same year my Aunt Signe took me on my birthday down to Dayton's bookstore to let me pick out my own birthday present and I fell upon a copy of Bertrand Russell's book Our Knowledge of the External World. Since I had read a little bit about Russell and found some of his political and theological views to my taste, I selected this one over the strenuous objections of the salesperson who kept telling my aunt that "This book is far to difficult for any 14 year old, even a bright one, to get anything out of." I daresay I insisted upon my choice partly to refute her, but the fact is that having reread the book a couple of times as an adult I recall quite clearly which portions of it I didn't understand at the time and which portions I did. I understood most of it. To the best of my knowledge this was my first exposure to technical epistemology, and I am not prepared to explain why it fascinated it me so much although during my analysis we spent some hours on this one with the usual scopophilic possibility which I did not strenuously reject in the light of my mother's tendency to exhibitionism but which I must say never really grabbed me at the affective level. My analyst had a lot going for this interpretation (e.g., I respond to literature and music but am blind to the visual arts; as a teenager I was fond of "shadowing" people, and preoccupied with telescopes and a toy periscope with which one could look around corners.)

About this time I read *A College Logic* by Alburey Castell (the "Augustine Cassell" of Skinner's *Walden Two)* and was as entranced as by Menninger and Russell. I read all the logic texts in the public library and prepared a summary of logic and scientific method which I circulated among my friends. This "Young Logician's Group" had a feeling about rationality that was as passionate as some boys of this age are about sports, politics, religion, or the Boy Scouts. In order to be *persona grata,* you had to be smart, and you had to think rationally. There was substantive variation among us in politics and religion, the group including a Roman Catholic, a Lutheran, several atheists and agnostics, some liberal Protestants ("on the way out"), and one Buddhist. Politically we ranged from Marxist—even one who was (as one could still be in 1935) an apologist for Bloody Joe Stalin—to a conservative Republican. So you could have a variety of political and religious opinions, but you had to do a respectable job defending them. An atheist who used dumb arguments would have been less acceptable to this group than a bright, articulate, highly rational Roman Catholic.

The following year, when I was 15 and in my first year of senior high, an older brother of my then closest friend who was in pre-law at the University came over one evening to show us his copy of Alburey Castell's A College Logic which had just been published and was being used by Professor Castell (of whom more later in connection with Fred Skinner) as a textbook in his freshman logic class. Castell's book, long out of print, I still view as one of the best pedagogical jobs I've ever seen in a beginning logic text, partly due to the punchiness of Castell's verbally aggressive style but also because the book begins after a very brief chapter on the nature of argument as "discourse containing inference" with a long chapter on fallacies. My friend and I were fascinated to discover that the mistakes in reasoning that we complained of in some of our peers and teachers, and which those peers with less permissive parents than we had had to put up with on the home front, had actually been catalogued and labelled like chemical elements or butterflies. As a result of reading this book, I proceeded over the next couple of years to check out all of the elementary logic textbooks available in the public library and the budding pedagogue in me led me to type up a summary of the results of my reading which I made available to my teenage brethren. This combination of the science teacher's propaganda and our reading of Castell's logic put a stamp upon our little "junior scientist" group that is unusual and which, in fact, I have not found any of my colleagues in various sciences reporting as having taken place in their teens. In most respects the peer group, which included a half-dozen intimate friends and another dozen or so who were more peripheral to the group, did the usual things that bright upper middle class teenage boys interested in science tended to do. (I should mention in this connection that less than half of the parents in this group had gone to college and only one of them had studied science, but he was a businessman trained to the bachelor's level in chemistry.) For instance, we belonged to the high school science club and most of us also to the chess club and a couple of us to the creative writing club. We all had home chemistry sets, and a favorite topic for debate was the relative merits of Gilbert and Chemcraft [two choices of chemistry sets at the time]. A dozen of us organized our own off campus science club. We met every Friday afternoon at one another's homes and somebody was responsible for presenting a paper which would be followed by a discussion. All of the people with whom I was in the habit of discussing logic, metaphysics, epistemology and "scientific method" were members of this science club but not conversely. The four teenagers I spent most time with and had the closest male bonds with subsequently became professors of physics, architecture, experimental psychology and consumer psychology. While we shared the substantive interest in the various sciences with the larger group, this small group had that special interest in logic and epistemology and, unlike most of the members in the larger group, were also interested in the process of argument. So that for this subset the methodological interests were actually as strong as the more usual substantive interest in various sciences.

While everybody who was interested in becoming a scientist and especially those who had taken Mr. Smith's course a year or two earlier was committed to thinking rationally and factually, that is, you should use logic and mathematics in your reasoning and you should base your opinions upon objective data, the smaller group, which I may call the "Young Logician's Group," had a feeling about rationality that was as passionate as some persons of this age are about sports or politics or religion or the Boy Scouts. In order to be persona grata with this group, even to be only peripherally connected with it, the criteria were that you had to be smart and you had to think rationally. There was substantive variation among us in politics and religion. The group including a Roman Catholic, a Lutheran, several Atheist and Agnostic, some liberal Protestants ("on the way out") and one Buddhist. Politically we ranged from Marxist—even one who was (as one could still be in 1935) an apologist for Bloody Joe Stalin—to a conservative Republican. He was the one whose chemist father was a fairly wealthy business man. The interesting point is that you could have a variety of political and religious opinions, but you had to do a respectable job defending them. An atheist who used dumb arguments would have been less acceptable to this group than a bright, articulate, highly rational Roman Catholic.

The extent to which thinking straight as such dominated or provided the social cement and furnished us with self-concepts can be illustrated by the following fact, which again I find unique in my experience when I have talked with others who belonged to adolescent male groups of this sort. We used to take long walks in the summer around Lake Nokomis or Lake Harriet and flip a coin at the beginning of the walk to decide who would be the Buddhist and who would be the agnostic or Roman Catholic, or who would be a socialist and who would be a conservative Republican. Thus you found yourself defending a position that was not your own based upon the flip of a coin, and the point was to see how good a job you could do at it.

We were never harsh with one another. It was excusable to make a mistake, although if you made too many egregious ones you would not be well accepted; but the unpardonable sin was to *refuse to recognize* that you had committed a fallacy, formal or material, when it was pointed out. A close second major sin would be to keep committing the same fallacy over and over again and having to be reminded of it. This kind of experience as a teenager, which persisted through my high school and undergraduate college days, I am afraid "spoiled" me as regards the life of the mind in academia. I had the expectation when I became a faculty member that anybody with the brains to get a Ph.D., who had taken courses in statistics and logic and the like, could be depended upon to be 95 percent rational, an expectation which was rudely upset by subsequent experience in faculty meetings and committees. While I have mellowed with age and become more tolerant of other people's frailties (as I hope they are of mine), I must confess that I have never fully recovered from the shock of realizing that one can become a college professor and not be able to think straight. This has led to a note of petulance creeping into my scholarly publications, for which I have been faulted.

The emphasis on rational argument and the ability to defend an opponent's point of view effectively meant (perhaps strangely) that intellectual one-upmanship or skill at verbal fencing, just being good at "winning the argument," were strongly disapproved of. This attitude has persisted into my adult life and old age. When I meet an academic who is an intellectual show-off—especially one for whom scholarly controversy has the character of a pissing contest—I lose interest in talking with that person. I look upon the intellect as a kind of sacred thing, and to have a conversation with the aim not of getting at the truth, clarifying one's ideas, or exploring new possibilities, but rather putting the other fellow down, showing that you are more resourceful and agile at debate, seems to me a corruption of the intellect and—speaking less melodramatically—a silly way to spend one's time.

To anticipate, not a single member of the 1938–1945 Minnesota psychology faculty engaged in this kind of vulgar gamesmanship. After I took my Ph.D. and began to move around the country and deal with professors from different academic subcultures, to find that there were quite a few who viewed an intellectual conversation like a chess game baffled and disappointed me. After all these years, I am still mildly surprised when I come across a flagrant case of it. Since I myself am intellectually resourceful and verbally fluent, and others tend to view me as "intellectually dominant" (by which, I think, they do not mean I am domineering), I am reasonably sure that my distaste for this kind of pseudo-intellectual discourse stems not from the fact that I am a loser at it, but from my belief that it's pointless, and a kind of spiritual corruption.

At age fifteen I decided to be a college professor, which troubled the high school counselor because I hadn't chosen what to profess. I said I might do psychiatry, psychology, philosophy, or statistics, but whichever it was, a professor I would be. It seemed clearly the only life for my sort of person, and he couldn't dissuade me from that conviction.

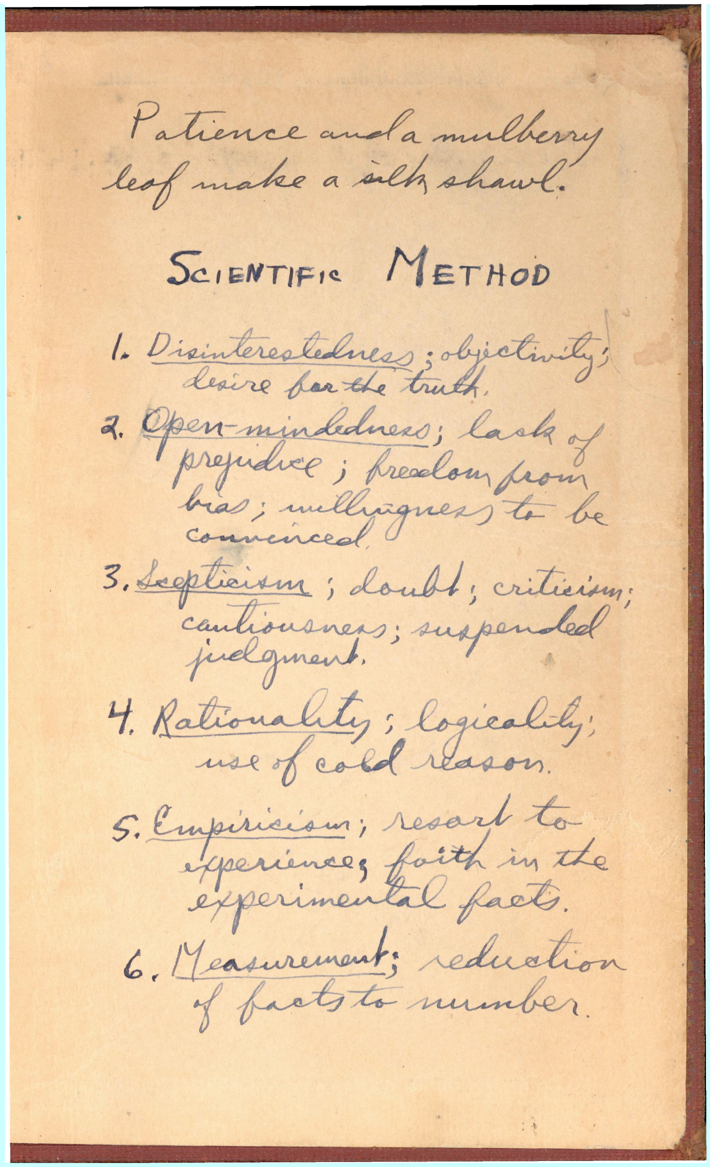
In my freshman year in high school I had the required interview with a precollege counselor who was not a psychologist but a chemistry teacher (and also the football coach) because in those days we didn't have school psychologists—there was only one in the Minneapolis School District. Filling out a preinterview form, where it asked what my vocational goal was I had written "college professor." The part-time counselor (a Mr. Wells, who was a bright and kindly man) pointed out to me gently but firmly that the blank was asking for a field of learning, not an academic position. As he patiently explained to me, "Paul, you see, this is not quite the way it works. The way it works is you write down here 'chemist' or 'lawyer' or 'forester,' and then if you're smart enough and lucky enough to be there when a rare job opens up (this was 1935!) then the University <u>might</u> ask you to stick around and be a teacher. But you can't just put down 'professor,' you have to put down what you're going to profess." I was not a rebellious youngster and I immediately sensed Mr. Wells kindly intentions to clarify something he thought I was mixed up about, but with equal firmness I repeated that my vocational aim was to be a college professor and that I couldn't put down lawyer or doctor or whatever because I didn't know yet what it was that I wanted to profess. I said that I had thought of becoming a physician as I was interested in psychiatry, but I thought I might be more interested in becoming a psychologist, and I also was interested in philosophy, and I had originally intended to go to law school and still found the law rather interesting but that I didn't want to practice law, and I didn't want to practice medicine either, except a little bit. Then there was also the possibility of being a statistician except that I didn't think I was quite good enough in mathematics. I told him that I was absolutely sure that being a college professor was the only life for me, that I was cut out for it and I doubted that I was cut out to do anything else, and that I knew that I would get good enough grades so that if <u>anybody</u> got asked to stay around and be on the faculty, it would be me. He had considerable trouble assimilating this although he could see I was not very tractable about it, and since it was rather pointless to put down that list of alternative majors he let it go at that. I had the impression that this was the first time in his counseling experience that a fifteen year old ever told him that he was going to be a college professor and that he was confident that he would "make it." I remember his asking me whether someone in my family was and I had told him no, I had never met a college professor (I met the first college professor I laid eyes on the following year, the father of a new friend) but that I knew that college professors got paid steady despite the business cycle, that they didn't have to deal with the public, and that they were paid to read books and have intellectual bull sessions and teach courses and do research, which is what I knew I wanted to do in one field or another. This is an example of self vocational diagnosis which was I am convinced was 100% correct. I have many times had regrets about the field I went into, because psychology is a frustrating enterprise, and clinical psychology more frustrating than most. I have often wished that I had become a geneticist or a statistician or had continued doing work in animal psychology. But I have never once, even in the most boring faculty meeting or dealing with the most recalcitrant educational task, regretted even for five minutes becoming a college professor. It is not surprising to me to read the statistics on job satisfaction which shows college professors as I recall to be up there with lawyers, physicians and clergyman. (I daresay the latter group has declined in recent years.) Nor have I been surprised that with the exception of clergymen, college professors have the longest life expectancy of any major occupational group. The combination of having interesting things to think about and interesting people to talk to (you are allowed at a big state university to be very selective!) with economic security and what is in fact even in American society a sizeable amount of prestige (despite the jokes about absent minded professors who have never met a payroll, the leisurely pace, and not having to punch the old time clock, simply can't be beat. I suppose I could have survived and maintained my mental health doing something else, but I must confess I find it hard to imagine.

There were a couple of special features in the ideology of this little group that I think shaped me greatly, although arguably I shaped the group in this respect for psychodynamic reasons. People with a strong dash of scientific criticality, especially if they are in the rebellious teenage mood, can at times be narrow, pedantic and dogmatic. There was one member of the group (he subsequently became a fairly well known physicist) who had a tendency to "super scientism," in the sense that he was not an effective participant in discussions of politics, religion, metaphysics or ethics, all of which interested us greatly, because one couldn't write equations and do experiments on these matters. For some reason not clear to me even now, and which did not become elucidated in my analysis, I was almost wholly free of this kind of scientistic negativism. (I am reminded here of my teacher Herbert Feigl's remark that "a positivist professor can usually be spotted by having negativist students.") It may have been partly my father's wide ranging intellectual curiosity, or perhaps my mother's high <u>n Nurturance</u> which lead her to emphasize tolerating a diversity of views, and a distaste for people that she called "bossy"; but in any case, along with the rules of the scientific game enjoining empiricism, criticality, skepticism and rejection of appeals to authority or emotion or other kinds of irrational arguments, the ideal of being <u>disinterested</u> and <u>fair-minded</u> was strong in me and with most of the others, I find a list of the "rules of scientific thinking" in the back of my tenth grade Latin book as follows:

(insert six rules)

SCIENTIFIC METHOD

1. <u>Disinterestedness</u>; objectivity; desire for the truth.
2. <u>Open-mindedness</u>; lack of prejudice; freedom from bias; willingness to be convinced.
3. <u>Scepticism</u>; doubt; criticism; cautiousness; suspended judgment.
4. <u>Rationality</u>; logicality; use of cold reason.
5. <u>Empiricism</u>; resort to experience; faith in the experimental facts.
6. <u>Measurement</u>; reduction of facts to number.



Patience and a mulberry leaf make a silk shawl.

SCIENTIFIC METHOD

1. Disinterestedness; objectivity; desire for the truth.
2. Open-mindedness; lack of prejudice; freedom from bias; willingness to be convinced.
3. Scepticism; doubt; criticism; cautiousness; suspended judgment.
4. Rationality; logicality; use of cold reason.
5. Empiricism; resort to experience; faith in the experimental facts.
6. Measurement; reduction of facts to number.

[Inside back cover of Meehl's copy of B.L. Ullman, N.E. Henry, & D.S. White, *Third Latin Book*. NY: Macmillan, 1931.]

Secondly, despite a tacit group belief in our superiority to school peers who did not think scientifically, we rejected "naked egotism" as an unacceptable motive (or personal style). For instance, most of us were A and B students, but the one college professor's son in the group was not much impressed with getting A's—for him the academy was nothing special, having been raised in such a family—so he only studied the things that were interesting to him. He did not study things that bored him with the result that he got, for instance, a D in English because he didn't bother reading the Shakespeare comedies, which he thought were not funny. So far as I could detect this dismal grade-getting performance, which also was true of him in some other courses, was hardly a source of comment among those of us who were "grade getters." Even if we mentioned it (rarely), I never saw any signs that it lowered our regard for him. Getting a D in Shakespeare was not important in a fellow who could beat you at chess playing blind when you could see the board, which he used to do playing chess with me. The most that our somewhat pedantic physicist-to-be ever said about this was that it was rather short-sighted of a person, because, after all, sooner or later our friend would recognize that the academic life was a natural life for him despite his rejection of it, and it was improvident to get a D, although the Shakespeare comedies are of course pretty boring. I think the ideal, which perhaps some of the group deceived themselves into thinking they had fully attained, was that one should be dedicated to scientific truth and passionate about the life of the mind but not be concerned with the more obvious kinds of ego pellets that society might deliver. It is interesting, thinking about a group like this who were bright, introspective and I think it fair to say somewhat neurotic (for example, most of us were a good deal shyer with girls than our less intellectual peers), that I never heard anyone talk about having trouble with his "identity." Nor did I ever hear anybody complain that it was hard to get himself to study because, after all, what was the meaning of it all? The world was going to pot anyway, so the hell with it. Some young people, friends of my own children or undergraduates, that I counseled in the Viet Nam period, rationalized their failure to study for the final exam or to take the Ph.D. prelims or whatever on the grounds that when you saw such horrible things as racism or the threat of a nuclear war or whatever was their main cause of complaint against society (complaints I shared) [nothing else was important?] never has rung true to me. Students of my generation were confronted with three gigantic and frightening spectacles in their teens and young adulthood: the Great Depression, fascism (including the early manifestations of what became the holocaust) and the corruption of the socialist ideal by Stalin's bloody tyranny. I find it hard to believe that the background knowledge that mankind might exterminate itself by nuclear folly, or that there is tension between races, or that many people of the world starve, really has a psychic impact on a twenty year old more profound than living through these three things could be expected to have had upon our generation.

Since I cannot dispute the existence of "problem of identity" among some of the young, as a psychologist I am at a loss to explain this generational difference. In any case, if somebody had asked me when I was sixteen years old <u>why</u> I kept on top of my homework in Latin and Geometry and Physics so that I could get A's, I would have replied that it was sufficiently interesting so that it wasn't difficult to apply one's mind to it; that some of the other things, such as most of my English classes, were boring but not very difficult (certainly Shakespeare was easier than Physics) and since I intended to make my living leading the life of the mind rather than being a car salesman or stockbroker, it was obvious that a sensible person would try to get good marks in school so that I could become a college professor, as I did not expect to inherit a million dollars from an unknown relative in Norway! If each of these teenage boys were asked "What's the <u>point</u> of studying?" they all would have given this answer (except for the professor's son). We would have admitted, perhaps a little grudgingly, that we admired intellect in others and would enjoy having the prestigious title "professor," although the larger society might not regard the professoriate as highly as we did. But to have ego gratifications, such as prizes, titles and the like as one's <u>main motive</u>, or even as a major contributory motive, was looked upon with disfavor. Although we were perhaps somewhat deceived about how much contribution of narcissism was involved in achievement, I think there was, on the whole, a pretty strong effort to minimize it. Those few members of the group whose scholarship, reading and knowledge were to a considerable extent aimed at being able to "show off" conversationally, were to the degree they did that, somewhat disapproved of.

The emphasis on rational argument and the ability to defend an opponent's point of view effectively, meant that certain kinds of intellectually upmanship or skill at verbal fencing, just being good at "winning the argument," were strongly disapproved of. This attitude has persisted into my adult life and old age. When I meet an academic who is an intellectual showoff—especially one for whom a scholarly controversy clearly has the character of a pissing contest—I lose interest in talking with such a person, and if I am honest with myself I would have to say that I despise him. The teenage idealism about the intellect still persists in me. I look upon the intellect as kind of a sacred thing, as most people view, say, conscience, and to have a conversation with the aim not of getting at the truth or clarifying one's ideas or exploring new possibilities but rather putting the other fellow down, showing that you are more resourceful and agile at debate, seems to me a corruption of the intellect and—speaking less melodramatically—a silly way to spend one's time.

To anticipate, not a single member of the Minnesota psychology faculty engaged in this kind of vulgar gamesmanship. After I took my Ph.D. and began to move around the country and deal with professors from different academic subcultures, to find that there were quite a few who viewed an intellectual conversation as some kind of chess game baffled and disappointed me. After all these years, I am still mildly surprised when I come across a flagrant case of it. Since I myself am intellectually resourceful and verbally fluent, and people who know me tend to view me as "intellectually dominant" (by which, I think, they do not mean I am domineering), I am reasonably sure that I don't deceive myself in this regard, that my distaste for this kind of pseudo intellectual discourse does not stem from the fact that I am a loser at it, but truly stems from my belief that it's pointless and a kind of spiritual corruption.

Psychology Department 1938 (1st edit)

The Psychology Department at Minnesota, when I entered the university in the Spring of 1938, was widely known and highly regarded, having been rated among the top half-dozen departments in the country at the time of the first national ratings of excellence in the middle 1920's. In all subsequent ratings it has subsequently been evaluated highly, but not always for the same reasons, as its composition and the Zeitgeist underwent changes. Considering its institutional eminence the department was, even for those days, remarkably small in number of faculty. The student enrollment in psychology courses was among the highest in the College of Science, Literature and the Arts (as it then was [called]), I believe exceeded only by English and Economics in student hours and number of undergraduate majors. Amazingly, the department faculty holding the rank of assistant professor or above was only seven! The chairman, Richard M. Elliott (known generally as "Mike" but never addressed by junior faculty or teaching assistantships, let alone students, as other than "Mr. Elliott") not being a clinician, and being a Harvard Ph.D., would have disdained being called "doctor," which for him meant dentists, physicians or veterinarians. Elliott was a Harvard Ph.D., a contemporary of Tolman there, and came to Minnesota as a last minute surrogate for Yerkes who had accepted an offer to chair the department, recently split off from philosophy, but who decided instead to head the new primate laboratory in Florida. Yerkes recommended Elliott (a young man who at that time had no visibility, having not published anything), and the university administration wisely decided to bring him here as chair. Mike Elliott was a New Englander of middle class origins whose Yankee parsimony later lead to complaints by some that he was niggardly in matters of salary and supplies. The usual comment was that it was amazing how he managed to build and maintain a first-class department operating on a shoe string. (One of the first things I did in becoming chairman in 1951 was to demand, and get without trouble, a sizeable increase in the departmental supply budget.) Elliott was a man of wide learning and general culture, and nobody who took his courses or had office conversations with him on matters psychological or philosophical failed to be intellectually enriched by the experience. I think of him as one of the best examples known to me personally of the fact that a college professor can be bright, clear headed, profound, incisive and integrative, and hence a stimulating teacher and colleague, without being a "knowledge-producer" in the sense of publishing research papers. While Elliott was listed as first author in the famous Minnesota mechanical ability studies in 1930, and while he did play an important role as the coordinator and fund raiser for that enterprise, he had very little to do with the design and nothing to do with the actual carrying out of the research. To the best of my knowledge he never published a single experimental or theoretical paper, including his doctoral dissertation (on graphology!) at Harvard. He gave a course called Human Behavior, which should perhaps better have been labeled "Philosophical and Biological Foundations of Psychology," which was required of all doctoral candidates. All of us, including those who disagreed with some of his theoretical positions, remember this among the half dozen most stimulating courses they had at either undergraduate or graduate college level. He also gave a course in "biographical psychology" which we might today call personality theory, in which he did his best to make a case for Allport and his idiographic emphasis, very much against the ethos of the department, especially the strong nomothetic views of Skinner and Paterson. He also taught one section of the beginning introductory psychology course, and he believed that course should always be taught by the major professors. Elliott would today be incapable of receiving tenure at a "research oriented" state university, due to his lack of publication yardage. I dislike the current semantic corruption of the word "scholarship" as used by college administrators and faculty, where it means publication yardage. It is impossible today for anybody, no matter how bright, learned and stimulating to colleagues and students, to get raises and tenure rank without producing publication yardage, [however] pedestrian and ephemeral its character. I feel strongly about this, although my publication record is pretty good, because I was exposed as an undergraduate student and a graduate student to several first class intellects who were true scholars in the old sense but not new knowledge producers.

While Elliott was serving in the U.S. Army sanitary corp, which was where the psychologists were located in World War I (at Camp Greenleaf) he made the acquaintance of Donald C. Paterson, who had been a student of Rudolph Pintner and was co-author of the famous Pintner-Paterson Performance Test of Intelligence. Somehow, Elliott had become convinced that applied psychology, which hardly existed as such in the early 1920's, had a great future provided it was scientifically based. When the first industrial psychology firm, the Scott Company, folded in the recession of 1921, Paterson was looking for a job and Elliott brought him to Minnesota with the understanding that he would develop applied psychology here and teach a course in individual differences, despite the fact that Paterson did not possess a Ph.D. degree. Donald G. Paterson is a major figure in the history of applied psychology and can be considered as almost among the founders—some would say <u>the</u> founder—of the movement that was then called "student personnel work" and which subsequently became counseling psychology. Paterson was my undergraduate advisor and had a marked influence upon me, especially in strengthening my psychometric interests and somewhat counteracting my yen to speculation and theory, for which he had relatively little regard. The derogatory phrase "dustbowl empiricism" characterized places like Minnesota and Ohio [did typist mis-hear Iowa?] State with their relatively atheoretical emphasis upon applied psychometrics and mental measurement and the like (not, as sometimes said, invented by Elliott or Paterson, but by graduate student John Harding who subsequently left Minnesota and took his doctorate at Harvard). When I think of that, I think of Thorndike's dictum that if a thing exists then it exists in some amount, and if it exists in an amount, that can be measured. Paterson was at that time clearly the dominant figure in the department and the largest proportion of undergraduate and graduate majors were in the fields of student personnel work, differential psychology, and what today would be called industrial psychology.

The abnormal psychology course was one of the most popular courses on the campus, taught by an Englishman named Charles Bird who, despite a slightly pompous style and histrionic delivery, gave one of the most informative courses in descriptive psychopathology that you could find in those days. It was vastly superior in organization and clinical illustrations to the descriptive psychiatry course we clinical students took in the Medical School as part of a "Neuropsychiatry" minor, and its superiority was recognized to such an extent that some of the psychiatry residents used to come over and listen to Bird because his stuff was better than what they were getting in the residency training program. While Bird did a little informal "psychological counseling" with troubled students who for one reason or another did not want to make use of the Student Health Service, he never charged a fee. Except for the class visits to the State Hospital and attendance at a few grand rounds in the psychiatry department, it is fair to say that Bird hardly saw a real mental patient for many years.

Howard P. Longstaff taught the personnel course. The experimental course and the undergraduate laboratory courses were taught by Miles A. Tinker. It is probably fair to say that the weakest element of the department was human experimental psychology, because Tinker had, under the influence of Paterson, shifted his interests to applied experimental psychology and his own research was on subjects like eye movements in reading and illumination levels required for various kinds of tasks. B. F. Skinner had come here from Harvard (where he was a fellow and did no teaching) and barely made it with a split vote where Elliott as chairman broke the tie. As Elliott described it to me, he went to the then dean of the college and said that his colleagues were split down the middle but that the ones who voted no were simply mistaken because he, Elliott, was convinced that "this man Skinner is a comer," to which Dean Tate replied "Well, Mike, your judgment is good enough for me." I suppose in these days of frenzied egalitarianism such a wise appointment would be impossible. Kenneth E. Clark, a recent PhD from Ohio State, taught psychological statistics and opinion polling. When I was an undergraduate there were several people with the rank of instructor including William Carlson, Robert E. Harris, Stuart Cook and Guy Hamilton Crook, the last three all having a part-time connection with the Medical School. The other major figure, Starke R. Hathaway, had only a small cross charge in the psychology department where he taught a course in physiological psychology. By 1938 he was involved with the head of psychiatry, J. C. McKinley, M.D., in construction of the Minnesota Multiphasic Personality Inventory.

William T. Heron taught learning, a section of general psychology, animal psychology and a strangely titled course "genetic" psychology which did not deal very much with genes but with comparative and ethological topics such as the social life of the bee. He lectured [in a] low key [manner with] a rural Kansas accent which some students found boring, and my friend MacCorquodale and I learned to take students' reactions to Heron's courses as kind of a litmus test of whether they were really smart or not. First-class intellects realized that they were getting a lot from Bill Heron and were not put off by the rustic style, but the second raters didn't see it.

I don't suppose any highly rated department in the country had a greater applied psychology emphasis than Minnesota's, although possibly Purdue and Ohio State were almost as much so. But Paterson insisted that all of his doctoral candidates take human behavior, biographical psychology, animal psychology, experimental psychology, and history of psychology. On the other hand, all doctoral candidates in experimental or animal psychology, including Skinner's advisees, took Paterson's Individual Differences class. It was amusing to those of us with Minnesota doctorates how Ph.D's in Experimental from other schools who joined our faculty knew almost nothing about the field of Differential Psychology. I remember one young social psychologist, who subsequently achieved high distinction in that field, commenting to me that it wasn't until he got to Minnesota that he thought of Individual Differences as anything other than the error term in the denominator of a significance test! There may have been a few Minnesota Ph.D.'s that did not take Bird's Abnormal course, but very few. The result of these customs was that Minnesota Ph.D.'s were broadly educated, and I'm inclined to think that that is still true today although, alas, less so than it was in the 1930-1950 period. As I have said, the weakest part of the doctoral program was the experimental course, which was a bit dated and taught by somebody who was no longer strongly interested in the theoretical aspects of Experimental Human Psychology.

Despite the dominance of Paterson and the applied tradition, most graduate students, while they entertained high regard for the various professors, tended to see two faculty members as sort of the super brains, Skinner and Hathaway. It is paradoxical that Hathaway and Skinner were good friends and saw a good deal of each other, considering the great difference in their interests, values, and conceptions of psychological science. To be accepted as a Ph.D. candidate by either of them was a feather in one's cap. Prior to the explosion of Clinical Psychology and the beginning of the VA training program after World War II, there were only two paid assistantships under Hathaway's control and he typically had only these two doctoral candidates. Skinner advised very few students during his period in Minnesota, but among them were such first class people as John B. Carroll, William K. Estes, and Norman Guttman. My friend and colleague Kenneth MacCorquodale, who later taught our basic "Skinner" course, was not a Skinner Ph.D., having taken his degree under Heron.

Bertrand Russell, in critically examining William James's "Will To Believe," remarked that what is needed in this world is not the will to believe but the wish to find out, and he contrasted wishful thinking with scientific thinking by saying that the scientist's ruling passion is the <u>passion not to be fooled, and not to fool anybody else</u>. Despite the diversity of research techniques and interests, and the division between people with theoretical interests and almost wholly applied ones, it could be said of the Minnesota faculty at that time that they were all strongly animated by that passion, not to be fooled and not to fool anybody else. And this overall spirit of extreme methodological criticality informed their research direction. Perhaps the most striking example of this was Paterson's two-quarter, six-credit class in individual differences, the only class in which his non-applied interest showed up in that he was fascinated by the problem of the inheritance of intelligence and, secondly, debunking the alleged physical correlates of psychological traits. He relied on the textbook for exposition of things like correlational methods and the various kinds of mental tasks, and concentrated his classroom lecturing upon the analysis of published research on physique and intellect and upon the "heredity-environment" controversy regarding IQ. He assigned Anastasi's first edition as a textbook, explaining to the class that she was somewhat biased on the environmentalist side and he was somewhat biased on the heredity. We should read her book and think about her arguments, and he would feel free to grind his hereditarian axe in the classroom, which he did so vigorously and effectively that Minnesota Ph.D.'s in the 1930's emerged with a strong predilection for genes at a time almost all social science was anti-hereditarian. In examining research studies, Paterson picked them apart methodologically piece-by-piece and bone-by-bone. By the time a student had sat through 60 hours of this, he was so hyper-conscious of defects of design, inappropriate measures, faulty causal inferences, etc.—a wide range of methodological mistakes that social scientists were still making in those days (and some are making even today), ranging from spurious index correlation to neglecting the possibility of selective migration, or a misleading faith in partial correlation and the analysis of covariance providing a kind of strict "control" of nuisance variables (which it does in agronomy but does not in correlational psychology)—that nobody who managed to get a Ph.D. at the University of Minnesota department would ever, as long as he remained sane, publish a paper containing any of these mistakes. I want to emphasize that is more than simply the standard textbook statement that science should receive [missing material]

I began at the University of Minnesota in March 1938, taking premed courses so medical school would be an option; in any case I wanted to learn some physical and biological science. I persuaded R. M. Elliott, Chairman of the Psychology Department, to break the "sophomores only" rule for the general psychology course by telling him of my high school reading. I enjoyed almost every class I took and regularly managed to top the class. After a year of premed I decided I did not want to be a physician and became a psychology major, partly because I learned that academic clinical psychologists could do some therapy, and I knew I didn't want to be a full-time practitioner, of *anything*! Also, most of my premed friends, and their older brothers who were medical students, interns, or residents, did not stimulate me intellectually as much as did majors in psychology, philosophy, mathematics, and political science. I took calculus and mathematical theory of statistics (rare for psychologists in those days), partly because my physicist friends talked about partial derivatives and I wanted to know what that stuff was all about.

I received the B.A. *summa cum laude* (my advisor, D. G. Paterson, insisted on it) with a minor in biometry (another Paterson requirement). I thoroughly enjoyed my undergraduate years, including the "anonymity" which students complain about. It never occurred to me that the professors at a big university were supposed to "love me as a person" or that an institution of higher learning should "give me an identity." I would have classified such talk as immature and irrational, and I still do. I picked my friends for brains, intellectual passion, and the aspiration to think straight. My undergraduate experience solidly confirmed my teenage view that the life of the mind was fun.

Undergraduate Days 1938-1941 (1st edit)

Graduating from high school in January of 1938 I was eager to begin college. I entered the University (College of Science, Literature and the Arts) in March of 1938 where I listed my educational aim as "premed," although I was undecided as to whether I ultimately wanted to go to medical school. My view was I should have that as an option, and that the pre-medical courses in basic sciences would be interesting and relevant if I should become a philosopher or psychologist instead of a physician. It was my intention to get all A's (preferably the top A in the class) with a mental hygiene resolution not to be distressed if I didn't manage it. As a matter of fact, I was consistently at the top of every class I took during my freshman and sophomore years, even in courses like German which did not interest me much. I had been warned by my Aunt Signe that my grades might undergo some decline coming to the University where I would be competing with people who were as bright as I was, and so I took for granted that one should study hard and systematically, being pleasantly surprised to discover after my first quarter that studying hard and systematically resulted not just in getting an A but the top A. Because of my intense interest in the mind, I wanted to start psychology immediately and was disappointed to learn that it was open only to sophomores or to third-quarter freshmen with a high B average. Blissfully unaware of how firm this policy was, I went to see the Chairman of the Psychology Department before registering, Richard M. Elliott, and explained to him that I was a premed intending to major in psychology and then become a psychiatrist and I would like a special dispensation to take the course as a beginning freshman. He said that that was not allowable under any circumstances and was curious as to what made me think I could swing it. I said that I had read a great deal of psychology including all of the basic writings of Freud, plus Watson, plus some educational statistics and the like. He gave me a little informal oral examination on my reading, and despite his highly controlled New England character it was evident that he was surprised at how much I knew and how much I had reflected on. He called up Professor Charles Bird who taught the big doubled-up beginning psychology section and said he was letting in a first-quarter freshman because of an unusual amount of previous reading, so Bird went along with it. I remember sitting in this class of roughly 150 people thinking to myself, when the professor entered the room, "He doesn't know my name, there are 150 of us here and I am one faceless, anonymous nothing to him—but he <u>will</u> know my name before the quarter is over!" After the midquarter, Professor Bird came in and said that there was a first-quarter freshman who wasn't supposed to be in the course who had missed only one item on the midquarter, and although he wrote the test he couldn't do that well himself. I thought to myself "Good, when it comes time for me to apply for a teaching assistantship if I don't go to medical school, [missing material here]

[What follows is about the group Paul ate lunch with in the cafeteria.] There were usually a dozen or so of these Jewish students, some with their girlfriends and only one or two Gentiles, which led to some amusing episodes. For example, all but one of the Jews were irreligious (although some had immigrant orthodox parents) and that one was planning to become a rabbi, about which he changed his mind later, Melford Spiro, who went on to become an eminent anthropologist specializing in comparative religion. He was arguing with one of the hard-nosed atheist premeds (Mort Hartman) about religion and was coming off, despite his brains and verbal fluency, second-best in the debate. At one point his antagonist, in the heat of argument said, "Spiro, you know what's the matter with you? The trouble with you, Spiro, is that you think like a goy!" In a couple of seconds Hartman blushed profusely as he glanced down the table and remembered that two Gentiles, George Collier and myself, were at the table. Another interesting thing that came to me by this group was an exposure to Communism which I might not have had otherwise, since I was not interested in being active in any campus political groups. At that time, Minneapolis along with New York City and I believe Newark, New Jersey, was one of the "hotbeds" of Trotskyist communism. A couple of my friends met through this group were active in the movement and in fact one of them was one of Trotsky's guards in Mexico. There were also some ambivalent Stalinists (although I don't believe they were card carrying members of the party) and in those days Stalinists and Trotskyists hated each other more than either hated Republicans. I was a Norman Thomas Socialist who already recognized the non-Socialist character of Stalin's regime. My interest in epistemology which previously had been focused on philosophy of science, and to some extent on ethics and theology, now found a new application which was a fascinating philosophical as well as clinical study. Here were people with high IQ's, getting A's in physics and chemistry, who continued to deny that Stalin was a bloody tyrant and who defended the Moscow trials as an unfortunate necessary protection against counter-revolution. Then on the other side were these Trotskyists who struck me as pathetic figures, a miniscule group of perhaps 2,000 people over the entire United States, vigorously defending the Soviet Union as a "workers' state," with a "bureaucratic "excrescence" on it, but still the only workers' state and therefore one whose interests in international affairs had to be defended at all cost; and who continued to do this after Trotsky was murdered by a Stalinist assassin in 1940. How, I asked, do we reconstruct the epistemology of political positions?

Another way in which I benefited from my contact with this group was that the physicists, chemists and mathematicians among them all studied mathematics. At that time I did not foresee that much later in my career I would have occasion to use a little undergraduate math in my own taxometric research. But I found it unbearable to be sitting across the lunch table from someone who was talking about partial derivatives and I didn't know what he was talking about. One of my physicist friends said to me dogmatically, "I don't care what they say in the English department about Shakespeare—I maintain that nobody is an educated man in this society who does not understand at least the elementary ideas of the calculus." As a result of this precept and example I did take more undergraduate mathematics than was customary among psychologists at that time (or, for that matter, even at the present time), for which I am very grateful.

My undergraduate democratic Socialism, a view that was extremely common, among the more "intellectual" of my classmates even in the middle west, was more motivated by notions of efficiency—we all felt that the capitalist system had broken down world wide in the Great Depression—than it was by compassion or egalitarianism. I think that a person's core views on social life could be pretty well captured by drawing a triangle with the French Revolution's three words as the apices and asking the subject simply to put a dot somewhere in the triangle representing how close he was in his emphasis on these three values. I have changed in many respects over the half century since I was a teenager, including no longer being a Socialist, but I think my position in that triangle has remained unchanged despite having altered my ideas about economics, ethics and politics. My great passion is liberty; equality (except as narrowly defined, such as legal procedural rights) is a poor second in my value system; fraternity is very low and something in which I feel the state has no business concerning itself. My Socialist orientation, while not passionate, was fairly definite. I did not consider the tyrannous corruption of the Socialist ideal and the malfunctioning of the Soviet economy as strong evidence against Socialism as a concept. I thought that Marx would have said that Russia is the wrong place to make a revolution anyway, and one had the usual explanations of how it turned into a tyranny. I took democracy, including both majority rule and minority procedural protections, plus some substantive minority rights, as fundamental to genuine Socialism. Hence I did not think there was any empirical example on earth by which we could fairly judge whether Socialism as an economic system could work well or not. As I now think, the empirical evidence on a variety of Socialisms of different colors is adequate to say that it is not a good economic system.

My orientation in these matters was somewhat fostered by my having joined a sort of “anti-fraternity fraternity” called the Jacobin Club which had no house, was the only group that admitted Jewish members, and was decidedly left of center, although we did have two “liberal Republicans” as members. The main thing, other than being intellectually interesting to talk with, that you had to have to get into that outfit was brains and a manifestation of them in having an extremely high academic average. Every spring the student paper would carry an article on grade point averages of fraternities and the Jacobins always came out way ahead of everybody else, being almost straight A students. We met weekly, in the student union for meals; we sometimes went out for beer and had discussions on a variety of subjects.

As a result of my undergraduate experiences, I have had a difficult time—despite being a psychotherapist and I think basically a kindly person—developing sympathy for some contemporary undergraduate complaints about the University being a big impersonal place where you are totally anonymous, nobody loves you as an individual, and the like. It never occurred to me when I came to the University that I would be loved by the faculty as an individual, and I found anonymity delightful. I didn’t have to attend any school functions that didn’t interest me. The academic course requirements seemed reasonable and minimal. I could pick friends from a wide array and I found the whole experience 99% pleasurable. The idea of having an “identity crisis” was foreign to me, as it was for all the people in my group. We were all bright, highly motivated, well organized, enjoyed most of our courses, had certain aims for our future professional careers, and were confident that we would make it.

Living cheaply with my grandparents, an easy streetcar ride from the campus (in good weather I sometimes preferred to walk), it was possible for me to attend summer school as I did not have to work my way through college. The combination of going to summer school plus what were then called “quality credits” enabled me to obtain the BA degree in June 1941. I had remained a premed for one year plus a summer session and at that time had a straight A average and I believe was looked upon by my premed peers as about the brightest in the bunch. So they were troubled—some of them seemed almost horrified—when I said that I had decided not to go to medical school but to go right ahead and get a Ph.D. in Psychology. Some of these premedical students had almost the axiom that if you had the brains, energy and money to “get your MD” (an interesting way of putting it) it was treason to your own talents or the universe or something if you didn’t do so. I anticipated no difficulties either academically or financially, I was not engaged to be married or seriously contemplating such, so there was no reason why I couldn’t have gone ahead, as I know I would have been admitted to medical school. For that reason I have never had the problem that some clinical psychologists have in relating easily with physicians. I am not an MD for the simple reason that I decided that I did not want to be one. I was getting so interested in learning theory from the psychology courses, and in philosophy of science, that I couldn’t see myself spending that much time learning the bumps or the bones and the biochemistry of the blood in order to be a college professor in the domain of the mind. But there was another reason which people didn't know about because it might have sounded insulting, and that is that through my premedical friends I had a certain amount of contact with older relatives and older brothers or friends of theirs who were interns or residents. By and large I did not find these people very much fun to talk with compared with those older brothers who were philosophers, mathematicians, political scientists and the like. This may seem like a strange reason for choosing a profession, but I think my instincts were sound. The only times in my professional career that I have briefly regretted not having the MD degree is when the psychotropic drugs began to appear and demonstrate effectiveness for things like endogenous depression and schizophrenia, where the inability to write that magic on a prescription pad was temporarily frustrating. However, like most clinical psychologists I have found it fairly easy to set up a working relationship with a psychiatrist willing to prescribe appropriate medications after interviewing a patient that I was seeing in psychotherapy, so that frustration was of relatively short duration in my career.

My undergraduate advisor, when I formally declared a psychology major, was Donald G. Paterson, one of the great applied psychologists of the first generation and co-founder of the whole "student personnel" movement, as it was then called. He urged his undergraduate advisees to minor in Biometry, so that by the time we entered graduate work we had a full 15 credits of statistics in our heads. I took an excellent course taught by biometrician Allen E. Treloar (located administratively in the Department of Public Health). I wanted to have a little more theoretical comprehension of certain matters and therefore in my senior year I took a course in the Department of Mathematics, taught by one Dunham Jackson, called "Mathematical Theory of Statistics." I had petitioned to get credit for this in graduate school because I had an excess of credits at that time, and so I felt very much under the gun to do well at it. I was made somewhat anxious when I realized that I was one of only two psychologists in the class, most of the students being math majors with a few statisticians and epidemiologists. Integral calculus was a prerequisite but since a few of the students hadn't had it Jackson made a practice of proving everything three ways. First with calculus, then taking a lot more time (and board space) with algebra, and thirdly—a special interest of his in which he had published a couple of papers—by theorems in spherical trigonometry. Since my only B in high school was algebra and one of my four B's in college was analytical geometry—I have a grave defect in spatial perception and spatial thinking—I was determined to get an A in this course but I did not entertain any hopes of getting a top A. I also knew that if we were asked to prove something in the final exam, I wasn't good enough in mathematics to think through how to go about formulating a proof, and consequently I not only required myself to understand all the proofs but I memorized them. Memorizing the proof of Stirling's approximation to the factorial when you're not a math major is a lot of work! The result of this compulsiveness, and the fact that I found it one of the most intellectually exciting courses I had ever taken, was that I did get the top grade in the class. I didn't discover this until two or three years later when I had almost completed my doctorate. I received a telephone call from a field representative of Goodyear Rubber who asked me if I wanted to take a job with their company as a quality control statistician because that would almost certainly keep me out of the draft. I told him I was already out of the draft on the grounds of rheumatic heart disease and, besides that, he must have the wrong name, since I was not a mathematician or a statistician but a psychology major. He said he was aware of that fact but that his old friend Professor Dunham Jackson, whom he had used as a resource person for years when he was looking for a statistician, followed his usual practice of giving him the top scoring person in the final exam in that course and it was mine. This suggests to me that I have a goodly amount of n Counteraction. It was important for me to show that I could do well competing with a room full of mathematics majors, that I wasn't a mathematical dunderhead as might be suggested by that high school B in algebra!

One of the few times in my life that I have put something off to the last was writing a thesis to receive the BA degree summa cum laude. I really didn't want to do this and I didn't think it was very important to my getting a T.A. job, because by that time all of the faculty in the Psychology Department had a high regard for me. But Paterson kept leaning on me to do it, so finally at the last minute I agreed to try, and in those days before word processors typing, revising, and re-typing was quite a task, I remember the night before the deadline day for submitting it to the Dean's office my wife and Rose Boucher, the then principal secretary in the Psychology department, frantically batting out the pages, which I would then read and correct. One revision had to do, because we wouldn't have met the deadline if we had multiple drafts to work through.

The summer after graduation I loafed, except for two leisurely reading projects, Pavlov's *Conditioned Reflexes* and Hilgard and Marquis's *Conditioning and Learning.* In September 1941 I began my graduate work as a T.A. in the Psychology Department, which I found fairly enjoyable. An attack of rheumatic fever in 1942 left me with a mild mitral regurgitation, not troublesome but sufficient to keep me out of World War II. While I felt the world shouldn't put up with Hitler and Tojo, they weren't worse than Stalin, and I was unabashedly pleased to be classified 4-F, unfit for military service. As the war went on, this became a term of opprobrium, but it never bothered me in the least. "Sticks and stones . . ." was a hard childhood lesson, and I believe I have practiced the precept with 95 percent success.

After receiving my BA degree in June 1941 (major Psychology, minor Biometry) I decided not to attend summer school, a thing which I had done every summer since my freshman days, and set myself two "scholarly projects," which I knew would be fun but which would leave me plenty of time for loafing before I started graduate work. I read two books and nothing else in psychology, Pavlov's Conditioned Reflexes and Hilgard Marquis Conditioning of Learning. In 1941 I married, which was financially possible on the TA stipend because my wife had a full time job in the private sector and because we decided to live for the first year of our marriage with my grandparents, which was close to the campus and had other advantages such as my grandmother's great pleasure in cooking excellent meals for people. The board and room rent and use of the house were at only the most nominal charge, and my wife got along well with both of my grandparents. In the fall of 1941 I began work as a teaching assistant in the Psychology Department which in those days consisted of assisting in the undergraduate laboratory course in the lab, and grading lab reports. I was dispensed from most of this because Professor Bird had started to use a new textbook in his Abnormal Psychology course, the excellent book by Maslow and Mittelman Principles of Abnormal Psychology, and he wanted someone to write multiple choice test questions for his midquarters and finals as well as update the material taken from lecture. I thoroughly enjoyed both my coursework and my TA duties, and unlike some who experience graduate school as frustrating, onerous and fraught with anxiety, I had no such negative responses with the single exception mentioned below. I did run into some trouble by coming down with a strep throat and a strep pneumonia in my first year, following which I developed the not uncommon sequelae of rheumatic fever and a resulting mitral regurgitation. This was mild and not troublesome, but it served to keep me out of the army in World War II. While my long-time pacifism did not extend to thinking the world should put up with Hitler, I was not at all eager to get into the fray, and was unabashedly pleased to be classified physically unfit for military service. As the war went on, "a 4F-er" became a term of mild opprobrium, and it tells something about me that this never bothered me in the least. "Sticks and stones…" was a lesson I had learned as a child and I believe I have practiced the precept with 95% success.

In 1938 Minnesota's Psychology Department had a small faculty despite its many majors and national prestige, consisting of chairman R. M. Elliott (theoretical, biographical), D. G. Paterson (individual differences, vocational), C. Bird (abnormal, social), W. T. Heron (learning, comparative), K. E. Clark (psychometrics, attitude measurement, polling), M. A Tinker (experimental, history), K. H. Baker (laboratory, advanced general), H. P. Longstaff (personnel, advertising), and B. F. Skinner (language, theoretical). The introductory psychology course was taught by full professors. Starke Hathaway was in the Medical School but taught a course in physiological psychology. Paterson, a founder of the "student personnel" movement, was the dominant figure, giving the department a uniquely applied emphasis. The local quasi-geniuses were reputedly Hathaway and Skinner, and to be accepted as a doctoral candidate by either of them was a plum. The scholarly ethos was objective, skeptical, quantitative, and behavioristic. Hathaway and Paterson disliked theory, and the human experimental side was weak because Tinker's research was mostly "applied" (reading eye movements, illumination levels). Gestalt psychology was ignored, and Freud's theories mentioned grudgingly and skeptically. All Ph.D. candidates took certain core courses, so that a future industrial psychologist had Heron's animal course, and a Skinner advisee heard differential psychology from Paterson. We were all more broadly educated than is true of many psychology students.

In addition to formal classroom and laboratory experiences I spent a great deal of time in conversation with faculty. When graduate students complain about having insufficient contact with faculty, I wonder whether this is entirely realistic. I had no such complaints, but I was fairly aggressive in seeking professors out for conversation on topics that interested me and that I thought would interest them. I never felt that the hundreds of hours I spent in the offices of Paterson, Heron, Skinner, or Hathaway were begrudged by these eminent and busy men. I was never docile in debate or hesitant to pursue an argument down to rock-bottom disagreements about epistemology or philosophy of science, but I was free of any chip-on-the-shoulder attitude, or the desire to show up smarter than the professor. I am quite certain that these professors enjoyed their conversations with me as much as I did with them.

The Minnesota selection system, which relied heavily upon the Miller Analogies Test along with undergraduate records from first class schools around the country, but did not steer away our ablest undergraduates from taking graduate work at Minnesota, provided a peer group of the highest intellectual caliber. Among students who were T.A.'s at about the same time I was were MacCorquodale, Frank Barron, William K. Estes, George Collier, Keller and Marian Breland, Norman Guttman, Howard F. Hunt, and William Schofield, all of whose names would become well known in their specialties. Other able students did not become as visible in the academy because they went into applied settings, among them Brent Baxter, William A. McClelland, William E. Kendall, Kenneth Millard, and Harold F. Rothe, who had successful careers in industry and government.

We talked very little about current affairs, and 95 percent of our conversations were "talking shop" over both theoretical and applied subject matter. It is tempting to fall into the old-oaken-bucket delusion in talking about one's graduate student peers, but I do not think I deceive myself in believing that for clinical psychologists a change has taken place over the half century since then. There seems today to be a bimodality. The majority, since the early 1960's until very recently, were oriented to clinical practice, having little interest in either methodology or substantive scientific questions. This was not true in the 1940's and until at least the middle 1950's, although some change was discernible by that time. Every clinical student that I knew in 1941–45 was interested both in the diagnosis and treatment of patients and, with equal passion, in theoretical problems of psychodynamics, learning, measurement, statistical prediction, and the like. Are psychometric factors real? How much of Freud is translatable into Skinnerese? Do neuroses have a genetic predisposition? Why do Rogersian reflections "work"?

Most current discussions by philosophers of the problems of testing psychoanalytic theory are pretty boring to me, not because I perceive them as incorrect (although they sometimes seem a bit clinically naive), but mainly because I heard them all 40 years ago as a graduate student. The department had recruited teaching assistants from all over the country, some coming from schools that were arch behaviorists and others who had been exposed to scholars like Gordon Allport or Harry Murray [Henry A. Murray], interested in Rorschach and TAT and Freud's theory of dreams. Now they were in a scholarly environment that was "objectivity" oriented and quantitative. Their Miller score IQ equivalents ran 150 plus and some up to 185 or so. Take the specific matter of the difficulties of evaluating evidence in a psychoanalytic session from a scientific point of view. What, if anything, is proved by the analyst's discerning that a patient's associations to a dream *seem* to "hang together" in a meaningful pattern? That one topic probably received at least 100 hours of intense scrutiny in these conversations during my three years of graduate work. It is not surprising that I come across few methodological arguments pro and con psychoanalytic inference that are new to me. I remain in doubt about what to conclude, but as to the arguments them-selves, I've heard them all before. I have written two papers on problems of inference in the psychoanalytic session (Meehl, 1970c, 1983b), a mixed epistemological and statistical question that has fascinated me since I was an undergraduate. I have not made much progress in thinking it through, except to say definitely that the evidentiary problem here is closely analogous to that in other "documentary" domains (e.g., law, history, even paleontology).

When Hathaway accepted me as a Ph.D. advisee, one of the consequences was that I was required to take my minor in the medical school, a strange minor he and J. C. McKinley had concocted for psychologists first called "Psychiatric Orientation" and later rechristened "Minor Neuro-Psychiatry." In that minor was a six-credit course taught by the world famous neuroanatomist Andrew T. Rasmussen, and I count this as the only aversive experience I had during graduate school. Psychologists were competing with medical students who had had a year of practice studying this kind of material. Psychologists were expected to get an A in the course and so far all of them had done just that, so it made one feel somewhat under the gun. I am not skillful at biological dissection, as I had already noticed when I took freshman zoology, and my severe spatial defect where three-dimensional relations were involved made the course content difficult. There were a lot of connections that didn't seem to have much sense to them, and I had the feeling that I was memorizing things that didn't cohere very well, the same sort of feeling I had when I didn't understand the (sometimes loose) balancing rules for certain chemical equilibriums in undergraduate chemistry. So I relied on my excellent verbal memory, plus a set of flashcards developed for the lab exam which one of the medical fraternities had. One of my old premed friends told me that they had kept a record of all the specimens shown in any of Rasmussen's lab practicals over many years, and that Rasmussen's assistant knew about it but nobody cared because if you memorized all of those, then you knew the content of the neuroanatomy course. I managed to get the required A grade, but there was enough anxiety associated with that course so that today, if I go into the anatomy building and get a whiff of formaline, I can still experience a little twinge of visceral anxiety.

In 1944 my good friend Howard Hunt enlisted in the Navy, and I was appointed instructor, while still working on my doctoral dissertation, to teach the introductory clinical class. I recall often skipping lunch because I was typing an outline of the lecture which I hoped would fill up the class time. As usually reported by young teachers having this experience, I never ran out of material, but I never got over the fear that I would do so. As Hathaway's T.A. I lectured to medical students on psychometrics, graded their Mental Status cue-sheets, tested patients, and helped with MMPI research. I did some T.A.T.'s on Dr. B. C. Schiele's well-heeled private patients, which was interesting and paid well but left me wondering just how much it helped the patient. Hathaway disliked formally designated therapy supervision—"too much like psycho-analysts and social workers," he said—but if you brought up a case informally, he was helpful.

The teaching assistantship under Hathaway was located at the University Hospital and was a mixture of teaching, clinical service, and sometimes helping with Hathaway's MMPI research. Hathaway's TA was responsible for giving lectures to junior medical students ("junior clerks," as we then called them) on elementary psychometrics and on the Mental Status Examination. Why this was done by the psychologists rather than the psychiatrists I have never figured out. Lectures on the mental status examination in the psychiatry lecture course which all psychologists took as part of our neuro-psychiatry minor were pretty skimpy. The psychologists were also responsible for grading the first few mental status reports by the junior clerks from a form called "Cuesheet."

At that time, I had not yet taken any formal Rorschach courses, although two instructors, Robert E. Harris and Guy Hamilton Crook, provided a little informal non-credit introduction to the test for a few of us. For some reason I found the TAT more interesting, and began to do some TAT work for B. C. Schiele, M.D., who saw some well-heeled private patients. After a few not very illuminating attempts to apply the formal scoring system in Murray's Manual, (I did not trust the prorating fraction for differences in story length on the various needs) l began to do what I believe most clinicians do with the test who use it today, namely, treat the protocol essentially as one treats the material produced during a psychoanalytic session. While my inferences were not completely blind because I did see the patient and listen to him talk, I scrupulously avoided having any other kind of knowledge, because I was curious to see how well I would do, and because Dr. Schiele preferred to have largely independent avenues to inferring things about the patient's personality. This was fun for him and me, and the wealthy patients could easily afford it, but looking back I have grave doubts as to just how much the payoff was in terms of incremental validity.

There was no formal supervision in psychotherapy in those days. What one had to do to get some time out of Hathaway was to casually introduce the subject of a patient one was working with because he dislike the formal supervision model of social work and psychoanalysis, to such an extent that although there was a course designated "supervision in psychotherapy" in the psychiatry department catalogue, he warded off psychology students who wanted to sign up for it! But once you discovered how to do it, it was possible to get some intense and useful help from him—as long as you didn't call it therapy supervision on paper. I did not have any strong orientation as to psychotherapy, although despite my long standing interest in psychoanalysis I was to some extent caught up in the enthusiasm over Rogers' book Counseling and Psychotherapy which appeared at the end of my first year of graduate school.

Hathaway and Hunt were doing quite a bit of hypnosis; though I did a little, I was never a skilled operator. I knew I had some resistance against it, which I didn't understand. During my analysis the best we could make of that inhibition was that the magical and irrational features of the process offended me so deeply that I could hardly believe my own suggestions! To say to a person that he won't be able to open his eyes or that his arm will move up involuntarily still strikes me—although I have seen it many times and have been hypnotized myself—as so preposterous that I don't manage to convey the required assurance. I was a moderately good subject for hypnosis myself, until at a social gathering Keller Breland suggested an analgesia of my hand which was not complete, and a post-hypnotic suggestion that it wouldn't hurt afterward (he had burned me with a smoldering match) also didn't take. Since then I have never been hypnotizable by anybody, including a couple of operators who had previously succeeded in hypnotizing me.

The academic anxiety produced by the neuroanatomy course was the only negative part of the required neuropsychiatry minor. The rest was fun. I particularly enjoyed going on the neurology rounds with A. B. Baker. Watching him or McKinley perform the neurological exam and zero in on the probable locus of a lesion was one of the few occasions in which I experienced some regret at not having gone to medical school. Strangely enough, the neurology rounds interested me as much as the psychiatry rounds. There was also at that time a widespread interest in psychological deficit psychometrics as contributing to the neurologist's assessing of the possibility of minimal organic brain damage, a subject on which Howard Hunt did his doctoral dissertation. We did about as much testing for psychological deficit in the 1940's as we did the assessment of general intelligence or of personality. Other components of the required 22 credits of the neuropsychiatry minor consisted of a reading course in neurophysiology and neuropathology with Rasmussen or one of the neurologists, regular attendance at the Grand Rounds on Saturday morning, some credit for psychological testing as part of one's externship, and the lecture courses in psychiatry and neurology taken with the medical students.

It amuses me to find psychologists who think that I was one of the "developers" of the Minnesota Multiphasic Personality Inventory, which I would be proud to be, since it is the most widely used psychological test as of this writing. But I was a high school junior at the time Hathaway and McKinley concocted the item pool, and I did not become Starke Hathaway's assistant until a year after the first mimeographed manual had appeared. While I have been author or co-author of some keys, my major contribution to this instrument was in expounding its theory and urging its actuarial interpretation. A colleague suggested that the accurate historical reconstruction would be "McKinley wanted it, Hathaway built it, and Meehl sold it." This last is an exaggeration of my role, since the encyclopedic scholarship of Grant Dahlstrom and colleagues at Chapel Hill in their handbooks and the work of my Minnesota colleague James Butcher with his annual MMPI workshops were at least as important as the lectures and papers I produced in the first decade or so after my doctorate, completed in 1945.

Because my early career and visibility and, I like to think, some of my worthwhile lasting contributions to the field involved the MMPI, it is appropriate here to say a few words about its origins. The scholarly antecedents go back to E. K. Strong, whose Vocational Interest Blank was built by "blind, empirical" item analysis of a heterogeneous pool of likes and dislikes for activities, occupations, kinds of people, and the like, with the selection of items for occupational keys being based upon an item's stable capacity to discriminate between men who were successful in a vocation and "men in general" (example: liking persons with big jaws earned you a point on the insurance salesman key). Starke Hathaway, who had taken his master's degree at Ohio and then came to Minnesota for the Ph.D. had, of course, taken Donald G. Paterson's famous course in individual differences, which all Minnesota students were required to take in those "good old days." Hathaway was impressed with the validity of the SVIB constructed in this way, an impression strengthened by Hathaway's own skepticism of psychological theory and Paterson's "dustbowl empiricism" lectures. When the Minnesota legislature set up a new unit called the Psychopathic Unit at the University Hospital, since Hathaway had J. C. McKinley, M.D. as co-advisor for his doctoral dissertation (which had nothing to do with psychometrics but dealt with the "motor theory of consciousness" à la Watson) he became the psychologist in that new unit, at a time when there was hardly such a discipline as "Clinical Psychology." He began to use, along with the then available tests of intelligence and psychological deficit, what personality measuring devices were then available. Hathaway's first paper on personality showed how the neuroticism scale of the Bernreuter Inventory could identify psychopaths by their supernormal ("non-neurotic") scores. The file research was suggested to him by a psychopath who, taking the Bernreuter, said, "It says 'I am easily embarrassed.' I've never been sure just what that word means." Right out of Cleckley, the lack of normal social fear! Research by Landis, Zubin, Page, and Katz at New York Psychiatric Institute revealed that many such items found on inventories built by academic, nonpracticing psychologists did not "work" in psychiatric populations. It seemed that one should not look upon the response to a verbal item on a structured personality inventory as merely a carelessly framed surrogate for what a patient would reveal in a diagnostic interview, let alone a psychotherapeutic interview of some depth conducted by a sensitive, perceptive clinician. Hathaway and McKinley conjectured that inventories such as the Bell, Bernreuter, Laird, and Heidbreder were not useful clinically partly because they were based upon the idea of obvious "face" validity for items, sometimes combined with rather crude measures of internal consistency, but also because the dimensions assessed were not clinically relevant to the diagnosis and treatment of mental patients.

Hathaway, although an academician, was in the habit of speaking somewhat scornfully of "academic psychologists," by which he meant professors of psychology who were interested in personality and built tests of this kind, but who had had little or no contact with patients suffering with full-blown mental diseases and who knew practically nothing about medicine. In the same vein, he had a distaste for what he called "captive fake clinics," that is, "clinics" under the wing of psychology departments which had no psychiatric personnel and, as he used to say, "don't deal with anybody crazy or anybody who has anything more wrong with them than a mild case of homesickness in a college freshman." Looking back I believe that this attitude on his part was somewhat exaggerated and parochial, but I am still aware of the difference between clinicians whose experience has been mainly in that sort of setting. In the days before more stringent requirements for medical personnel was set up by accreditation rules, I had always thought that I could tell, not perfectly but better than chance, whether a psychologist publishing in the area of psychopathology and personality assessment was a real clinician who had dealt with mental patients and worked with psychiatrists, or one of these academic psychologists that Hathaway distrusted.

My first publication, "The Dynamics of Structured Personality Tests" (1945), was in response to a paper by Max Hutt on projective methods. I argued that structured tests like SVIB or MMPI should not be viewed as superficial approaches trusting the accuracy of "mere self-report," but were samples of verbal behavior that could be treated in a psychodynamic way (e.g., the "subtle" items on the Hy key reflect the hysteroid preference for repression and denial as defense mechanisms, never mind how objectively correct their content). This I tried to link up with the "blind empirical keying," not perhaps very successfully, by contrasting SVIB and MMPI with face-valid tests (e.g., Bell, Bernreuter). Although I now think the pure "dustbowl empiricism" keying doctrine too strong as I presented it 44 years ago, the paper made several points important at the time and is still being cited. It's an example of how something can be a half-truth worth pressing hard at a particular stage of scientific development.

There was no pressure at Minnesota to do a doctoral dissertation on the MMPI. My first thesis ideas involved the Rorschach or the T.A.T., I suppose because of my psychoanalytic interest, but it was easy for Hathaway to convince me—not by any contentiousness against projectives but by simple methodological points—that the designs were not capable of answering the interesting questions I was trying to put and, if souped up adequately, were too grandiose for a doctoral dissertation.

My final choice of a dissertation topic was a fortunate one as it made a respectable thesis and was fun to do, so I am one of those few academics who can look back at the dissertation period as pleasant and having yielded respectable work. The continuation of that work by Hathaway and myself for several years subsequently was also a worthwhile contribution and an enjoyable one for both of us. The topic arose from a confluence of several factors. Hathaway and I were interested in the psychological source of "false positives" on the MMPI. Three factors had aroused my curiosity about this problem. First, I had several friends and relatives who, having taken the MMPI out of curiosity, generated quite pathological profiles. I knew these people intimately enough to be confident that while they may have had their problems in the psyche, they did not have a diagnosable mental disorder, they were not in therapy, and they were functioning academically, socially, and sexually. I had also been interested in the history of the Humm-Wadsworth Temperament Schedule (from which many of the MMPI items were borrowed); that test included a so-called "normal" component suggested by a theory of the psychiatrist Rosanoff. He conjectured that there was a sort of steadying, stabilizing, or "normalizing" component of temperament that acted on pathogenic traits of the psyche, the way we think of modifiers that protect against the development of a genetic disease. Third, I had listened to recordings of Hathaway's psychotherapy sessions with clients that Howard Hunt (who took his degree with Hathaway the year before I did) referred to as having a "psychiatric hypochondriasis." They weren't really hurting very much, but they thought they were, with excessive introspection and preoccupation with signs of poor mental health—a syndrome confined almost wholly to intellectuals familiar with psychological jargon.

So I embarked on a project of constructing a "normality scale" for the MMPI, proceeding according to the accepted blind empirical keying method by item-analyzing the entire pool of 550 items on psychiatric patients whose MMPI's were matched individually, within a point or two scale by scale, with profiles drawn from the general file of Minnesota standardization "normals." The resulting scale I christened *N*. Studying the item content and the (sizable and consistently patterned) correlations with clinical scales and with unpublished nonclinical scales derived in a variety of ways, I became convinced that I was not measuring a "normalizing" buffer or safety component of temperament à la Rosanoff, but rather a test-taking attitude. (I had to get permission to change the thesis title from "An Investigation of the General Normality Factor in Personality" to "An Investigation of a General Normality Factor in Personality Testing.") The statistical rationale for applying such a scale had been provided in the discussion of suppressor variables in Paul Horst's *Prediction of Personal Adjustment* (1941).

After my doctorate Hathaway and I embarked on a project improving the suppressor variable, or test-taking attitude, calling people who got high scores "plus-getters" and people with low scores "defenders." We had the clinical impression that in some subjects plus-getting was downright faking at being bad, in others a cry for help, in others deviant semantical habits, and in others what has been called acquiescence. We studied various groups such as patients in a psychiatric unit under court order who obtained normal profiles and were presumably being defensive and nursing and medical students who attempted to present them-selves either as mentally ill or as paragons of mental health. The items in my N-scale being culled more carefully, we finally ended up with a smaller set of items that behaved consistently in many substudies; this we called *K.* As in my dissertation, the relationships of K with the clinical scales and with the various trial keys that had been developed in finally choosing K allowed a coherent interpretation. The correlations were good-sized ones, holding up in normal and abnormal samples, in both sexes. Factor analysis of a half-dozen scales of suppressor type, constructed in very different ways and in different populations, yielded one large factor which accounted for all of the communal variance. We called it the *K factor,* published in Paterson's journal (Meehl and Hathaway, 1946).

We did suggest a possible psychological relationship between the K factor as a test-taking variable and the opposite poles of hysteroid and obsessional personality, and noted a mysterious relationship to education and social class which we didn't explain. But our emphasis in the original article on the K factor and in the subsequent paper with McKinley (McKinley, Hathaway, and Meehl, 1948), showing the optimal amount of statistical correction as a suppressor, focused mainly on the psychometric suppressor function. Subsequent research has made it clear that the truth about the psychological nature of this factor lies somewhere between our emphasis in the K articles and my original intent when investigating the Rosanoff notion. The K factor is not *merely* a test-taking attitude but has a somewhat broader meaning that one might characterize psychodynamically as the adequacy of repression, suppression, and denial as defenses. Most MMPI users consider a moderate amount of elevation on K as being healthy and only an extreme deviation as having pathological significance as in a hysteroid character or gross dissimulation.

Another of my early publications on the MMPI was the first "profile sorting" study in which the emphasis on the profile pattern, already generally shared in Minnesota circles, rather than doing single significance tests on scales against single formal diagnoses for which the scale was named, yielded positive results. My paper on profile analysis (Meehl, 1946), was adopted with improved "objective" profile pattern rules by one of my first doctoral candidates, Donald R. Peterson, in an impressive study (Peterson, 1954) involving patients who were diagnosed anxiety neurosis when seen but whose MMPI's appeared schizophrenic by the rough psychotic/neurotic profile rules I was then using. The MMPI, on follow-up several years later, turned out to be right more often than the psychiatrist, if we define "right" as predictive of a subsequent hospitalization with florid schizophrenia. This finding set my switches to be receptive to the concept of pseudoneurotic schizophrenia in the classic paper by Hoch and Polatin (1949).

Today, after Goldberg, Dawes, Weiner, and others have shown that linear combinations, even nonoptimally weighted, of variables can do about as well as configural approaches, most MMPI users still believe in eye-balling the configuration, whether or not they use any of the formal cookbook rules. Out of that early work of myself and Peterson, combined with the implications for profile interpretation of the clinical/actuarial comparisons (see below), and doubtless influenced by hearing my lectures on the problem in the introductory clinical psychology course, Minnesota Ph.D.s Marks and Seeman, and then Gilberstadt and Duker, developed the first "codebooks" for configural analysis of the profile generating trait symptom attributions of the patient. It remains unsettled whether Goldberg and Co.'s strong generalization that "linear composites are good enough" applies to the kind of configural taxonomy presented by these investigators and their computerized successors, Butcher, Caldwell, et al.

In my presidential address to the Midwestern Psychological Association (Meehl, 1956a) I argued strongly on philosophical, mathematical, and clinical grounds for development of "mechanical" or objective, actuarially based profile interpretations. My student Charles Halbower showed that actuarially derived attributions (based upon therapists' blind Q-sort procedures) did markedly better than experienced MMPI interpreters in describing patient's personalities, *both* in descriptive and psychodynamic aspects. The differences he found between the two methods of profile interpretation were not merely statistically significant but reflected a sizeable improvement in the percent of variance validly predicted. I then conducted a large-scale study with profiles from a variety of clinical installations in which 29 local clinicians, ranging from graduate students to seasoned MMPI interpreters in the VA clinic and on the University faculty, the diagnostic discrimination being between psychotic and neurotic diagnoses, did not do as well as a system of rules of highly configural character developed by Grant Dahlstrom and myself. I am still of the opinion that it is hard to find any clinician who can interpret the profile as well as actuarially based methods. That opinion is not, of course, influenced by the proliferation of insufficiently validated computerized interpretations presently available. The main point of that early work is not that one could automate it, or program a computer to do it. The main point was that the basis of the inferences was actuarial <u>and</u> properly cross-validated. Halbower, for instance, showed that the four particular profile patterns he had picked out to study held up pretty well in the sense of validity generalization from an in-patient to an out-patient setting.

In 1951 Hathaway and I published the *Atlas for Clinical Interpretation of the MMPI*, presenting actuarial data on curve types (grouped by the numerical code he had recently invented) and case histories of patients with various codes. In the early 1950's we wasted considerable time and taxpayer money trying to compare the efficacy of a half-dozen measures of profile similarity, the results being so weak and inconsistent that we never submitted it for publication. We had not examined critically the whole notion of "overall similarity" between two personalities and concluded by wondering whether it could mean anything either clinically useful or theoretically illuminating.

My Midwestern Presidential Address led to an episode which puzzled and troubled me at the time as reflecting a serious problem in the profession. Though aware of the tension between clinical practitioners and academic experimental psychologists, I was surprised by its emotional intensity and was not skillful at defusing it. In presenting empirical data relevant to the idea of formalizing profile interpretation rather than "clinical eyeballing," I had told a couple of funny stories, employing some snide expressions about clinicians who reject objective data. Following the talk, which was well received both by scholarly clinicians and nonclinicians, I was invited for drinks in the hotel room of a distinguished experimental psychologist. There were a half-dozen of his experimental brethren along with two academic clinicians. The general flavor of the discussion was "Meehl, you sure gave those clinicians a good beating," an overinterpretation of my message which I found troublesome but let pass. The sentiment was that it was fine to see a clinical psychologist who also ran rats and knew how to take a partial derivative getting elected to a prestigious office and thereby provided with a big audience. The experimentalists had not seen my recent book on clinical and statistical prediction, but via the *anti*-actuarial arguments in that book, one of the clinicians was able to bring up the subject of the clinician's "third ear" and those kinds of inferences about the psychodynamics or historical past that it would be hard to imagine objectifying.

That there were such "pro-clinical" examples in the book came as a surprise to the experimentalists, and I was asked to illustrate this by examples. I used what to me are the most striking examples of an inferential process difficult to actuarialize and objectify, the interpretation of dreams in psychoanalysis. I had not then completed a full-scale analysis but I had some 85 couch hours with a Vienna-trained analyst, and my own therapeutic mode was strongly psychodynamic. I recounted examples from scholarly sources (e.g., Reik's *Listening with the Third Ear*) and some that I considered punchy and fascinating from my patients. The glowing warmth of the gathering cooled noticeably. A well-known experimental psychologist became suddenly hostile. He glared at me and said, "Now, come on, Meehl, how could anybody like you, with your scientific training at Minnesota, running rats and knowing math, and giving a bang-up talk like you just gave, how could *you* think there is anything to that Freudian dream shit?" I made the mistake of raising sophisticated epistemological questions, including some notions from current philosophy of science with which they seemed unfamiliar and perceived as obscurantist. It didn't degenerate into a real fight, but when I left the gathering I felt much less an honored pal of experimental psychologists than when I entered the room! After all these years I am still not clear in my mind about the extent to which this reaction by academic experimentalists to psychoanalytic concepts is mainly a matter of lack of exposure, or the kind of defensive mechanisms that Freud and other analysts have used ad hominem from time to time.

My teenage interest in logic and epistemology was focused on philosophy of science by my college freshman year, and while I did very well in science courses and found them interesting, books like Reichenbach's *Experience and Prediction* (1938) were more exciting.

My teenage interest in logic and epistemology became focused on philosophy of science in my senior year of high school, and I must have become aware of logical positivism and its derivatives by the time I was a college freshman in spring 1938; I recall discussing the logical positivist view with fellow students at lunch, and I have a copy of Hans Reichenbach's Experience and Prediction (1938), as a Christmas present from my girlfriend that year. Although I was enrolled as a premedical student and enjoyed my basic science courses in zoology, physics, chemistry and especially psychology, getting top grades in all of them without any trouble, I found reading books like Reichenbach's or Ayer's Language, Truth and Logic more intellectually exciting than the sciences. The older brother of one of my high school friends was a teaching assistant in psychology and it was through him I came across S. S. Steven's influential paper "Psychology and the science of science." My knowledge of philosophy was largely self-taught. Prior to the arrival in my senior year of Herbert Feigl I had only taken two classes in the philosophy department, a five credit class taught by its chairman George P. Conger, a traditional history of philosophy with an emphasis upon the Greeks, and a superb course in logic of science taught by a then unknown assistant professor named Donald Oliver. I only moderately liked the chairman's course, and found it hard to appreciate Plato to the extent that was apparently expected, since it seemed to me that many of Socrates' arguments were rather shoddy and tendentious, and because of my interest in philosophy of science I found it hard to identify with Socrates' ethically oriented enterprise. I still think the intellectual merit of the Platonic dialogues is exaggerated and that intellectuals are supposed to have a high regard for Plato in the same way that it is de rigueur to esteem Shakespeare, Bach, or whatever. For the first two years of my undergraduate education I was fairly orthodox logical positivist and, like many undergraduates under that influence, was rather free with the pejorative adjective "meaningless." In my defense I will point out that I was not satisfied with the alleged proof of the position. Ayer's book was sort of a positivist Bible. I read it and reread it, and my friend George H. Collier, subsequently known in experimental psychology for his important work on drives, realized that in the earlier chapters of the book Ayer kept referring to "the demonstration of the verifiability criterion of meaning in chapter seven," but when you read chapter seven the demonstration did not quite come off. I think we were aware, even before Feigl came here, that it was misleadingly called a demonstration, and instead should have been called an attempt at a persuasive definition of cognitive or empirical meaning. I was also aware, I don't know how, that my teenage hero Bertrand Russell, despite having been one of the father figures of the positivist movement (via, for example, his influence on Carnap and Schlick), did not accept the verifiability criterion. He took the sensible view that if he understood the grammar of a language and he understood (approximately) the meaning of the words in a well formed grammatical sentence in that language, then he would, more or less, understand the sentence, whether or not he knew how to test it. I don't know exactly when I first came across that statement of Russell's view but I remember being immediately impressed by its cogency.

In 1940 Herbert Feigl, the Vienna Circle member who introduced logical positivism to English readers, joined the Minnesota faculty. Mostly self-taught, I was pleased when he said I had a better grasp of the subject than most fresh Ph.D.'s in philosophy, which shows one *can* learn about a subject without being lectured at. (Most faculty seem unable to believe this well-attested truth.)

… Herbert Feigl came to Minnesota from Iowa and I remember how thrilled I was at the lecture he gave, well attended by a couple of hundred people in the big lecture room of the physics building, of the core theme of logical positivism, the claim "there are no synthetic a priori propositions." Philosopher Alburey Castell, who was antagonistic to the positivists on several counts, somewhat discombobulated the lecturer afterwards by asking what kind of a proposition that was; I do not believe Feigl gave a satisfactory answer, and I know today he would say that he had not. I took Feigl's course in advanced logic which used Tarski's text, and a course entitled "Philosophy of Science" and found it thrilling, I suppose partly because of my knowledge of Feigl's role in the Vienna Circle. He was one of the two people who could be counted as initiators, because he suggested having regular meetings and he co-authored with Alfred Blumberg the first paper in the English language on the Vienna Circle's position, published in 1931, where they christened the movement in English by the title of that article, 'Logical Positivism.' It was also Feigl and I believe Kaufmann, who were directly responsible for Ludwig Wittgenstein's return to philosophy from his period of the 1920's (turning aside from it and totally rejecting it) by persuading him to attend a lecture by Brouwer which so irritated Wittgenstein that he revived his philosophical interest. Unlike his suggestion to start regular meetings of the Vienna Circle, and unlike his introduction of the phrase 'logical positivism' into the English language, persuading Wittgenstein to re-enter philosophy Feigl considered an unfortunate event since he remained unconvinced, despite the current adulation in some quarters, that the later Wittgenstein made significant contributions. In my work as a psychologist I have been aided by the writings of a variety of philosophers who disagree with each other in fundamental ways, some quite passionately. Such diverse people as Bergmann, Braithwaite, Carnap, Feigl, Feyerabend, Glymour, Grunbaum, Hempel, Lakatos, Maxwell, Moore, Nagel, Pap, Popper, Reichenbach, Russell, Salmon, Scriven, and Sellars have stimulated and illuminated me, but I have never come across anything either in the later Wittgenstein or in "ordinary language" that was the least helpful to me as a psychologist. I am continually amazed at the esteem in which these thinkers are held among many philosophers.

My somewhat radical views about higher education (as expressed in a still unpublished MS "The Seven Sacred Cows of Academia") stem partly from my experience as a largely self-taught amateur philosopher. When Feigl arrived here I had read enough philosophy of science and general epistemology on my own that, after the oral examination that was required if you wanted to get an A in his courses and a term paper I wrote on John Maynard Keynes' Theory of Induction (in his Treatise on Probability), Feigl told me he was amazed at an undergraduate knowing so much about the subject, that so far as he could tell I was better informed and better able to discuss philosophy of science that the average Ph.D. candidate in that field. The point being that you don't have to learn something, even in considerable depth, by going to a classroom and writing down what somebody standing up in front says to the group. The extent to which American academics identify studying something with taking a course, and then in turn taking a course is identified with the standard lecture mode, continues to puzzle me, as many scholars can report the same kind of experience I have had in this matter.

Feigl was slightly heretical among positivists because he worried about the mind/body problem, the justification of induction, and the reality of the external world. He was not a strict "operationalist" and was sympathetic to psychoanalysis. From the first class I had with him as a senior, we got along famously. After my Ph.D. we co-led a seminar in philosophical problems of psychology. In 1947 the philosopher Wilfrid Sellars came to Minnesota and a group of us began to meet one night a week at our homes to discuss epistemology. In 1953 Feigl, Sellars, and I founded the Minnesota Center for Philosophy of Science, which became the model for other such centers around the world. Eminent philosophers and scientists came to the Center for conferences, some for longer visiting professorships. The Center has been highly productive, its renowned *Minnesota Studies in Philosophy of Science* having recently published volume 12, with others in preparation. It is hardly necessary to say that my writings on methodological problems of psychology with Cronbach, MacCorquodale, Golden, and Rosen, as well as solo have been influenced by my Center connection. My papers on substantive matters (e.g., theory of schizophrenia, latent learning, taxometrics, prediction, psychoanalytic inference, psychiatric diagnosis) all show this influence clearly, whether or not I explicitly invoke philosophical concepts. The main change in my views over the years has been toward greater tolerance of "open concepts" and the recognition that what some psychologists proudly label "operational definitions" are pseudo-operational. For a short time I counted myself a Popperian, but today I am a "neo-Popperian" philosophical eclectic.

My reservations about strict Vienna logical positivism were of course, strengthened by Feigl because, while he still called himself that, he was starting to shift to the phrase 'logical empiricist.' He had always been slightly heretical in the Vienna group because he did attach meaning to certain components of the old mind/body problem which Carnap and the rest of them tended to liquidate as cognitively meaningless. He also thought it necessary to provide some sort of justification for induction. And he had a strong streak of scientific realism which became even stronger under the influence of the young philosopher Wilfred Sellars who came to Minnesota shortly after World War II. Sometime in the late 1940's a group of us (called "the pentagon" because initially we were five in number) began to meet weekly at one another's homes in the evening for a discussion of philosophical matters. Philosophy of science was the emphasis, although general epistemology, and even kosher metaphysics, were also allowed. I was the sole non-philosopher in the group, the other four members being Herbert Feigl, Wilfred Sellars, Burnham Terrell and John Hospers (who subsequently had the distinction of being the first presidential candidate to run as a Libertarian!). Subsequently, the pentagon became a hexagon with the arrival of May Brodbeck, a student of Gustav Bergmann from Iowa. I look back upon these pentagon meetings as the most intellectually interesting that I have had, and I must confess that I have not found many group discussions devoted to matters psychological or psychiatric as much so. Sometime in the early 1950's, Feigl and I began offering a graduate seminar in philosophical problems of psychology which was taken by a few of the psychology doctoral candidates but mostly by philosophers and a few from other fields. In the early 50's Feigl or Sellars conceived the idea of giving these discussion sessions a formal academic status. Out of that grew the Minnesota Center for the Philosophy of Science, the first of its kind in the world, and which, because of its generative stunning success and the series of volumes it produced, became a model for other such enterprises elsewhere (e.g., Indiana, Pittsburgh, Toronto, Boston). We obtained a grant from the Hill Foundation, a Minnesota charitable outfit, and since the psychologists were the scientific group on the campus most interested in philosophy of science at that time, it was decided to spend the first three years of the Center, which began to exist formally as a special non-teaching unit in the College of Science, Literature and the Arts in 1954, on philosophical problems of psychology with specific focus on the mind/body problem. I was on a half time appointment to the Center, and being department chairman and with my various other obligations, something had to give, so I arranged with the Dean to teach no courses except the philosophy of science seminar during a three year period. This experience gave me an insight that was surprising namely, that I did not miss classroom lecturing at all. When I went back to teaching lecture courses at the end of that time, I had no feeling of eagerness to resume it. If the Dean had said "Meehl, you aren't going to teach any more lecture courses for another three years," I would have been quite happy. This despite the fact that whenever I administered the teacher rating scale to see how the students perceived me, I got very high scores on almost all of the rated dimensions and my overall impact as a college teacher was consistently in the top 5% on the all college norms on that instrument. I suppose the moral of that anecdote is that you don't necessarily want to do something because you do it well. I was aware that students considered my introductory clinical psychology course (mainly for graduate students but open to seniors because we wanted some premeds in there) was looked upon, along with Charles Bird's abnormal psychology course and MacCorquodale's course in advanced general psychology, as among the most stimulating courses in the department. Many students who had no direct interest in clinical psychology—students in other fields such as industrial, social and experimental psychology—took the course "just to hear Meehl lecture." I don't know what a good Skinnerian would say about this, since it would seem on the face of it to be a case of something being reinforced but ….

The funds available to the Philosophy of Science Center in those early years made it possible for us to invite distinguished philosophers of science and also some younger "unknowns" whose work we thought was promising (e.g., Paul Feyerabend). Also the Center could travel to places when somebody else was unable or unwilling to come to us. I recall, for instance, the first time I met the great Rudolf Carnap at his home in Santa Monica. I was instructed by Feigl that it was not necessary to call him "professor" or "Dr. Carnap," but that following the Vienna practice, everyone was on an untitled <u>last</u> name basis: Carnap, Feigl, Meehl, Sellars, Scriven. I was also informed that at the evening meal one did not discuss philosophy of science although it was all right to talk gossip about various philosophers of science (who influenced whom and where somebody went, etc.). After dinner any discussion of philosophy matters was absolutely interdicted, because talking about those subjects sometimes got Carnap so excited that he would have severe insomnia.

I was told that he never read anything at night other than detective stories. Upon our arrival, while Carnap's wife was preparing some refreshments and the conversation was desultory, I found a copy of my monograph on "Prediction" on an end table, and I was thrilled when I opened it to see various sentences underlined with penciled comments in the margin in Carnap's German script. We had never met, but, Feigl had told him about his psychology sidekick Meehl and about Meehl's book and I was pleased to see that the great man, preparing for our visit, had read my little book and reflected upon it.

The Philosophy of Science Center, on whose staff I remained without cross charge after that first three years, continued to produce volumes of distinction, the Minnesota Studies in the Philosophy of Science, which is still going strong, Volumes XI and XII being in press at this writing. The enterprise was a roaring success and some administrators and heavy weight faculty who had initially been skeptical and voiced mild opposition to it (some faculty regularly object to any non-teaching unit, because they don't like the idea of privileged drones) became enthusiastic when its high visibility on the international philosophical scene became obvious. The only major disappointment was that we did not succeed in arousing much interest in any of the other scientific disciplines on campus other than psychology. There was one physicist who attended meetings and published an article in one of the volumes; there were no mathematicians or economists, and despite Feigl's interest in biological science we never succeeded in arousing much interest in the life sciences faculty. The main contributors to the volumes on physics or mathematics were scientists or philosophers who came here for symposia, or in some cases were appointed to this Center's faculty for longer periods. After Feigl's retirement the Center lost some of its vigor and began to run into difficulty getting grant support for conferences and visiting appointments. I do not know the reason for this, although I was in close touch most of this time and attending some of its sessions. Observing the Center as an early participant and co-founder has lead me to speculate about whether interdisciplinary academic outfits have some mysterious property that gives them a kind of "natural life-span," such as Spengler, Toynbee and Quigley believe to be true of nations or whole cultures. I don't know how many examples there are around the country, but there are others besides ours in which a period of initial enthusiasm and high productivity, such as the Yale Institute for Human Relations, is followed by a period of lessened energy and excitement. There was also grudging support or active hostility from the Philosophy Department, which I feel free to mention because it is no secret on this campus, well known at all levels up to top administration. Departments' ambivalence to a research unit that could constitute its main claim to fame, sometimes its only claim to excellence, is a puzzling phenomenon that seems fairly frequent.

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Is what follows the missing matter:

Since some of the writings which made me a visible psychologist as to contribute an autobiography to this series are at least as "philosophical" as they are "psychological," I think it relevant to say something here about my philosophical development. I have moved from the somewhat dogmatic and simplistic logical positivism which I originally espoused, although even there modified by the quasi-realist arguments in Reichenbach's 1938 book and by my dissatisfaction with the status of the verifiability meaning criterion, through the first slightly and then greatly modified positivism that Feigl came to refer to as "logical empiricism," to my present position which could be called "empirical realism," if any name at all is appropriate for it. With the abandonment of strict Vienna positivism led in my case partly under Feigl's influence and partly simply my own reflections on various concepts in psychology (for instance, especially the Freudian ones, but also concepts of psychometrics and learning theory) away from strict operationism as a philosophy of psychological science. It puzzles me today to find psychologists who are still saying that all terms in psychology must be operationally defined, when I don't think of a single philosopher of science who espouses strict operationism as an adequate analysis either from the standpoint of reconstructing history of science or from the standpoint of "inductive logic" and epistemology. I came by 1950 at the latest (and it would perhaps be better to say 1948, the date that MacCorquodale and I published our paper on hypothetical constructs and intervening variables) to realize that in psychology as well as in the other sciences, including advanced sciences like physics, chemistry and genetics, and including scientific concepts in their early stages as well as in their most advanced stages of development, only a proper subset of the theoretical terms are tied in a fairly [direct] way to observable predicates and functors. (I set aside here the controversy about precisely what is "an observable.") The most powerful theoretical concepts in the other sciences are many steps removed from the observational data, and linked to it by complicated and sometimes only probabilistic inferential steps. It puzzles me that many psychologists and sociologists are still teaching their students that "all operational definitions have to be observable," and I am convinced that most such so-called operational definitions are a fake. Arthur Pap's paper (1953) on open concepts was constantly referred to in the early Center meetings, and Pap spent a couple of quarters as a visitor to the Center as well as going to one of the meetings we had with Carnap in Los Angeles.

I was thrilled when I read the first English translation (1959) of Popper's Logic of Scientific Discovery and for a few months considered myself nearly a Popperian, although I did not understand Popper's strong line against induction, and I have never seen why his denial that there is any such process was crucial to the rest of his position. The article on construct validity which Cronbach and I published in 1955 was an outgrowth of the deliberations of the special APA Committee on Test Standards for psychology, where I think it correct to say I initially fought an uphill fight against Cronbach and all (or almost all) the members of that committee to convince them about the acceptability of open concepts and the incorrectness of the strict operationist philosophy of science. This was particularly difficult because, due to coming down with an attack of chicken pox (at age 31!), I was unable to attend the first meeting of that committee and their ideas along these lines had already begun to crystallize. I'm not clear whether Lee and I saw exactly eye to eye in writing our paper, and perhaps we didn't see its construct validity in quite the same way; students have told me that they think they can tell which sections of that paper were written by Cronbach and which ones by Meehl, because Cronbach's sections are more "operational" than mine.

Contrary to what some believe, knowing my interest in the philosophy of science and my connection with the Center, I have never advocated formal classroom exposure to philosophy as a suitable minor field for psychology Ph.D.'s. Some of my own Ph.D.'s, especially in the early days of the Center, seemed surprised when they suggested that they minor in philosophy and I told them not to do that but that I would give them a very short reading list of propaganda that would serve them well. I think it far more valuable for psychology students to study some mathematics (as distinguished from standard social science statistics) and computer programming. If they are interested in behavior genetics or psychopathology they should learn some biochemistry and neurophysiology. I agree with my colleague, David Lykken, and with Fred Skinner that the overall impact of philosophy of science on the social sciences has done at least as much harm as good, and maybe more. There is nothing intellectually healthy, for instance, about holding to operationism if it leads you to offer phony definitions that are pseudo-operational, and meanwhile leads you to exclude open concepts from your thinking that might be highly productive of interesting research ideas on the grounds that they are not operationally defined. Nevertheless, I see nothing obscurantist in my position, and the admission of open concepts because one recognizes their unavoidability and their ubiquity in all of the sciences, even physics, is not saying that since we are not tied to operational definitions and since we don't claim that everything we talk about can be accurately measured with present techniques, therefore "anything goes." The trick is simply in realizing that skepticism is not incompatible with open-mindedness, that there is a kind of hyperskepticism (springing partly from the psychologists' inferiority complex vis-à-vis the other sciences) that is actually a form of dogmatism, albeit negative. My friend MacCorquodale once said, "Maintaining an open mind is not like maintaining an abandoned open-pit mine with an invitational sign: Throw garbage in here." Needless to say, recognition that it is possible and desirable to be both skeptical and open-minded does not make it easy to apply this dual principle in practice. It is sometimes difficult—perhaps in the short run impossible—to decide whether allowing an "open concept" at the cutting edge of theoretical science is gullible, or preferring not to admit it (as too fuzzy) is being dogmatic. In the seminar in philosophical psychology that David Lykken and I give, I begin the course by saying loud and clear that it probably will not make them better theorists or experimenters, that it is mainly offered for fun, but that to the extent that it will bear upon their thinking as psychologists it is primarily prophylactic rather than constructive in aim. We hope, that is, it will give them rebuttals against philosophically naive arguments, especially arguments wherein someone is abusing philosophy of science—usually a philosophy of science a half-century out of date!— as an easy way to beat up on somebody's substantive theory that he doesn't happen to like. For some reason social scientists are more prone to this abuse of philosophy than scientists in the physical and biological fields, and we do our best to propagandize the graduate students against this type of abuse.

Even when I am not writing explicitly on some methodological problem, I know that what I have written about animal behavior or learning theory or psychometrics or psychoanalysis (or, more recently, in behavior genetics or taxometrics) is infused throughout with my philosophy of science knowledge and orientation whether I employ any of the technical terms of that discipline or not. So that while I tell students that they can get what they need by reading a couple of books and taking our seminar, spending other non-major course time on learning electronic laboratory equipment skills and computer programming and biochemistry, I do consider myself an exception. I believe what notions I have contributed that have any enduring value to the thinking of clinicians, for instance, would probably not have been generated by someone with only a superficial acquaintance with philosophy of science concepts. So I think that in my particular case a good deal of exposure to first class philosophers of science, reflection upon their writings, and extended discussions in the Center made a valuable contribution to my intellectual development as a psychologist, despite my belief that this would not be the case for most persons. Because of a certain "legalistic" side to my conscience, and perhaps also because of the strong empiricist emphasis of my undergraduate psychology education, it was important to me to retain my title as a Professor in the Philosophy of Science Center, and subsequently to receive an additional rank as Adjunct Professor of Philosophy. These "official job titles" meant I could spend quite a bit of my scholarly time and energy reading, thinking, and publishing in that area with a clear conscience. Albert Ellis would say that this is a foolish and irrational attitude, and I suppose he would be right about that. The fact remains that it has made me feel more comfortable than if I were seen merely as a psychologist who spends too much of his time doing something else than what is suggested by his academic title.

In one respect I have remained fairly close to the old positivist position. I tend to dislike the current stress on the psychology and sociology of knowledge, and I view some current developments which are enthusiastically greeted by some people in psychology as obscurantist in their likely effect, even if not so motivated. For example, emphasis upon the extent to which theoretical preconceptions influence what is observed is at times invoked as a justification for what I see as kind of a delighted wallowing in subjectivism. As I read the history of the other sciences, the chief way in which they have successfully handled the problem of the influence of bias and theoretical preconceptions on first level observational reports is <u>not</u> by delving into the psychology of cognition and perception (although the famous personal equation in astronomy is an exception here, important because it was the beginning of experimental psychology as a discipline) but by the careful training of observers so as to minimize the contribution of the personal equation. If that could not be done, impersonal instruments were substituted for the human eye, ear and hand, so that what had to be observed was a meter reading, or a count, or today frequently nothing but a digit on a computer printout.

I do not mean to suggest that reliance on the history of science by the younger philosophers of sciences is a mistake. On the contrary, I believe one bad effect of the logical positivist position was the extent to which they believed the whole business could be done from the epistemological armchair. The little history of science they used was in the form of pedagogical examples, rather than as an empirical (1930-1950) source of ideas about the process of knowledge growth. For that reason, we students were exposed in lectures and textbooks to the same standard list, e.g., Kepler, Newton, Einstein and the 1919 eclipse, the kinetic theory of heat, and the atomic theory. Very few examples were from the life sciences, and almost none from the social sciences. So I don't dispute that there is a sense in which the philosophy of science is an empirical discipline, so that treating it as a pure matter of syntactical analysis (as Carnap wanted to do) is neither philosophically justified nor turns out to be feasible. I now see philosophy of science as an attempt at a rational reconstruction of history of science, which includes criticism of certain episodes in the history, and which also allows for some scientists doing things that are a bit counter-rational yet having a certain success frequency, as Feyerabend points out. Admittedly, the problem of how to relate in a metatheoretical reconstruction the prescriptive and descriptive features of an empirically based philosophy of science is a tough problem awaiting solution by the new generation of philosophers and historians of science

After World War II money became available for rapid expansion of psychology departments, and we decided that theoretical psychology, especially in the "soft" areas of clinical, counseling, social, and personality, was underrepresented. By 1950 we had added a group of "Young Turks" (K. E. Clark, L. Festinger, J. J. Jenkins, K. MacCorquodale, E. Rosen, W. A. Russell, S. Schachter, and myself) who could outvote our elders, although it rarely happened. There were vague anxieties which began to surface in faculty meetings, and after one somewhat stormy session in which I had played effectively a clarifier-and-compromiser role, Mike Elliott told several of the Young Turks that he was resigning as chair and "you should make Meehl chairman." At first I flatly refused, but they worked on me in a series of meetings until I capitulated. I was a Minnesota Ph.D., with feet in both applied and theoretical camps, and trusted by both old and young. I felt an obligation to hold the crew together during the transitional storm, and of course it was a prestigious job at the age of 31. Status I like, but my power motive is singularly weak. I have A's on the C.P.A. and Public Administrator keys of the SVIB—the "managerial" and "let's do this rationally" side of my nature. I was a pretty good chairman, kept the job for six years, wrote an excellent department constitution, held things together until they settled down, made some superb appointments (e.g., Gardner Lindzey, Lloyd Lofquist, Marvin Dunnette), and count my administrative stint as a worthwhile social contribution and a personal growth experience.

The Psychology Department faculty expanded rapidly in the late 1940's, partly because of a general feeling in society after World War II horrors that social science was lagging behind physical and biological science and should be beefed up. We also realized that the Minnesota department was becoming slightly out of tune with the times and that the "soft" areas of psychology (personology, social psychology, clinical, etc.), both applied and theoretical, ought to be expanded. Elliott, despite his Yankee fiscal conservatism, realized this and made strong representations to the Dean so that we got extra money. Furthermore, John G. Darley, who had returned from the Navy with an interest in small group dynamics, was pressing for development in that area. Charles Bird was not doing experimental work in social psychology, his emphasis being mainly on the old work on crowd behavior, together with the more recent emphasis on attitude measurement, but Bird was not highly competent in mathematical psychometrics. The Laboratory for Research in Social Relations was set up around 1949, mostly with Carnegie Foundation funds. Dean T. Raymond McConnell was strongly oriented toward social psychology and specifically (he had sympathies) group dynamics. Reflecting the response to Hitlerism and Stalinism on the world scene, we thought there could be an integrating concept of "social responsibility" in the research conducted. It was originally interdisciplinary, including anthropologists, economists, sociologists, faculty from child development and educational psychology as well as the psychology department, the latter being in the minority. The Arts College Psychology Department brought three new young people on the staff, Ephraim Rosen (clinical), Wallace Russell (experimental psychodynamics) and Stanley Schachter (social). James J. Jenkins, although a Paterson PhD (differential psychology), was shifting his interest to psycholinguists and ultimately became an experimental psychologist of distinction. These men, plus MacCorquodale and I were all roughly of the same age and all had strong theoretical interests rather than the applied interests that had more or less dominated the department, with the exception of a couple of faculty, for so many years. This group began to refer half jokingly to themselves as the "young Turks," and I believe were called that by some of the older faculty. As would be predicted, some group tensions became mobilized, especially among the applied psychologists influenced by Paterson (several located elsewhere on the campus) who feared—understandably but incorrectly—that the young Turks would somehow erode or sabotage the "great Minnesota applied tradition." In 1950 there were some conflictful faculty meetings and after one of these, I do not now recall whether it dealt with the applied–theoretical axis or not, I took a mediator role and also "laid it all out" in what I thought was a clear, rational and fair-minded way in accordance with my "always be scientific and reasonable" self image. Immediately following that meeting, Elliott called each of the young Turks individually to his office and said to them (as he explained to me several months later) "Look, I am getting too old for this job and am no longer in sufficient touch with developments to stay on top of it. I'm going to resign as chairman. What you fellows should do is persuade Paul Meehl to be the chairman. Everybody respects him, he has a fair-minded approach, he has one foot in the theoretical camp running rats with MacCorquodale and the other in the applied psychology camp working on the MMPI with Hathaway. He is a Minnesota Ph.D., so Paterson and the older people will trust him. If he doesn't want to do it, you should talk him into it." This the young Turks proceeded to do, without at first letting me in on the Elliott story, and I flatly refused to consider it. I was not being coy, I simply didn't want to be involved in administration. I was already doing too many things: running rats, doing research on the MMPI, consulting twice a week at the Veterans Administration Hospital, doing therapy to pile up further clinical hours towards my ABPP requirements, teaching several courses, and writing my book on prediction. Three things led me to have ambivalence about flatly saying "No": (1) the altruistic argument, which I had to admit had validity, that I was the obvious best candidate that everybody trusted and that nobody would suspect of being anti-applied or anti-theory; (2) the considerable prestige involved in becoming chairman of one of the top psychology departments in the country at the age of 31; and (3) a lurking doubt about myself as able to function in this kind of role. Whereas I knew since early childhood that cognitively and in terms of social potency I could usually persuade others about ideas, I was not quite so sure that I could handle a bunch of prima donnas who were mostly as smart as I was, in a setting involving more possibilities of conflict than I had been accustomed to in the various groups where I had played a dominant role. "Ideas," sure. "Running things," maybe not. In short, I had doubts about myself as captain of the ship and that of course leads one to try prove that he can.

MacCorquodale and I tried to persuade the oldest of the young Turks, the only one on the faculty when we were in graduate school, Kenneth E. Clark, who we knew would be a good administrator (and who subsequently became one as department chairman, Dean at Rochester), but for reasons that have never been clear to me, he firmly refused. All he told me, years later, about his refusal was, "I didn't think I was ready for it." When MacCorquodale and I were unable to persuade him, first cold sober and then full of beer, to be considered, we dropped the project. The young Turks had a second meeting at Wallace Russell's home in which they really put the arm on me, and I had a combination feeling of a duty, because the department might be headed for trouble unless it had a leader who was both a theoretical and applied psychologist and who was skillful at integrating when possible and compromising when not, plus challenge, plus status. I succumbed and agreed to take the job. I was the last department chairman in the college who was not appointed for a specified three-year term after which, according to the present rules, things are up for grabs. I made clear to the then Dean J. W. Buchta (from whom I had taken freshman physics as a premed) that if it interfered with my research productivity or my maintaining a decent amount of clinical practice—I believed then, and I believe now, that a person ought not to teach courses in <u>any</u> applied area of psychology in which he is not currently engaged in some kind of practice—I would have to resign.

I quit, to everyone's dismay, because I got bored with it. Doubts I had as to my "social potency" were largely allayed. I exercised more leadership (e.g., strong urging of my views in faculty meetings) than is considered proper in these days of frenzied egalitarianism, and "lost" only one vote in six years, most votes being unanimous. I also learned two important facts: (1) bright, scientifically trained persons may become grossly irrational when issues of territory, dominance, and bonding are involved; (2) when you become alpha baboon, the communication tends to deteriorate. One knows these facts intellectually, but sitting in that chair gives a real appreciation of their power. Ethology rules the academy more than logic.

I have never regretted serving as the department chairman, partly because it gives you a perspective on what it's like to sit in that chair that I don't think you could get any other way. It also gave me a further appreciation of human irrationality. Although I was liked <u>and trusted</u> as a chairman, I early learned that the communication system tends to become warped even if you are a trusted figure, and that you cannot assume just because a person has a Ph.D. in a social science that he knows how to think rationally about human behavior or group action when it's not a subject of scientific theory but a group of which he is a member. I offer just one example: For many years the department had offered a sophomore level course called "Applications of Psychology" which some students took in the Spring quarter having taking six credits of Introductory Psychology with laboratory in the Fall and Winter, a course nominally run by Paterson (who gave one or two lectures) and taught by eight or nine faculty, each of whom came in for a short stint of one to four lectures. Such multi-faculty courses can be excellent under special circumstances, but they almost never are. In this case there was no common denominator conceptually, since anything vaguely "applied" went in there. Tinker talked about lighting levels and reading speed, I talked about the MMPI, Hathaway talked about hypnosis, and Bird talked about study habits. The result was a disorganized hodge-podge which didn't please either the professors or the students. We had student ratings of courses and non-mandatory ratings of instructors in the 1940's, partly as a result of Paterson pushing my <u>summa</u> thesis results; and while this course was often described by the applied people as a "feeder" to seduce students into majoring in psychology, there was not any statistical support to this idea, and the anecdotal evidence was to the contrary. Some majors told their advisors that they were almost deterred from a psychology major because of the applied psychology class being so boring and chaotic. The subject of what to do with this "problem" course had come up almost every year since I joined the faculty in 1944 as an instructor, so the notion that there was something wrong with it was hardly controversial. Professor Heron, who had begun to lose interest in learning theory and animal research and had started doing a good deal of hypnosis, teaching hypnosis to physicians and dentists, and seeing a few patients (without fee), offered to take the course. The idea was that while he would retain certain useful sections of it, such as how to study efficiently, he would give it somewhat more of a mental hygiene slant, and we would have at least one course that would be closer to what the typical undergraduate thinks he's going to get when he signs up for a psychology class. To improve Heron's morale and beef up his contribution to the department's program, and also to do something about this dreadful class seemed to me a reasonable approach. I circulated a memorandum among the brethren saying that Bill Heron had offered to do this, suggesting that they talk about it informally, and that one day we would put it on the agenda for a faculty meeting. There followed within twenty-four hours an intramural tornado. Paterson got on the phone to call up all of the applied psychologists around the campus, who then called their friends—even some non-faculty in the community—and within forty-eight hours the story was that "just as we feared, Meehl and the young Turks are out to destroy the great Minnesota applied psychology tradition." I could hardly believe my ears when people came to me anxiously asking what the trouble was, what terrible catastrophe was about to happen? Mind you, all I had done was circulate a memorandum suggesting conversations, and that we would put this proposal on the agenda for discussion at some faculty meeting! Well, we simmered it down, but I learned a lesson which I have never forgotten, namely, just because college professors are bright and educated and have secure jobs and, by and large, don't play rough with one another, and have habits of thinking critically and fair-mindedly in their subject matter domain, one cannot infer that they will think rationally or behave appropriately when the primate ethology of dominance, territoriality, bonding, etc. are involved.

I occupied the chair for six years, the first couple of years enjoying it greatly and realizing that my "A" on the Public Administrator and C.P.A. keys of the SVIB had some validity after all. There is such a thing as managerial motive, the desire to make something work well, even if the mechanism involved is a collection of humans, and I seem to have a dash of it. (It is not, I think, the same thing as n Dom.) [need for Dominance, from Henry Murray's personality theory] For the next couple of years I was no longer getting a big kick out of being chairman in either the prestige or managerial sense, but I was still doing a good job and nobody was unhappy with me. In the last couple of years I began to find it boring. Things that earlier had appeared as a challenge to my interpersonal skills or my intellectual clarification talents began to be experienced as irksome and in the last year downright oppressive. During the last year, my secretary had to lay the lash on me to get me even to do routine signing of things. Meanwhile, I developed some mild psychosomatic symptoms. My wife could tell when I came home and collapsed on the couch whether I had had what we called an "administrative day" or a "non-administrative day" (writing or reading or seeing patients or analyzing data, anything other than doing chairman-like things). She thought that I should quit the chairmanship because I was no longer enjoying it and in fact was not really being very good at it because I let things slide. My closest friend MacCorquodale advised the same. My physician told me that for the first time in many years I had a somewhat elevated blood pressure which he suspected was the explanation of the fatigability and headaches. My analyst who, being Radovian, was sometimes rather directive, said that so far as he could see I didn't enjoy it much anymore so why did I hang on to it? I thought that the combination of my wife, my best friend, my longtime family doctor and my analyst was a solid lineup of people to listen to, and since my strong impulse was to quit the chairmanship I decided to do so. I realized that this would come as a shock to people, none of whom except MacCorquodale knew I'd been thinking about it. I also knew that despite my slight decline in efficiency in staying on top of the paperwork, nothing really bad had befallen the department from this, so the odds were practically certain that the group—not just the young Turks but the older people as well—would pressure me not to resign. So I wrote a letter of resignation to the Dean and the first sign the faculty got of my decision was when I circulated this letter. In that way I avoided having to go through people trying to persuade me to keep it. It is interesting that although I had several months more to go at the time I resigned, the mere fact of that resignation resulted in an immediate lowering of my blood pressure and its associated headaches and tiredness.

As I say, I do not regret having spent time as a chairman. I think it was a period of personal growth, it was important to my self image to find that I could be "a leader of men," and it gave me some important insights into the human condition which I theoretically had before but not in such a blood-and-guts concrete way. I am I think somewhat more immune to shock in the presence of gross human foolishness than most people I know, and I think that serving as chairman is partly responsible for this attitude. Speaking less egocentrically, I think there is no doubt in the mind of anybody who was around during that transition period that I played an important role in keeping the thing steady, and turning us into a department more like that of the typical psychology department, although our attitude towards the applied fields is still stronger than many outstanding departments in the country.

After the tempest in the teapot over "ruining applied psychology," a lesson which I took to heart and modified my behavior accordingly, unless I'm repressing something all of the remaining six years ran quite smoothly. My "administrative style" combined genuine democracy and untrampled expression in faculty meetings and outside. I wrote one of the first formal constitutions in the Arts college. At that time the University Regents' rules did not require such, and I was flattered when the political science department requested a copy of ours in preparing theirs. It was a thoroughly democratic constitution both as regards votes and participation and various procedural protections (e.g., faculty must receive a memo in their mailboxes a week before a meeting, certain big topics have to be discussed in one meeting and can only be voted upon in a subsequent meeting), and I can say this was not a "fake democracy" but really functioned that way. Despite that aspect of the formal rules, the faculty at that time did not view the idea of faculty democracy as incompatible with the exercise of strong leadership by the chairman, and that included allowing the chairman to express his opinions in the discussion. In this respect we were departing from a core notion of parliamentary law, even though in other respects we followed Robert's Rules of Order. I did not, after my initial trauma about the applied business, run into any conflict over this. So far as I could make out then, and so far as people have spoken to me since, that strong expression of my own views on either big or little topics did not arouse any resentment and in fact the faculty relished it. I have a hunch, although I have no hard data to support it, that the combination of frenzied egalitarianism in our culture in recent years with great increase in complaints within the university as well as litigation in the courts, has had a chilling effect on leadership. Department chairmen have the idea that they ought not to have any views, or at least not let them be known vigorously in the meetings. I look upon this as an unfortunate development but I have no cure for it. I remember on one occasion the discussion of some mildly controversial issue, in which I had taken a definite stand in the discussion, became fragmented and desultory and, as often happens, somewhat irrelevant. I felt I had to put up with it patiently until it ran down, but Professor Longstaff put an end to the discussion not by moving the previous question but by saying "God damn it, we elected Meehl Chairman, let him be Chairman!" I believe I'm correct in saying in the six years of my chairmanship, during which we made numerous appointments and many changes took place, and a great deal of money became available for a variety of things, I "lost" only one single vote where my own opinion was definite and strongly expressed, and that was a vote that the group knew I really didn't care about. (It was merely on passing a resolution expressing an opinion concerning another department that was in trouble in the college.)

My general approach as chairman on a "charged matter" (after the tempest in the teapot my first year) was to speak with individual faculty that I had reason to believe might be skittish on some subject and to find out what they thought before telling anyone what I was inclined to think. I think it important in such matters to behave more like a Rogerian therapist than a psychoanalytic or cognitive one. I think college professors, even more than other people, attach a kind of intrinsic sanctity to their views on subjects, and to the senatorial notion of unlimited debate, so that if they get the feeling that a boss is trying to twist their arm before hearing them out or overpower them—even with what may be, on the merits as seen by an objective third party, a valid argument—they become restive. I of course made use of committees but I am sure the record would show much less use than is currently made in departments. I do not believe much in committees. Ideally, my view is that if we have a complicated problem the thing to do is to get the smartest person you can find who is willing to spend time thinking about it and boning up on relevant facts, ask for a very brief "report" suggesting what to do, and give that report to be critiqued by the second smartest person you can find; then read the original report and the critiquer's report, come to a conclusion, and attempt to persuade the group to adopt it. I didn't proceed this way formally much of the time, although I did on some occasions, and frequently did so by oral exchanges. It is, of course, important not to carry on a logrolling or plotting activity in matters controversial, nor to give the appearance of doing so. On the other hand, it's stupid to pretend that you don't listen to some people more than others, because nobody will believe that anyway, and unless you are a frenzied egalitarian, there is no good reason to say that the chairman should never find some people's thoughts more useful than other people's. Surely psychologists know that some persons think better than others? One important feature of our constitution that I saw as a good compromise between democracy as formally defined (and strictly adhered to within the rules!), and the fact that Michels' Iron Law of Oligarchy begins to apply even when you have a couple of dozen people on the faculty, a recognition of individual differences in competence—there are people who are extremely bright and logical in thinking about their research and become somewhat irrational in thinking about departmental matters—was a constitutional provision for an executive committee. While the constitution has been amended since I was chairman in the 50's, we have never had the faintest indication of anybody's dissatisfaction with this part of it, and it works very well. The executive committee was elected by the faculty and two members had to be elected from the top half of the age distribution and two from the bottom half of the age distribution. (That's the only provision from the constitution which has been subsequently amended, because it turns out that the young faculty are not that suspicious of the older ones, rather there's a tendency in the whole group to look upon certain of the older members as wise and experienced in departmental affairs, whose judgment they are willing to trust. We regularly have some younger faculty on the committee, but not by rule. Sometimes all of the members that have been elected democratically turn out to be full professors.) Terms are staggered, and one may not serve two consecutive terms. The main thing about the executive committee was that if an urgent matter came up (e.g., over the summer or between quarters) and the chairman was in doubt whether he could act without calling a meeting of the faculty to discuss it, he could convene the executive committee (or if they were not all around in residence, phone them). Their first judgment was to advise him whether a faculty meeting was required. If they decided a faculty meeting was required, the chairman was bound to call one, however inconvenient, to discuss the matter. If the executive committee held unanimously that a faculty meeting was not required, their second function was to "advise and consent" to whatever decision he proposed to make without one, so that in addition to having their advice substantively the main point was that the chairman was shielded from a possible objection that he made the wrong meta decision on the procedural question of whether he was entitled to decide without a faculty meeting, since the democratically elected committee had unanimously held that he could, in consultation with them. The faculty might still complain of high handedness, but they would be complaining against high handedness not of the chairman solo but of the chairman and four people that they had elected to perform this advisory job. We are pleased with the system, and as I say nobody has ever proposed getting rid of it or altering it in any important way.

When I was a student and young faculty member, the big debate in learning theory was between Hull and Tolman and had in the 1940s converged on the phenomenon called latent learning. MacCorquodale and I published several experimental papers on that subject, some of which are still being cited. We showed, for example, that the Blodgett effect—a steep drop in time and errors following the first goalbox feeding—could be produced even when the feeding was not in the goalbox or at the end of a run, but in an extra-maze box, elevated and behind the entry box. Perhaps the Blodgett effect was attributable to a kind of "drive-conditioning," yielding a boosted Hullian drive-multiplier on differential habit strengths accumulated during the "latent" period. We also showed that rats make nearly errorless runs after prolonged free exploration of the Blodgett maze with no food reward involved. On the other hand, hungry rats who have been running the maze to goalbox food reward with culs closed will, when culs are open for the first time, enter every cul to get nearly 100 percent error scores. On the theoretical side, we published a tentative formalization of Tolman's expectancy theory, since its inexplicitness was one of the major Hullian complaints (MacCorquodale and Meehl, 1953b; 1954).

Due to Professor Tinker's "applied" interest in topics like eye movements in reading and illumination levels, the major course in human experimental psychology was rather weak on the theory side. Learning theory was a major interest of Bill Heron and in his courses on "Psychology of Learning" and "Animal Psychology" there was a good deal of time spent in class plus reading assignments on the theory of animal learning. Heron's mild sympathies were with Hull, but he also did a surprisingly convincing job defending Guthrie's single postulate theory, which, as one looks back, would seem an impossible task but he did it. The latent learning controversy, perceived from the 30's into the 1950's as the big empirical touchstone between Hull and Tolman, was at its height when I was a graduate student and a fledgling Ph.D. I had somewhat more sympathy with Tolman than my peer group or Heron, I suppose partly because of Tolman's philosophy of science concerns. Reading Tolman's big 1932 Purposive Behavior in Animals and Men book as a senior had a marked influence on me. Kenneth MacCorquodale, whom I had first met in Heron's class as a junior and who was a good friend by the time we entered graduate school in 1941 as teaching assistants, was a Heron Ph.D., although he had an M.A. in Psychometrics and had actually worked for awhile as a counseling psychologist in the student counseling bureau under Jack Darley. We shared a strong interest in the theoretical problems of animal learning. I did two unpublished rat studies in my senior and first graduate year, one on Spence's Theory of Discrimination Learning and one (ill-designed) project in the famous Heron Automatic Maze, a twenty-unit multiple Y maze which operated automatically connecting two huge turntables with living cages in the tables so that no human handling of the rats went on throughout the experiment. It had the disadvantage that once in a while something went wrong and it would cut a rat in two which, as Heron said blandly, "tends to disturb the rest of them a bit." I didn't do any more rat work for a while after I shifted into clinical psychology as Hathaway's assistant and advisee, but shortly after receiving our degrees, MacCorquodale and I embarked on a series of studies using the single unit T-Maze and the Blodgett Maze, Blodgett's classic study of learning being one of the main supports for Tolman's cognitive view of how rats learn. We did some interesting things that I believe contributed something to the movement of that controversy, of which I will mention only a couple of examples. After one failure to get a sizeable Blodgett effect, a difficulty encountered by others in varying degrees, it occurred to us that even when one succeeds in getting the effect (a marked decline in errors and running time following the first goal box feeding for animals who had up to then been running the maze without food as a reinforcer, although goodness knows how many other positive and negative reinforcers may have been operative), Tolman's conclusion did not follow unless one started with a very strong "cognitive map" bias, that what was happening was the rat was putting to use the information contained in his map. That's the most obvious inference to draw and, as I now believe looking back, it's still the best one. I think Tolman was probably right about this, but the point then was that it did not follow directly, all that followed was that <u>some change</u> had taken place due to the feeding experience.

What else could be doing it? Since our previous data in the Blodgett Maze, and other researchers' data, showed that there is some significant decline in errors and running time prior to the introducing of the goal box reinforcement on the pre-critical run night, a Hullian might argue that there was a building up of differential habit strength in favor of the correct choices based upon other reinforcing consequences [rather] than anything that was happening in the goal box. We had previously shown that a moderate amount of free exploration in the maze—the animal is introduced at randomly chosen points, removed at a fixed time but never removed from either the goal or entry box—results in a distinct tendency to stay out of the culs when one does a test run and counts "errors" as usual. We had also found in an unpublished study that if all the culs are blocked by a door indistinguishable from any other portion of wall, and the rat is repeatedly run under 23 hour drive and fed in the goal box, then despite this history of strong reinforcement under high drive, the first night a rat is run with the cul doors removed he doesn't make the standard near-chance score of approximately 3 errors in the 6 unit Blodgett Maze, nor does he run at the high rate he was running for several nights pretest. He averages close to the upper possible number of six errors, he enters every cul and usually to the full extent of his body rather than merely inserting his head. This was evidence that the exploratory tendency—whether or not one called it an exploratory <u>drive</u>—was powerful, capable of competing successfully with a strongly stamped in series of maze choice habits. Considerations like this made the Blodgett design look far more complicated than one at first thinks. It was no longer a straightforward dichotomy between the stamping in of choice-point habits by food reinforcement versus putting to use a cognitive map. Tolman could explain the Blodgett effect by a kind of cognitively [cognitivity?]. But Hull could rather say we have a set of differential habits which before food reward already favored staying out of the culs. The first (in Hull's intervening variable chain, drive acting as a multiplier on habit strength) goal box feeding experience then served, <u>in one way or another</u>, to activate these habits. Instead of saying the rat knows about the maze but doesn't bother to do much about it—only a little bit of cul avoidance in the pre-feeding runs—until he discovers that food is to be had in the goal box at the end of a run, one could instead say that the rat has formed a set of differential habit strengths at the six choice points and that these habits strengths are now multiplied by a bigger motivational factor. In Hull's famous equation of the intervening variables the motivational factor is presumably the only one available that could be readily explained as boosted as a consequence of the feeding experience, although one might also speculate it's doing something about the reactive inhibition component. We opted for an explanation in terms of conditioned drive, a notion based partly on observation in human affairs but also having some independent experimental support in animal behavior. Our idea was that a feeding experience in the general maze context would condition the already present 23 hour hunger drive and heighten it so that the habit strength would now be multiplied by a larger drive factor on the test run. How test this? A feeding experience, not at the end of the run and not in the goal box, but in a maze-like surround in the experimental room ought to do it. So a special waiting or feeding box was placed on a six foot tall standard, and located at the opposite end of the room from the goal box, a few feet anterior to the entry box of the Blodgett Maze. It was necessary to desensitize the animals to this box, by placing the rat in waiting box briefly before each run: so we departed from Blodgett's design. It's one of these cases where you can't do what you want to do without changing the original experiment in what might be an important way. The (weak but non-zero) reinforcement in the goal box consisted of being removed and returned to a cage other than the home cage for a couple of hours. The experimental group showed a slow but clear decline in time and errors as in Blodgett's study, and after a feeding in the special elevated feeding box we got a Blodgett effect, so a feeding "worked" even if it had nothing to do with the goal box as a location. Despite this nice result that might have been disconcerting to Tolman, we continued to have an interest in Tolman's theory and made some efforts at formalizing it in terms of postulates, expressing a low order of quantification, partly just to see if one could do it because one of the stock Hullian criticisms of Tolman was a philosophy of science complaint about the necessity for explicit formulation, operational definition, and the like. Meanwhile, MacCorquodale was drifting increasingly in favor of Skinner, partly because he was teaching a new course in advanced general psychology which he taught as pretty much straight operant behaviorism and, shortly thereafter, gave a more appropriate title "The Analysis of Behavior." Skinner, and so far as I know most of his followers, have never been particularly interested in the latent learning controversy although I am puzzled as to why not, since it would seem that clear-cut latent learning effects speak against Skinner's theory as much as they do against Hull's.

Following a conference at Indiana University (where Fred Skinner was chair), a group of us obtained a grant to spend the summer of 1950 without teaching or other responsibilities examining learning theories at Dartmouth College. Participants were W. K. Estes, S. Koch, K. MacCorquodale, C. G. Mueller, W. N. Schoenfeld, W. S. Verplanck, and myself. The book we produced, *Modern Learning Theory* (1954), was an influential work, and some think it sounded the death knell of Grand Theories in psychology. Its effect on me was marked, as I never published another rat experiment, partly because my colleague MacCorquodale became a Skinner disciple and lost interest in latent learning, but mostly because I became skeptical about the possibility of devising strong experimental tests of theories like Hull's or Tolman's. So many bright people had cooked up designs they hoped would be *experimenta crucis,* but it turned out they never quite were. I began to suspect there was something fishy about psychology and its theories. Unfortunately, my reading in philosophy of science about ad hoc postulates and auxiliary theories was not reassuring in this respect.

Sometime in the late 1940's MacCorquodale and I attended a conference of psychologists at Indiana University where Skinner was now chairman of the Psychology

Department. Somehow, I no longer can reconstruct the details and MacCorquodale is deceased, a small group consisting of William S. Verplanck, William K. Estes, W. N. Schoenfeld, MacCorquodale and I thought it would be worthwhile to arrange a summer conference on the major competing learning theories. To that group were added Schoenfeld's colleague at Columbia, Conrad G. Mueller, and a non-participant in the Indiana meeting, Sigmund Koch. We obtained a grant from the [Carnegie Corporation] and spent a wonderful ten weeks at Dartmouth College in the summer of 1950 with no teaching or administrative responsibilities, in regular meetings of a small group sharing basic methodological views and, to a considerable extent, similar theoretical sympathies. The usual semantic hassles and ideological collisions that occur at conferences among scholars who do not know each other well, may not understand each other's characteristic terminology, and lack the kinds of bufferings you need when you find a certain word is a red flag for another fellow, were avoided. We divvied up the theories and despite MacCorquodale's fairly strong Skinnerian sympathies at that point, he and I elected to "do" Tolman, which was a lot of fun. I have recently had occasion to reread our chapter in the book that emerged from the Dartmouth Conference (1954) and I still think it had a lot of good stuff in it. In addition to our attempt to formalize Tolman along the lines we had tried in previous papers on expectancy theory, an attempt we were pleased to find Tolman regarded highly and disagreed with on only a couple of points, we had a chance to think and write about some other important problems, such as the definition of the response. Our analysis of the Blodgett design was far and away the most thorough that anybody had attempted up to that time.

As a result of the Dartmouth conference, especially our analysis of the latent learning experiments and what they could and couldn't prove, I formed a strong and permanent appreciation of the difficulty of conceiving experimental arrangements which make it possible to exclude or, lacking that, to assign quantitative values to, the influence of various nuisance factors that are inextricably connected with the variables you are manipulating. When the philosopher of science Imre Lakatos visited the Minnesota Center and we began reading his amendments to Sir Karl Popper's falsificationism, I found it easy to go along with most of what he had to say. In pursuing the latent learning controversy, the more you thought about it and scrutinized critically the experiments done by either Tolmanites or Hullians, each purporting to be a kind of <u>Experimentum Crucis</u>, it was always possible without much finagling and without any intellectual dishonesty, to come up with plausible <u>ad hoc</u> objections regarding the extent to which the theoretical auxiliaries, and even more the highly problematic <u>ceteris paribus</u> clause, were acceptable. It was difficult to avoid the feeling that it was <u>just too hard to unscramble things</u>. Nevertheless, in the chapter we wrote for the Dartmouth conference book our conclusion was more sympathetic to Tolman than either of us would have anticipated before the conference. Whether that chapter had any impact on the revival of an unembarrassed interest in cognitive psychology in both humans and animals I have no way of knowing, but my impression is that it had rather little effect as judged by the scanty citations of either the chapter or our early papers on expectancy theory.

One traumatic event marred the time at Dartmouth and, in its long-term effects, had an adverse effect on my professional career: walking along a ledge above a stream at a place called the Flume in New Hampshire, I had a grand mal seizure. If MacCorquodale had not turned around and noticed me convulsing and pulled me back from the edge, I would not have survived. I had no history of seizures even as a small child and no epilepsy in my family. My EEG was definitely abnormal, with a focus in the right parietal area (the few seizures I had subsequently were definitely Jacksonian, beginning with a tingling numbness and twitching in the fingers in the left hand plus some nystagmus). I had an anomalous blood sugar curve and there was diabetes in my family, so the neurologist concluded that the seizure arose from a hypoglycemic influence on a focal brain lesion. I did not go on any medication at that time, and did not have another seizure for five years.

A more thorough neurological study showed only a mildly anomalous glucose tolerance curve, and the focal EEG convinced Abe Baker, the head of our Neurology Department, that while I should avoid carbohydrate breakfasts, that was not the main problem. He put me on Dilantin, which controlled the seizures, but despite some clinical claims that Dilantin has negligible psychological side effects (although it can make your gums bleed), a perceptive psychiatrist colleague said that VA patients with brain injuries who were on Dilantin for long periods of time did suffer a definite side effect, a kind of dulling of affect and loss of energy or zest, though they did not become depressed. He told me that frequently the first indication of this long-term slow effect of Dilantin was observed not by the patient himself but by the wife, who would notice that he had "lost interest" in his usual hobbies of fly tying, playing golf, and the like. I am convinced that in the seven years (1955–61) when I was on Dilantin I had a definite lowering of hedonic tone and motivational level. A trial of going off Dilantin during that period resulted in another grand mal seizure, and then I had one during sleep, inferred from the fact that my tongue was badly chewed up in the morning. The best etiologic bet of the neurologists was a small brain scar attributable to the rheumatic fever I had had at age 22.

In 1962 my physician took me off Dilantin and put me on a new anticonvulsant which, as we subsequently learned, produces depression in a sizable minority of patients. It had that effect in my case, which was hard to put up with because it was the year I was president of the APA and had to write a presidential address, preside at meetings, deal with correspondence, etc. I called my former psychoanalyst (now at Hartford) who looked into the matter and recommended taking me off the new drug, whereupon my depression lifted in a couple of weeks. A depression on becoming APA president might exemplify Freud's "those wrecked by success," but since its onset was a year after my election and three weeks after the new drug, I incline to the pharmacologic interpretation. I now take an anticonvulsant (Cytadren) which has no side effects and has controlled the seizures for 30 years. This personal experience has given me more awareness of the problem of pharmacologic side effects than some clinicians have, especially the danger of believing negative statements arising from the fact that patients have not been observed for a long enough time period, or that minor signs of change have taken place so slowly that neither the patient nor any professional notices.

There was another adverse effect of the Dartmouth conference of a more subtle character, which I did not become aware of until my analysis in the middle 1950's. Despite my many hours of contact, in class and at his home, with the great B. F. Skinner, clearly the most significant contributor to our science among the persons with whom I had contact as a student as either teachers or peers, for some reason Skinner's theoretical "system" (from a philosophy of science standpoint he of course has a theoretical system whether he calls it that or not) never mobilized my cognitive passions. This lack of enthusiasm for something that clearly has truth in it so far as it goes, and which has no competitors among theories as regards its technological power, cannot be attributed to any distaste for behaviorism (I was for quite awhile enthusiastic about both Tolman and Hull), nor to my psychoanalytic interests, since I was quite comfortable putting those in another cognitive box. I didn't feel the urgency that some psychologists seem to feel to carry out a conceptual reduction from one level to another immediately. Two theories that one might view as far apart as you could get that were discussed at Dartmouth, Skinner's and Lewin's, both failed to interest me intensely, although I had more scientific respect for Skinner's than for Lewin's. There is something about the kind of learning theory, the relationship between its theoretical concepts and the observational data alleged to support them, that made both Hull and Tolman interesting to me. I have never figured out just why this was. The Dartmouth Conference conversations, the devastating critique of Hull in that long, superb, brilliant chapter by Sig Koch, and reflection on our own chapter on Tolman as it tried to ferret out the hidden assumptions and complexities about auxiliaries and ceteris paribus clauses in what purported to be strong experimental tests of competing theories—all of this led me to think, putting it crudely, "scientific learning theories ain't all they're cracked up to be." I became somewhat pessimistic about the enterprise. I am not prepared to deny that the death threat of the seizure was without effect, but exploring this possibility during my analysis did not have fruitful results. MacCorquodale and I conducted only three rat experiments following our return from Dartmouth, none of which we published. I do not regret having once been a rat runner or spending time theorizing about Hull and Tolman, and I like to think that along with the hundreds of studies on latent learning that were done during that controversial period, we did contribute something to the field's advancement. It is widely held that whatever else its merits, the Dartmouth volume influenced many young (and a few older) psychologists, and was partly responsible for what is sometimes referred to as the "death of the grand theories of learning," and I believe that in itself was a worthwhile scholarly contribution. Even today from time to time, especially in discussing "clinical" observations on our two cats with my wife (Ph.D. Berscheid and MacCorquodale) [second wife; first died of cancer], one of us will cook up an experimental design to test a hypothesis about "what is learned" or even "just what it is that they know," but I have never been turned on enough to go back into the animal laboratory with any of these experimental ideas.

I cannot recall exactly when I became interested in the problem of clinical versus statistical prediction, but it was at least a decade before the publication (1954) of the little book that made me somewhat famous (perhaps I should better say, at least in clinical circles, "infamous"). I was lecturing briefly on the topic in 1944, and Arthur H. Brayfield, auditing the course, called my attention to T. R. Sarbin's classic paper (1942), which was in a sociology journal and hence unknown to me. I believe Paterson, in his individual differences class, mentioned a controversy in the 1920's between the industrial psychologists Max Freyd and Morris Viteles. Gordon Allport's monograph on personal documents appeared in 1942, and I read that monograph shortly after its appearance. It is easy to understand why someone with my psychological history should be fascinated by this question. Having undergone an intense bibliotherapeutic experience from reading Menninger, I had then studied under faculty who were skeptical about psychodynamic theories, especially those arising from clinical experience rather than from the experimental laboratory or statistical studies of clinic file data. This skepticism, which to more freewheeling psychologists appears as negativism, reflected a *methodological* more than a *substantive* stance. Paterson and Hathaway may have had an intellectual distaste for the content of Freudian ideas (including some based on personal resistances), but the main thrust of their complaint was not substantive, rather it was the lack of a trustworthy method for *testing* such conjectures from the evidentiary base provided by the psychoanalytic hour. Skinner was somewhat different in this regard, as his main objection was to the kind of concept invoked in psychodynamic explanations, and was more philosophical than it was a matter of statistics or the dangers of free wheeling inferences. For him, what was wrong with the doctrine was that it invoked inferred entities, and with properties duplicating the explananda at that. No bright, reflective, theory-oriented student, coming to psychology from an interest in psychodynamics and exposed to this environment of first rate minds who gave it little credence—and not for silly reasons—could fail to experience intense cognitive dissonance and a strong, persistent need to resolve it.

I reread my 1954 book recently and am still of the opinion that it was an even-handed treatment, which is what most—not all!—of the reviewers said, whether they were primarily identified with the clinical or the statistical approach to prediction. It was easy for me to be relatively fair-minded about this charged topic, as I had strong identifications on both "sides." If you combine that with my interest in statistics and my epistemological interest continuing from our little group of teenage logicians, and add my exposure to some of the ablest intellects pursuing philosophy of science, you have a setup for writing a pretty good book. The first chapter states that I became fascinated by this problem at the 1947 meeting of the American Psychological Association where E. Lowell Kelly presided at a symposium on clinical and statistical methods at a joint meeting of the clinical section of APA and the Psychometric Society. While it may be that that's where I became fascinated, it certainly is not the time that I first became interested enough to include a discussion of this topic in my lectures. But it was in 1947 that I began to think of writing an article on the subject, because it seemed obvious to me that despite the able participants in that symposium, they were largely talking past one another, and that neither the philosophical nor the mathematical issues were treated in depth and incisiveness. I found myself choosing this topic as a subject for lectures delivered at local VA installations and invited, colloquia at the stage of career when I was accepting such invitations from universities here and there. It must have been shortly after that 1947 APA meeting that I began to write a draft on various aspects of the controversy, and by 1950 I had written and revised enough material that it was too long for an journal article, but somewhat on the short side for a book. I continued to make some revisions here and there as a result of further reflection or conversation with people on both sides of the issue, but it was substantially complete by 1950. The lapse of four years before publication was partly due to my taking over the chairmanship in 1951 which cut back somewhat on my revising the MS, but the main difficulty was my inability to find a publisher.

In fact, I had trouble finding a publisher, and when Margaret Harding, director of the University of Minnesota Press, took it (as a favor to Psychology's chairman), she expected to lose money on it. When the book went out of print in 1973, it had gone through seven printings and sold 12,500 copies. I do not recall exactly how many publishers I approached, but two with whom I had special leverage, The Century Series (editors Elliott and MacCorquodale) and Prentice Hall (I was one of their editors), turned me down on the grounds that it was not of general interest, it could not be used as a textbook, and it would not sell. Finally, I approached the University of Minnesota Press which had, as do many university presses, a policy of publishing good quality works that they didn't expect to sell well, and a willingness to give special breaks to Minnesota faculty. The then director of the press, Margaret Harding, thought it wouldn't sell well so in order not to lose too much money the Press obtained a special grant of money from the President's office to cover printing it as a favor to the Psychology chairman. As of this writing, the book has gone through seven printings, which I suppose testifies to the fallibility of human judgments in such matters, and to the scholarly value of university presses which hope to make enough money on some of their books to subsidize others.

The reviews were uniformly favorable and some were enthusiastic, even "rave" reviews. Even those who didn't like the overall "message" said that I had *tried* hard to be evenhanded but hadn't quite succeeded. Both clinicians and anticlinicians reacted to it as a projective technique. The subtitle "A Theoretical Analysis and Review of the

Evidence" shows what I was up to. Only one chapter dealt with empirical comparisons, and I did not view that chapter as the most important part of the book. Many more pages are devoted to defending the unique inferential activity of the clinician than to criticizing his predictive deficiencies. I had spent much time reflecting on clinical inference, especially during psychotherapeutic sessions, trying to get clear about just *where* the unique cognitive activities of the clinician took place and *why* it would be difficult to teach a clinically inexperienced "clerk," as I provocatively labeled the actuarial competitor, to do the same things.

The profession's reactions to this book, while I can hardly complain about their contribution to my becoming a highly visible psychologist, gave me my first real insight into the extent to which social scientists read superficially and carelessly. Perhaps this is because so much written in the "soft" areas is not conceptually precise, deep, or methodologically sophisticated, so that one gets into the habit of reading carelessly because it usually doesn't do you any harm! Some more insightful psychologists have asked whether, in addition to the frustration at having written (as I thought) clearly and making some important distinctions, being dealt with in a sloppy and superficial manner (I include here some people who liked what they <u>thought</u> I said as much as people who didn't like it), was I not also frustrated by the fact that despite its visibility and many citations, it had so slight an effect upon clinical practice, and still has had a slight effect after thirty years? To this I think I can answer honestly that I did not find that frustrating because it is precisely what I expected.

An indirect derivative of that book was the "cookbooks" for MMPI interpretation discussed above, the fusion of computerization as a technology with the actuarial approach to making inferences from tests. I think motivations for resistance to its implications for a rational clinical practice are almost insurmountable. The subject no longer exercises the fascination it did for me as a young man, partly because the accumulation of the research evidence is so overwhelmingly on the actuarial side of the debate that reading it becomes rather boring, as one knows in advance how it will come out. Either the clinician will be about equal to the mechanical prediction formula or table, or (in around a third of the studies) he will be inferior. I do not see much point in showing that over and over again, since the studies currently available (over 100 in number) have shown it about clinicians of varying degrees of experience, with varying degrees of feedback opportunity to correct their errors, with various combinations of input information, making predictions over a qualitatively diverse domain of predictive tasks. Those who still resist the generalization that the human mind is not very good at this kind of thing now have the burden of proof to come up with clear and replicable studies showing the exceptions (Meehl, 1987).

In the years following publication of that book, I myself wrote some papers listing a half-dozen factors about the predictive task, subject matter, kind of data, etc., that might make the clinician superior in his success rate or, better, make a qualitative difference where the clinician would be able to come up with a prediction and there would be no actuarial method of *doing* so, accurate or otherwise. My own efforts at finding empirical examples of this superiority were confined to one of my predictively pro-clinical factors, namely, configural effects in multivariate profiles. Having MMPI protocols and MMPI experts available to me, I pursued that one, the diagnostic decision being the dichotomy between psychosis and neurosis, which is both theoretically interesting and of practical importance. It seemed a good bet for the study of configural effects in profile interpretation because one kind of psychotic patient has a different profile pattern from another kind of psychotic patient, so it seemed likely that a non-configural approach, such as a linear discriminant function of thirteen MMPI scales, would not capture the configural effects. Perhaps I suffered from some reaction formations, or perhaps a bit of defensiveness toward those clinicians who thought I was out to "beat up the clinician." I was hoping to find that the skilled clinical eye could discern features of the profile pattern that the statistician could not unless he went into configural effects—pairwise (Meehl, 1950c) and even perhaps higher order scale interactions.

This pro-clinical bias led to the only paper I've published in which the finding is literally incorrect, not merely not replicable but incorrect on my own data (Meehl, 1959b). I have a lame excuse in that the discriminant function job was done not by my research assistant but by one working for my colleague David T. Lykken, who had the same bias because he was interested in showing the superiority of an actuarial method that he had devised for profile interpretation. So when it turned out that the linear composite of MMPI scales did very poorly, he was willing to accept that result without careful scrutiny of the data, and so was I. It was foolish of both of us, for our different reasons, to trust a finding that showed a linear combination of scores doing as poorly as it did. Subsequently Lew Goldberg showed that even a nonoptimally weighted linear composite on that same set of data did as well as the more complicated configural rules Dahlstrom and I had developed (Meehl and Dahlstrom, 1960; Meehl, 1960a) or Lykken's "function-free actuarial box" method (Lykken, 1956). It was obvious that we were relying on a computational mistake, I cannot recall the details, but it came about from a transformation into octals for the computer, done under time pressure by a bevy of undergraduate research assistants. We should have known better.

Arguably I ought to spend more time propagandizing for the actuarial approach to clinical decisions, since the evidence is so massive and consistent. There never was any good reason to think that the clinician could do as well as an equation, unless one believes that the human mind is a good assigner of weights and consistent (reliable) applier of such weights. There are three kinds of jobs that computers still cannot do very well in comparison with the human brain: pattern recognition, language translation, and theory construction. To the extent that *some* clinical inferences have the same kind of cognitive character as these activities, we can expect the brain to do better than a computer. But almost the only such example is psychoanalytic inference from complex data, such as the analyst's knowledge of the patient's life history and previous interpretations, put together with the manifest content of a dream and the patient's free associations to it. Whereas if one is trying to forecast whether a subject will respond to one antidepressant rather than another, or will be a premature terminator of therapy in a VA clinic, or is a likely recidivist if paroled, or is a suicide risk, or is a better bet for Rational Emotive Therapy than behavior modification, or will survive in dental school, or will be washed out in flight training in the air corps—these kinds of predictions, for reasons that I set forth in 1954, are simply not predictive tasks which we should expect to be done well by an individual clinician or by a team meeting or case conference. There is a tremendous waste, involving patients' or taxpayers' dollars, as well as the human waste involved in predicting less efficiently than is mathematically possible, in current clinical practice whether in the mental health, criminal justice, or educational systems.

Example: I heard a radio program in which the Deans and faculty of a posh eastern private college described how many hours they spend reading letters of recommendation and staff conferences in which they discuss which students should be admitted to the freshman class. This kind of task would be more efficiently done by actuarial methods, and that does not depend upon which criterion of "success" as a student one wishes to emphasize. I estimate that by saving N hours of faculty and administrative time combining predictive data inefficiently ("conversations in a smoke filled room") one could reduce the fees paid by these entering freshman by fifty percent. In the criminal justice system, court services social workers provide fat narrative dossiers for the delectation of the sentencing judge, who flips through the pages superficially in most cases—I have watched these judges in action and in some instances it seemed quite apparent from watching the judge that he has hardly looked at it before. In connection with a prisoner class action where I served as an expert, I reviewed two dozen of these dossiers, and since I was being paid per hour I kept track of my time. A little arithmetic shows that if a Minnesota district judge carefully read, and reflected even a little bit, on these documents he would hardly have time to do anything else, so they do not. This is fortunate, since the evidence suggests that if the judge did study and rely on these presentence investigation reports his predictive efficiency in sentencing would decline. (The importance of his predictive accuracy is slight, since Minnesota has a system essentially actuarial in character, adopted partly under the influence of a distinguished faculty criminologist who was greatly influenced by reading my book, so there I did have some effect!) As is well known, when recidivism, parole violation, or first offense by individuals at risk is the predicand, the same half dozen predictive variables show up again and again in different studies, done in different countries. They consist essentially of the track record, e.g., how many previous offenses? Age at first offense? When dropped out of school? How long employed in the same job in the private sector? Chemical dependency? and (maybe) characters of the self-selected peer group. Hardly any psychometric instrument except the Q-score of the Porteus Maze has appreciable validity, although the MMPI has some, and when interpreted actuarially is not costly. I don't know how many millions of dollars are wasted annually in the preparation of these nonpredictive documents, but I am not about to waste any time or energy trying to convince the responsible officials to abandon them, a politically undoable task.

Assuming some movement toward scientific rationality in these matters, about which I have no confidence, I would think that the diagnostic and prognostic functions of clinicians someday will consist largely in the clinician's role as elicitor of information in the interview. Even the non-optimally weighted criteria for the diagnostic rubrics in DSM-III is essentially a move in the actuarial direction by psychiatry, whether the psychiatrists involved in that enterprise had ever read my book or not, which some of them had and most had not. I think that further moves on a large scale in the actuarial direction in psychopathology will not occur until the comparative evidence accumulates in organic medicine, which it is presently doing.

Clinical psychologists often say that it can't be right to diagnose and prognose actuarially because (nonpsychiatric) physicians haven't been doing it all these years, an argument which is worthless absent a showing that physicians do it better than an equation or table. Some psychologists seem unaware that both the interphysician reliability and the validity as shown by autopsy of diagnoses in organic medicine leaves much to be desired. I still hold to my original conception (Meehl, 1954, pp. 24-25 and references to the "broken leg case" in subsequent papers) that even a complex, souped up, multiply cross-validated actuarial method would make us slightly uneasy without some clinician available to take a look at the prediction with an eye to the possibility of a broken leg case. But I insist that this will not pay off unless the "last chance" clinician is highly sophisticated about the clinical actuarial problem. He has to know that true broken leg cases in psychopathology are rare, *so* rare that his departures from the actuarial prediction should be held down to a low rate, and if they increase appreciably, the long-term result will be a decrease in predictive efficiency. I am not optimistic about educating clinicians to think this way for mathematical and philosophical reasons, but the rising costs of health care may bring about a pragmatic movement, not explicitly principled, in that direction.

In the middle 1950's the Ford Foundation solicited psychologists in the social science domain to submit large grant proposals, and a group of us Minnesotans received a grant to study "the skilled clinician's description of personality, with emphasis on developing an adequate language." I was named the principal investigator, the other members of the team being Starke Hathaway, Donald Hastings (head of our Psychiatry Department), William Schofield, Bernard C. Glueck (my former analyst and analytic supervisor), and research assistant Walter B. Studdiford. Subsequently, the statistician and computer specialist Dean J. Clyde was added to the group. In the 1950's many clinicians and social psychologists were infatuated with Q-technique as an approach to the study of personality, and I must confess that this is one of those rare cases in which I fell for a fad. Only brief accounts of the project have been published (Glueck, Meehl, Schofield, and Clyde, 1964; Meehl et al., 1962; Meehl, Lykken, Schofield, and Tellegen, 1971), but I will cover it briefly because we still anticipate publishing at length.

We were troubled by the extent to which the items appearing in structured personality inventories and rating scales were drawn from a traditional and rather narrowly focused list of traits or behaviors thought to be relevant in psychopathology. Since the success of the MMPI and the SVIB were partly attributable to their deliberately diversified item content, we began by constructing an item pool as free as possible of these traditional restrictions. We did include item content from numerous rating scales in clinical use that had appeared in the literature, plus a provisional phenotypic and genotypic pool on which I had done some research (largely unpublished, but see Meehl, 1960a, p. 131). We also scanned the famous Allport-Odbert list of trait names; our group discussion eliminated, on an armchair basis of multiple criteria, most of those trait names, paying attention to Raymond B. Cattell's earlier screening of that list. We thought that even using ordinary human trait names as a source of item content was culturally stereotyped. For example, it is known that there are many more trait names in the dictionary mentioning undesirable human attributes than desirable ones. So we proceeded by what turned out to be a time-consuming and costly process that didn't yield as much as we had hoped. We gave both clinicians and intelligent, educated but not clinically trained people (e.g., professors of literature) brief episodes of randomly sampled speech or conduct from a variety of sources such as recorded interviews, social-work case histories, modern and Victorian novels, and even a random sample of episodes from the Bible. These readers were asked to write (or dictate) short paragraphs "characterizing" the sort of person who would do such-and-such and to assign a phrase or composite or disjunctive trait name. The team members were urged to concoct items from our clinical experience that could be sentences or short paragraphs for which there was no standard common language or psychiatric term available.

The initial 1,808-item pool in the Ford Project was a so-called phenotypic pool, not in the geneticist's sense, but in the sense that while it was not strictly behavior items, it was intended to be descriptive of traits summarizing first-order behavior dispositions with a minimum of theoretical inference. First, 586 items were eliminated when too many psychotherapists (after 25 interviews) said they could not make a judgment on the items because of insufficient data from the interviews. One surprising finding was how many items that dealt with rather simple and obvious aspects of the patients' behavior therapists claim to have heard nothing about. Although we did no formal statistical analysis, we were surprised that psychotherapists often learn amazingly little about overt features of the patients' sexual behavior. The lay stereotype that "shrinks like to make you talk about sex" does not seem to be true, even for psychotherapists in the broadly psychodynamic or Freudian tradition. Considerations of reliability, a crude measure of therapist effect versus true differences among patients, and an initial factor analysis combined with examination of quasiredundant content resulted in elimination of items down to a final set of 329. Factor analysis of the final pool of phenotypic items yielded 40 factors.

Unfortunately, for a variety of reasons not connected with the project, the research team dispersed geographically. Dr. Glueck, who had taken over as principal investigator when the Ford grant ran out and the project continued under NIMH support, made practical applications of the individual patients' factor profile at the Hartford Institute of Living. Starting with our results, he constructed several subpools (such as the doctor's subdeck and the nurse's subdeck), and for a period of time when he was research director at the Hartford Institute of Living, what had been rechristened the "Minnesota Hartford Personality Assay" was in routine use on the wards and in connection with research such as comparative efficacy of psychotropic drugs.

We had also constructed a genotypic pool consisting of the Murray needs and the twenty mechanisms of defense. A configural task assigned to our therapist raters was to identify the most salient Murray needs, together with the patient's preferred mechanism of defense, in turn linking this to the *salient objects* (spouse, country, or whatever). Those genotypic data have never been analyzed although they are on computer tape and as of this writing I am trying to find out whether the material is retrievable for research purposes, as there was a grave error made by someone years ago in discarding identifying information. Whatever else may be claimed for it, I think I can say that the Minnesota Hartford Personality Assay is one of the most carefully constructed sets of personal descriptors available. Despite the "unjudgeability" by therapists of items eliminated from the final MHPA instrument, the second-stage set (m = 1,222 items) was constructed with such loving care for content diversity and niceties of language that it provides a superb item source for research purposes. We were therefore surprised and disappointed when it found negligible use by clinicians and personologists.

A spinoff from the Ford Project was a theory of schizotypy as a personality syndrome, socially learned on the basis of a hereditary neurological disorder ("schizotaxia") presented in my APA presidential address (Meehl, 1962b). Today this conception is almost trite among informed persons, but it was a radical (and unacceptable) doctrine in psychological circles a quarter century ago. I am currently working on a revised formulation, but see Meehl (1972c) and Gottesman and Shields (1982). I contributed numerous "novel" schizotypal items to the Ford Pool, based on my clinical experience and the literature, and developed a schizotypal checklist for detection of the Hoch-Polatin syndrome (Meehl, 1964). Scores of clinical researchers and training directors have requested copies of the manual, but for some reason very little use of it has ever surfaced in the literature. Another spinoff of the project was a method for reducing the subjective element in interpreting psychometric factors, the "recaptured-item technique (RIT)" (Meehl, Lykken, Schofield, and Tellegen, 1971).

[Initial dictation of schizotypy material:]

At that time, as a result of Glueck's emphasis on Rado's theory of schizotypy, I became interested in the problem of identifying semi-compensated or decompensated but nonpsychotic schizotypes in my own clinical practice. I constructed a schizotypal checklist (Meehl, 1964) and using a priori weights generated a distribution of scores that matched almost perfectly my global dichotomous assessment of schizotypy. I did not think it appropriate to publish these "one-clinician statistics," since the obvious complaint of contamination would be properly asserted. It was nevertheless reassuring to find that Meehl's checklist, at least as filled out by Meehl, was capable of giving a substantially perfect dichotomization of the global judgment. I believed it supported the concept of schizotypy that even though only a few of these parents had ever been formally or subsequently diagnosed as florid schizophrenia, and only a few more had the touchstone indicator of a definite micropsychotic episode, the characteristic 'gull wing' profile that MMPI experts have come to associate with pseudoneurotic schizophrenia and with borderline conditions was in evidence. From my reading of Hoch and Polatin, Rado and other writers on borderline conditions, from my own clinical experience, and from extensive conversations with the members of the team, I concocted many items for our phenotypic pool, not in the literature, that I think relevant to identifying the schizotype.

Although my 1962 A.P.A. presidential address on schizotypy is fairly often cited, I have found it puzzling that clinicians and geneticists resist the notion of treating schizotypy, or its neurological substrate (which I christened 'schizotaxia') as a dispositional type rather than some sort of "attenuated schizophrenia." I advocate a research strategy based upon a causal and statistical model analogous to what one finds in many other nonbehavioral diseases, such as gout. Those who have read my writings on schizotypy misunderstand them more often than not (although there are some encouraging exceptions) because they persist in thinking not only of pseudoneurotic schizophrenia but also the compensated schizotype as somehow "on the spectrum," which turns out to mean "having the symptoms of schizophrenia but only an itty bitty of each," so that the Rado–Meehl schizotype is a kind of watered down case of schizophrenia. Now, I don't believe in trying to tell other people how to think, since I am resistant when people tell me how I have to think about subjects that interest me. But it does bother me when readers assimilate my theory to the conventional wisdom about the schizophrenic spectrum. The kind of causal model I have in mind (and the associated taxometric approach appropriate to it) does not conceive the schizotype as somebody who is just a itty, bitty schizophrenic. Rather I think of it as closer to something like gout. The great majority of persons having the gout genotype (as inferred from their clinically gouty relatives plus their own elevated uric acid titer) do not develop clinical gout. Similarly, Rado and I hold that the great majority of schizotypes never fall ill with a diagnosable schizophrenia, even a "mild" case of it. The point is that a mild case or a forme fruste is still schizophrenia, just as a mild case of gout is still gout. But schizotypy is not schizophrenia. When somebody has the gout genotype but by some combination of genetic modifiers and environmental factors (e.g., diet, as in the folklore) does not develop clinical gout, we think of him as a gout-prone individual, and we document that by reference to the elevated uric acid titer in the blood. We do not mean by a gout prone individual, however, that he must show in his big toe at least a little bit of the medieval doctors' inflammatory tetrad (turgor, dolor, calor, rubor). He may be 100% free of even a small amount of any of these four symptoms of inflammation in the toe joint. This kind of thinking, which is taken for granted in non-psychiatric medical genetics, seems hard for behavior geneticists to adopt. In a case of schizotaxia, it may be that the only perfect indicator of the schizoid genotype (which I am still betting is a major locus dominant) is some endotypic feature of CNS function. One may be hopeful about detecting subtle forms of cognitive slippage, short of Blueler's clinical "loosening of associations," but one need not insist upon that as part of the definition. My prediction is that only soft neurology and psychophysiology, and perhaps a few pleiotropic effects such as the capillary nail fold anomaly, will have high marker value for the schizogene, if such exists. I have been asked in correspondence and by students why I invented the neologism 'schizotaxia' when in my 1962 address I specifically stated that all schizotaxics become schizotypic, hence I only need one word. This is a blooper stemming from too much operationalism, and the erroneous practice in classical psychometrics of saying that if two measures correlate perfectly after attenuation correction, both tests must be measuring "the same thing." In psychometrics maybe they do, more often than not, but there is no guarantee of that conceptual identity from the statistical relationship. We do not say, because we accept Charles' Law, that volume and temperature must be the same concept, so that we can dispense with one of the two words! Or to take W. V. Quine's favorite example, from the fact that all organisms with a heart have a kidney, one does not conclude the terms 'heart' and 'kidney' have the same designatum, so one of them can be dropped from the biologists' vocabulary. 'Schizotaxia' designates a conjectured neural integrative defect for which there may be only fallible exophenotypic indicators, even in the area of soft neurology and psychophysiology, although it begins to look that there may be a few that are pretty powerful, such as the eye tracking anomaly or ± dysdiadochokinesia—which I learned from J. C. McKinley (in 1943!) is often found in schizophrenics.

Whether the main results and spinoffs have warranted the Ford grant money and brain time expended I do not know, but I am inclined to doubt it. A possible exception may be my work on developing new taxometric methods, which has been my main research preoccupation in recent years (Golden and Meehl, 1978; Meehl, 1965b 1973b 1979, 1986b; Meehl and Golden, 1982). I consider taxometrics potentially as important as the dimensional statistics of classical psychometrics (e.g., multiple factor analysis, regression theory, and multidimensional scaling). I do not share the prejudice of American psychologists against types, taxa, and disease entities. "No types, only dimensions" was one of D. G. Paterson's favorite principles, and within the "normal" range of individual differences, it is doubtless valid as a strong best bet. But the dogma that *every* class name is merely a crude way of denoting regions in a dimensional hyperspace is not safe in the domain of psychopathology. My approach to the taxometric search problem is heterodox, as I am skeptical of cluster methods, uninterested in the usual Fisherian issues (M.L.E.?), and instead favor emphasis on numerical agreement among non-redundant estimates of the *sample* latent values ("consistency tests"). My efforts in this area have been hampered by my inadequate mathematical education, although it is better than 90 percent of psychologists and 99 percent of clinicians! There's a moral there somewhere.

My first psychotherapy patient (1942–44) was a severe obsessive-compulsive who I now think may have been schizotypal. He had a morbid fear of damaging his brain, whether by rapid or sudden motion, minor shocks, poor diet, "overwork," or emotional excitement. An ex-physics major of high IQ, he had quit college because his phobic avoidance of protracted study (brain fatigue!) led to poor grades. Orgasms being intense, he avoided sexual activity, including masturbation. He once walked up twelve stories for a dental appointment, lest the elevator acceleration damage his brain. He exemplified the fact that a severe neurosis can be more incapacitating than some psychotic conditions.

I initially treated him, doubtless unskillfully, by a mix of Rogersian and psychodynamic therapy, with no results. He had at age twelve killed a boy "accidentally" by shooting him in the head, an event whose thematic relation to the brain obsession he easily accepted with the usual lack of affect. Hathaway suggested that since he was so hypercathected on intellect and could relate to me on that basis, that was the only leverage I had, so why not use it somehow? We embarked on a series of philosophical discussions in which I challenged his complicated theories about the neurophysiology of "pure" versus "derived" pleasure and repeatedly demonstrated that, on his own premises, he was depriving himself of net pleasure more than cumulative minimal brain damage would. He was ingenious and resourceful in argument, but so was I. We enjoyed our conversations immensely. His emphasis on intellect and his need for me to perceive him as internally consistent and rational within his own premises slowly moved him into doubting the long-term rationality of his constricted way of life. I then shifted to systematic desensitization (pre-Wolpe!) and accompanied him on walks and automobile rides. He became 90 percent "cured" of the symptoms, returned to college, became a high school physics teacher, married, and twenty years later was symptom-free and functioning effectively and contentedly.

This rewarding experience as a healer using cognitive and behavioral methods contributed to my later open-mindedness to Joseph Wolpe, Albert Ellis, Aaron Beck, and the operant behavior modifiers. But at the time I remained psychodynamically oriented. I had 85 couch hours with a Vienna-trained analyst (H. S. Lippman, M.D.) and later 300 with B. C. Glueck, M.D., trained at the Columbia Psychoanalytic Clinic under Sandor Rado's aegis. With Glueck I did a couple of controls and a continuous case seminar with three psychiatrists. For several years I practiced fairly classically, enjoyed the work, and I believe benefited some of my patients. But I could not help noticing that my rare departures from classical technique were often effective, and after some contacts with Albert Ellis I increased their frequency. I was also puzzled by the rather low correlation between interpretative closure and therapeutic results. At present I would have to call myself "eclectic," although I dislike the term, because it often means pure seat-of-the-pants therapy with no attempt at theoretical integration. I still have a couch in my office and from time to time put a client on it, imposing the Fundamental Rule. Otherwise I am quite "active" (although less so than Ellis) and employ several interview tactics, including information-giving (e.g., learning theory, sex differences, primate ethology, genetics). At times I even encourage "intellectualizing" discussion of ethics, politics, and other cognitive frameworks bearing on the client's lifestyle. If asked by colleagues or sophisticated prospective patients to label my approach, I sometimes say "mixed rational-emotive and psychoanalytic." As would be expected from my Menninger experience, "understanding how the mind works" is an important element in my psychotherapeutic interest, and in this respect the work is often frustrating. I don't think we understand neurosis or its treatment well in any scientific sense, and I have not found reading the process research on psychotherapy illuminating.

Psychotherapeutic practice (1st edit of raw transcription)

During my first year as Hathaway's assistant I had an experience as a psychotherapist that influenced me more than I realized, and contributed to my openness to cognitive and behavioral approaches despite the fact that my main interest on the therapy side was psychoanalytic although I had not as yet had an analysis myself. The impact of Rogers' classic 1942 book was considerable on many Minnesota students, myself included, despite the fact that Paterson and Hathaway were not enthusiastic about it. I had as an undergraduate done some informal "personal counseling" with acquaintances, of course without fee, and not so labeled. My first real psychotherapy case was not a University Hospital patient but a college dropout who came to me because we had known each other slightly in high school. In those days there were no community health centers and the facilities for gratis treatment were inadequate. He could not use the university health service or counseling bureau because he was not currently enrolled, having dropped out as a result of his severe neurosis. Since my personal acquaintance with him had been minimal and some six years back, Dr. Hathaway and I agreed it would be all right for me to see him without fee. It was leaving school that led him to seek help although he had other severe neurotic problems that he did not fully appreciate as such, although he recognized that his behavior was different from other people. I shall not go into the fascinating details of his symptomatology and psychodynamics: In brief, he had an obsessive idea, with phobic and compulsive behavior consequences, that he would somehow damage his brain by some kind of physical or emotional "violence." He was a person of high IQ, certainly Ph.D. caliber, and was majoring in physics. He had concluded that intensive study of difficult material might damage the brain, as a result of which he was getting unsatisfactory grades in his major and was advised to drop out until he "got his problem straightened out" rather than to continue accumulating a poor record on his transcript. This man was an interesting example of how a severe neurosis can be more incapacitating for life, in all sectors, than a psychosis of the right kind. Some examples: He always drove his car from campus to home by exactly the same complicated route, having memorized where a street had bumps or hollows, and either avoided them or slowed down to the irritation of other drivers. He was afraid of driving on any unfamiliar roadway and if something forced him to do so he would drive at an illegally slow speed. He had been a good tennis player but had to quit playing tennis because obviously that leaping about on a concrete tennis court was very shocking to the brain. He didn't associate with women because he thought that sexual excitement might have that effect, and he had almost totally abandoned masturbation because obviously an orgasm, however induced, was a big shock to the brain cells. On one occasion when he went to visit a physician about a physical problem he walked up twelve flights of stairs because he was convinced that the accelerated motion of an elevator was brain damaging. He also had some special dietary prescriptions that complicated his life, and he was having trouble living at home because if somebody in the family slammed a door a little hard he would have a temper tantrum because he thought the sound waves would harm his brain. He said there were two reasons he came to see me, other than the fact he couldn't go to anybody that would charge money: First, he had heard I was an expert in the psychometric detection of organic brain damage (not strictly correct, although at that time there was a good deal of interest in that among all of Hathaway's students); secondly, he didn't think that anybody could be of help to him unless their IQ was higher than his, which he though mine probably was.

I was so clinically inexperienced that I was confident of a psychoneurotic diagnosis obsessive compulsive (with attendant phobic features) and it did not occur to me that he might be a latent schizophrenia. Rado's writings on the schizotype had not appeared, Bleuler's great 1911 classic was not Englished until 1950. He was exempted from military service because at the induction station he refused to stand in front of the x-ray machine, and when they told him they could force him to do so he said he realized that but that's what they would have to do—they would have to drag him and several of them hold him, because he wasn't going to let these x-rays impinge upon his brain cells. The anamnesis revealed an obvious life history episode as thematically related to his focal symptom, namely, that when he was around ten years old he had a dispute with a neighbor boy who was insisting on riding the patient's bicycle. The patient had a .22 pistol and after threatening this other boy he (accidentally? not clear in his recall) shot the boy in the head and killed him. I had independent information about this trauma, because my patient lived only two houses away from my parents-in-law and my mother-in-law was a pretty close friend of the patient's mother. He was not prosecuted, nor was any other intervention attempted at that time. A few years later he did have some psychotherapy in the school system with an able psychologist whom I knew slightly, who had had an analysis himself but was predominantly of Adlerian persuasion. The patient had also had a few sessions with an analytically trained school psychiatrist. The patient and his mother didn't think these professionals had helped him appreciably and they both confirmed this in conversations with me. He easily had verbal insight into the possible connection between the homicide and his brain preoccupation, but it was completely isolated. His ready willingness to say that there might be some connection was devoid of affect, and he emphasized that one had to distinguish between psychological factors that could lead a person to take a theory seriously, and the rational grounds for believing it. My groping efforts to deal with him by some mix of psychoanalytic and Rogersian approach went on for quite a few hours with no movement, although he seemed to enjoy our conversations. It occurred to me that if this man's characterological rigidities and intellectualizing defenses were as severe as they seemed, to me and his two previous therapists, to be perhaps one ought to try relating to him the only way he could relate, and the only way he respected, namely, by the work of the intellect. Hathaway agreed, saying "Freud underestimated the importance of the intellect; besides, nothing is working with him, so what do you have to lose?" From that time on our sessions took the form of intellectual discussions that were a mix of fairly dogmatic interpretations on my part of some of his dynamics (e.g., reiterating that shooting the kid in the head almost certainly had something to do with his focus on the brain as his hypochondriacal concern), constantly labeling it as obsessive and hypochondriacal thinking; and dealing patiently, acceptingly but forcefully with what from a normal perspective were the cognitive errors in his reasoning. I must emphasize the very high level at which these goofy ideas were defended by him, with intellectual resourcefulness. For example, when I pressed him about all these other people who went up and down regularly in elevators without suffering any apparent consequences, he made the (essentially valid) point that I was not really in a position to assert that they suffered no consequences, since what he was talking about was not gross brain injury like a stroke, but tiny amounts of damage to the fine structure, or something subtly altered about the neurons' functioning. He reminded me that very old people, even if not clinically diagnosed as suffering from "dementia senilis" (a technical term known to him) did show definite post mortem evidence of the dropping out of cells; and psychometric evidence showed that people begin to undergo some degree of decline in "sheer intellectual power" (which he was careful to distinguish from things like wisdom, experience and perspective coming with age). There is a decline in sheer brains in learning new complex abstract material like mathematics, and of course, in recent memory. So what most of us view as the normal, non-pathological decline of intellect with age, he perceived as the result of small cerebral insults too subtle to be detected earlier on as the process continued.

His main conscious concern about this minimal damage to the nerve cells was not that it was life threatening, or even that it greatly impaired one's intellectual powers, a concern one might anticipate in somebody with his values and self image, but that it impaired the brain's capacity to <u>experience pleasure</u>. He made a distinction between "pure, primary pleasure" and "indirect, derived or secondary pleasure." On the basis of his reading of biology and psychology, he had decided that the only "real" pleasure was pure, primary pleasure, that the other kinds of pleasure were in some sense illusory or self deceiving. Pure pleasures were pleasurable sensations, as contrasted with the pleasure of, say, playing chess or socializing or making money or practicing a skill. We spent many hours on the merits and demerits of that distinction, and again, as the reader can easily imagine, I was not in a position to definitively refute him by any hammer blow arguments, empirical or philosophical.

(I think my work with this patient gave me some insight into certain problems of epistemology that surfaced with the rise of Bayesian and personalistic probability, and an understanding of why disputes about matters ethical, political and religious are usually so fruitless, because both the conditional probabilities and, more importantly, the Bayesian priors are ultimately personalistic. I don't like subjective Bayesianism and would always like to answer that by saying yesterday's posteriors are today's priors, but since there is, except for simple cases like games of chance no algorithm for inductive logic, it is hardly possible to dislodge a belief—no matter how goofy it seems to a critic—when the defender is sufficiently well informed, intellectually resourceful, and strongly committed to maintaining it come hell or high water.)

The plan of attack that I finally settled on after many hours of this material, some of which were in a professional office setting and others taking long walks along Minnehaha creek (he was strongly cathected on nature, and classified nature's colors, like sounds and smells, in the superior category of primary pleasures), was to accept <u>arguendo</u> his minimal brain damage premise, while emphasizing that he hadn't proved it just as I hadn't dispproved it. Also I would provisionally adopt his distinction between the two kinds of pleasures. I would then focus upon the one incoherence I could discern in the system, an internal inconsistency, namely, that he was, by his protective mechanisms depriving himself of many primary pleasure and of almost all secondary ones. The fascinating thing is that as much as he resisted this line, his over cathexis on intellect and <u>his need that I should perceive him as internally consistent and rational within his own premises</u>, slowly moved him into doubting the long term rationality of his constricted way of life. Despite the old adage "A man convinced against his will is of the same opinion still," we all know from personal experience that there <u>are</u>, from time to time, clear and profoundly distressing exceptions to this. People can lose their political or religious faith under the impact of new evidence, despite the fact that for years their entire life has been founded upon some ideology. Persons can be forced to admit reluctantly that they have been betrayed by a trusted friend or lover. I am persuaded, and I think if I had made tape recordings I could persuade others, that this patient was "convinced against his will" but finally abandoned the position. He never fully abandoned his brain theory although it underwent a good deal of attenuation from our sessions. What he abandoned was the associated praxis, because I was able to convince him that his life was not much fun, and never would be, <u>either</u> in terms of primary or secondary pleasure, unless he was willing to "take the chance." We even went through a little pseudo-mathematics about derivatives of the graph of declining primary pleasure capacity in relationship to the total pleasure he would experience living his way or living more like other people.

Following that we ran into the expected problem that when he came down to it he still couldn't quite make himself abandon some of the defective practices. I handled that by a mixed cognitive behavioral approach in which I emphasized that he would be cooking up some disturbing brain sensations if he took an elevator but that since these sensations were not reported by other people, at least <u>that</u> part of it was not the normal effect on the brain but a result of his anxiety. In a couple of phobic sectors, I accompanied him while he tentatively practiced less protective actions, such as driving his car in a different street or in a familiar one but not slowing down quite as much for a bump, and also walking more normally—he had a way of gliding off curbstones when he crossed the street which unfortunately enabled a couple of clinical student in Dr. Hathaway's psychotherapy seminar, where the case had been discussed, to recognize him as what they called "the brain." On our walks I accustomed him by small increments to walk more normally. I never rode an elevator with him, but instructed him to do it by stages, going up one floor, and then when he felt comfortable with that, two floors, and so forth, until he finally he was able to ride the entire twelve floors to the physician's office. Despite exacerbations and recurrences of his original stance, he continued to improve and was able to go back to school. He decided that he did not want to subject himself to the competitive stress of higher academic life, took the civil service exam to be a mail carrier, but finally decided that he would like to teach science in high school. He got a degree in the College of Education without any trouble, and took a job teaching physics in a high quality suburban high school. He began dating a girl whom he married, and sixteen years later he survived a traumatic episode of a pregnant daughter committing suicide. And I also had some follow-up on him partly from my mother-in-law and partly because my wife met him at duplicate bridge tournaments where he gave her a glowing report of his personal and vocational situation. For many years I received a Christmas card from him with expressions of gratitude.

Making the distinction that a psychotherapist, however scientific, makes between what he can <u>prove</u> and what he <u>believes</u> about a particular case, I have little doubt that I "cured" this very sick neurotic by pertinacious application of what would now be called cognitive-behavioral therapy, albeit a deviant form of it. Having such a roaring therapeutic success with such a difficult patient as one's first experience of therapeutic practice was highly reinforcing. It is somewhat surprising that I retained my interest in psychoanalysis after this episode, considering the fact my mentors were so anti-Freudian and the further fact that my admired teacher Hathaway's practice was an early form of RET. I suspect my own modifications of the psychoanalytic approach in later years in the direction of cognitive therapy, and my readiness to value the contributions of Al Ellis before most academics were willing to take him seriously, was attributable as much to this patient as it was to the influence of Hathaway and Skinner on my thinking.

In 1949 I began a course of psychoanalytic therapy for the usual mix of personal and professional reasons with H. S. Lippman, M.D., at that time the only fully accredited member of the American Psychoanalytic Association practicing in the Twin Cities, although there were several psychiatrists who had been analyzed and done varying amounts of institute study and controls. We had just bought a house and were heavily in debt to several friends and relatives, and Lippman being director of a child guidance clinic could only see me once a week, so I classify this as "psychoanalytic therapy" rather than "analysis" but it is certainly illustrative of Sandor Lorand's view that you can, if you have to, practice classical analysis with low density, even once a week, because in all respects except interview density it was classical. Lippman had been analyzed by Nunberg when Nunberg was still in Vienna in the late 1920's, had seminars with Anna Freud and August Aichhorn, used the couch, and his interview tactics were confined 99% to reflection and interpretation. The interpretations were offered with great tentativeness, only rarely with constructions, in the last ten minutes of the hour. I had eighty-five hours of this and found it both beneficial and intellectually fascinating. He recommended that when my finances permitted and there was available an analyst who could see me more densely I should get a full scale analysis, which I did, beginning in 1955. The analyst was Bernard C. Glueck, Jr., M.D., the son of the Glueck who translated Adler and was the chief "alienist" witness at the famous "Leob-Leopold Trial"—Glueck had some analysis with Helene Deutsch when he was a medical student, was subsequently analyzed as a resident by Nathan Ackerman, and received the Diploma in

Psychoanalytic Medicine at the Columbia Clinic when Sandor Rado was running it. He also did a control under a member of the Horney group. His approach, while officially "Radovian," was in fact, I assume because of these diverse training exposures, quite eclectic. I saw him at densities of five to three days a week for three hundred hours, and it was interesting to contrast his analytic style with Lippman's. Glueck was both temperamentally more aggressive (a pronounced mesamorph, he had a residency in surgery before he shifted to psychiatry) and interpretations were offered through much of the hour, with considerable assurance although not dogmatically. I was not surprised at these differences in technique, having read the classic questionnaire study of Glover on the variations and interview tactics reported by the (then quite homogeneous) group of British analysts, all of whom would have classified themselves as "classical."

I believe that psychoanalysis was beneficial to me in several ways, and my wife and close friend MacCorquodale shared this view, as did others who knew me "before and after" well. When clinical psychology students interested in either psychotherapy or personality theory ask my advice as to getting an analysis, I tell them that they would probably find it profitable, and that although my analysis and subsequent controls cost me what would be roughly $50,000 in current dollar values, I have never regretted it. It is difficult to explain to non-clinicians why one counts a personal analysis as educational, personal growth and healing aside. They tend to say "If the studies show it sometimes 'works,' what's the <u>cognitive</u> benefit of undergoing it?" However, there is a psychological difference in the impact between experiencing it on the couch oneself and knowing some statistics, either process or outcome. I am troubled when experimental psychologists (or non-analytic clinicians) say that the only reason somebody who's had couch time tends to think it worthwhile and advises students to get some of it is that he had a quasi-religious conversion to the analyst's theoretical ideas. I think, as Freud thought, that one of the main benefits of couch time is that one learns by accumulating examples, rather than theoretical precept "how the interview goes." My own view, on introspection, is that this is a more important educational factor than the erosion of a future practitioner's own resistances, although doubtless there are individual differences in this respect. Despite my main 300 hour analyst being a Radovian, I never bought the Rado theory, and I never acquired a natural use of Rado's terminology with a couple of minor exceptions. If you ask a typical Ph.D. candidate in Clinical Psychology what it means when we say "Interpret defense before impulse," although he has been exposed to this notion in probably several courses in classes and books, he will not usually be able to come up with even a passable concrete example of words spoken by analyst and analysan, whereas somebody with personal experience of it can usually give several possible examples. A further factor not commonly mentioned in psychoanalytic literature belongs to the <u>motivational component of praxis</u>. How pertinacious one can be on those occasions when one ought to be will depend considerably, I think, on the "punch" carried by personal experience of how one's own resistances worked in concrete ways. Example: I had no mental reservations about the reality of repression as a mechanism of defense, considering that what I had read in Freud as well as the numerous experimental studies were adequate evidence to subscribe to such a notion. But my own preferred mechanisms being primarily obsessional rather than hysteroid led me to take a somewhat washed out view of the power of repression, especially for recent events rather than childhood strivings and happenings. As a result of this, when I worked with patients for whom denial and repression were highly preferred, I had a hard time fully believing that, say, a hysteroid woman could so distort what happened in the preceding interview, and I think my tactics suffered from this lack of gut level conviction. My first hour on the couch with Lippman brought home quantitative power of repression in a degree I would have supposed impossible in myself. I had awakened with recollection of a short dream involving Little Orphan Annie. I repeated the brief manifest content of the dream to myself upon awakening, repeated it to myself again while shaving, and repeated it for a third time in the twenty minute drive from my home to my analyst's office. So the manifest content of this simple short dream had been rehearsed three times within approximately an hour of awakening. In the session, I reported the dream, but when I got to the last part of it where Little Orphan Annie is leaning out of a window, she hears somebody shouting and then an absolute blank, I simply could not recall what the words shouted were. I told my analyst that I simply couldn't believe I couldn't remember this, I had just ten minutes ago in the car recited it for the third time, and I even knew it was a very short sentence. But it just wouldn't come out. He placidly said to go ahead free associating, it would no doubt come to us. Sure enough, after some five minutes in which I got on the subject of my father's suicide, up it popped, the short sentence "Get that pull motor over here!" which was what I had first heard upon awakening from the sound of fire sirens in our alley. This, of course, was the fireman's voice. Now, my point is that in some non-affective, non-motivational sense of the word 'cognitive,' the full bodied sense of firmly believing my long time "official" belief in the phenomena of repression underwent a marked increment. I have rarely since that time had trouble concluding that a client was repressing something that had happened quite recently.

Although I had been practicing psychotherapy beginning in 1942 with that "brain" patient, it was typically only a couple of hours a week. In the early 1950s, partly because I wanted to pile up enough clinical hours to meet the ABPP experience requirements, I started to see more people, and during the decade 1950-60 I was averaging around ten sessions a week (varying from a half dozen up to fifteen) as well as supervising psychotherapy with recorded sessions at the VA Hospital and Mental Hygiene Clinic. During that time my approach was psychoanalytic, usually employed the couch and the fundamental rule, confining my interventions largely to reflections, interpretations, and (sparse) constructions. Even then, the strong cognitive emphasis of Hathaway, perhaps my law interest as shown on the Strong, and what I like to think was a basic flexibility stemming from my commitment to non-dogmatism, led me to behave non-classically at times even with a couch patient, employing approaches that were more like information giving, reassurance, and RET. Consequently, when Albert Ellis began publishing his work I found it congenial to further modify my approach and over a period of time slowly evolved into more of a RET therapist, so that in recent years, while I still have a couch in my office and under certain circumstances utilize it, I would today have to classify myself as distinctly mixed, a psychodynamic and rationally emotive therapist. Part of the source of this movement when I was no longer in analysis myself and no longer doing any controls with my ex-analyst who had left the city for a job elsewhere, it seemed to me retrospectively that while my couch hours with Lippman were therapeutically beneficial although very classical despite the low density, in my 300 hours with Glueck it was easy to classify most of our sessions in an almost bimodal way, as "classical" versus "Rodovian" hours (in which he was much more active, eliciting a behavior report and engaging in what classically would be considered mere intellectualization. Retrospectively it seems to me that by and large the classical hours were more interesting but the Rodovian hours were more helpful. I had an almost litmus test as between these two which didn't depend upon classifying the analyst's behavior, namely, the slight feeling of haziness or even dizziness on arising from the couch which often follows a classical hour but, I think, never once followed a Radovian hour.

While I have found the work of a psychotherapist rewarding, and will probably continue to do several hours a week for the rest of my working life—although I should be reluctant to ever get up to the fifteen hours I sometimes did in the 1950s—I have found the process somewhat frustrating from the intellectual side. There is no doubt that psychoanalytic therapy is more fun, engaging in what Al Ellis calls "detectiving," and in my opinion it is also less strainful, in the sense of ordinary social interaction strain, than the other mode. As we know, Freud retained the couch after abandoning hypnosis partly because he "could not bear being stared at 10 hours daily." I suspect my mixed legal and philosophical talents and proclivities get much freer expression in RET than in psychoanalysis. However, I am sure I thought in 1955 that thirty years later, after thousands of hours of variable technique with varying clientele, I would have come to some more definite conclusions as to how the psychotherapeutic process "works" when it does, which is to me as puzzling as the more commonly discussed puzzle why it fails when it doesn't work. The fact is I find myself almost as unclear about this today as I was in the middle 1950s! My present view, which I cannot document by hard quantitative data in the research literature, but which is not refuted by anything in the research literature, is that the impact of psychotherapy comes from at least a half dozen factors. They include Shoben's "warm bath effect," the reassuring and modeling aspects of the ethological bonding, the erosion of resistances (less important than I once thought) and the challenging of irrational life postulates à la Ellis. I would even include the reassuring effect of having some kind of plausible interpretation of psychological and social phenomena that were frightening partly because they were mysterious. I think that some day the recent insights of the ethologists will be seen to have a more direct bearing upon what happens in the therapeutic relationship than most clinicians currently emphasize. I take it as obvious that different therapists, working with different kinds of patients, will rely [on different contributing factors] from the half dozen major components of the relationship and process. It is disappointing that so much research time, brains, and taxpayer money have been expended in efforts to understand the process quantitatively with relatively slight payoff. The things that seem to be reasonably clear would be obvious from common sense, whereas the more interesting things are still not satisfactorily answered, and in my opinion may not be researchable in the present state of general psychological research methods. Looking back, I am gratified that I abandoned my earlier intention to spend research time studying psychotherapy process after one inconclusive study of formal speech features with MacCorquodale, under an ONR grant on verbal behavior. We found some interesting things, like the well known shift in the verb/adjective ratio, and a change in verb tenses with clinical improvement, but a great deal of feeble and uninterpretable trends. I decided that fascinating as this process was, for some reason it was not something we knew how to research powerfully, and I 'm afraid I think we still don't.

From around 1950, when I was consulting twice a week at the VA Hospital and Mental Hygiene Clinic, I asked the chief psychologists to shift me from supervision on psychodiagnosis to psychotherapy and research. Sharing Rogers' view that in studying of a process done with words, you should hear the words (or at the very least read them), I thought it strange that psychoanalytic supervision was almost never done by recorded sessions (I did persuade my analyst to go along with that in controls). Dr. Daniel Weiner, chief psychologist at the Mental Hygiene Clinic, felt the same way. I supervised four VA trainees, two of whom would meet together and listen to their own and the other's fifty minute session, so that in an hour and a half per patient session we could often stop the tape and discuss what was done. It seemed an efficient way to use the time with such doubling up, but it was also obvious (I think anybody who has supervised psychotherapy by recorded sessions will agree with this) that there is a great deal of uninterpretable "sawdust" that you have to listen to. I never enjoyed supervision as much as I did doing therapy myself, and began increasingly to dislike it, partly because it seemed to me that I did not have many useful suggestions to offer and I was not entirely comfortable taking the taxpayers' money this way despite the obvious justifications that can be offered. My own clientele was heavily academic, professionals and business executives, upper class, psychologically oriented people fitting Schofield's YAVIS Syndrome [Young, Attractive, Verbal, Intelligent, and Successful]. As time went on following the Korean War, the Mental Hygiene Clinic clientele consisted increasingly of chronic cases getting 80% service connected disability, not much motivated to improve, and definitely not the YAVIS Syndrome! At times a trainee would stop the tape and ask "Dr. Meehl, I don't know what to say, what do you say when a patient of yours goes on like that?" I had to answer, in all frankness, "It beats me, you see, none of my patients talk like that, or at least not over and over, week after week the way this fellow does." My growing distaste for supervision was already occurring in the latter portion of my analysis with Glueck, and from time to time it would come up in the sessions and he would offer one or another interpretation, but neither of us was convinced that we fully understood it. We never got much past the phenomenology of it, namely, the feeling that "I don't quite know what I am doing here, and I have little confidence in the efficacy of the few suggestions I can make to you as a trainee." I think that a great deal of nonsense has been written about the big countertransference problems arising from the patient's seductiveness, dependency or hostility. My experience as supervisor and practitioner suggests that some of this emphasis is defensive in nature. It is more comfortable to talk or write about one's erotic countertransferences (which, if you are a healthy therapist and have a reasonably good sex life with your partner, is minimal), ditto the dependency, although perhaps a little worse than the hostility. I think the most important single threat to the fledgling psychotherapist is none of these things but the simple fact that he is committing himself to help this individual, but much of the time he does not know what he is doing, of whether what he is doing is working, or why. My own insecurities in the therapeutic situation have been far more related to cognitive puzzlement and technological frailty, doubts as to whether I could help and, when I did, <u>how</u> I was doing it, than these commonly mentioned psychodynamic processes labeled 'countertransference.'

From 1957 to 1962 I served on the American Board of Professional Psychology and still favor academic clinicians being boarded. As an examinee (the first "non-grandfather" to be appointed) I had felt strongly about the poor quality of the written examination, and there had been numerous complaints. I was astonished to learn that in ten years the Board had never researched the scoring reliability of its research exam, an essay test scored in the usual "global" manner. Ken Clark, Ed Henry, and I (Ph.D.'s from Ohio State and Minnesota!) insisted on a reliability study, and it turned out that the interscorer reliability was .25 (i.e., an examinee's score depended 4 percent on his behavior and 96 percent on "chance," the random assignment of readers). Ed Henry explained the "school solution" scoring system used in the War College, which preserves the essay format (requiring inventive *production* rather than mere answer *selection*) but achieves a high interrater reliability by means of a content checklist. I was asked to build a school solution research exam, and it had a scoring reliability of .86. My prize effort was an imaginary experimental report that contained 31 errors in design, analysis, and interpretation—some examinees only spotted two of these! We also constructed a large pool of multiple-choice items, building each annual exam stratified by content areas, the domain proportions being based on a questionnaire sent to recent examinees.

Soon after Clark, Henry, and I went off the Board, all this was abandoned, mainly because "too many people didn't like or understand it." The lesson I took from this was twofold: (1) psychologists outside the lab, clinic, or library may not think like psychologists; (2) don't invest time in problem solving if the solution's acceptability is a matter of politics, PR, ideology, etc., rather than scientific objectivity.

Before reading Menninger, I had intended to be a lawyer, and on the SVIB my law interest score has equaled my psychology score in five retestings over 48 years. (Around age twelve I studied and mastered my father's six-volume book set on law and in junior high school became expert on *Robert's Rules,* the school paper's typifying Meehl quote on graduation being "I rise to a point of order." In watching baseball games I even tended to identify with the umpire!) In the 1960's I served as an expert witness in two notorious murder cases and audited several law school courses. For ten years I cotaught, with a lawyer and a psychiatrist, a class in Law School. I read extensively in jurisprudence, cotaught a class in it, and felt honored when the law faculty voted unanimously to okay my teaching it alone. (Law students are great fun to teach, as are philosophers; psychologists are a poor second; medical students and psychiatry residents are boring.) I authored or coauthorcd several articles in law reviews, including one cited in a landmark federal case (Lessard v. Schmidt) on civil commitment (see Livermore and Meehl, 1967; Livermore, Malmquist, and Meehl, 1968; Meehl, 1970a, 1971c).

My latent interest in law was revived by reading political theory and ethics, and was more closely related to jurisprudence and the meta theory of legal reasoning than it was to the "psychological" questions of civil commitment and the insanity defense. In the early 1960s I was not otherwise committed over a summer and audited two courses in the law school. Shortly thereafter I was called as an expert witness in a murder case where insanity was pleaded as a defense. I had been a periodic "court-watcher" since my late teens, finding the courtroom scene fun to watch, and now for the first time to participate in. My scores on the law interest key on the SVIB (which I have taken repeatedly as a subject for students in the industrial and counseling programs) were always A, even during the period when I had consciously turned my interest completely away from that subject matter. In 1967 I audited a course in criminal law from a young assistant professor Joseph Livermore, who subsequently became dean of the law school at Arizona and at this writing is a judge on the Arizona Court of Appeals. Simultaneously with this I participated with a psychiatrist Carl P. Malmquist, psychiatric consultant for Hennepin County Court Services, in a second insanity defense murder case where, because of an odd ruling by the judge about corroborated testimony of experts reporting the defendants interview material, it seemed desirable for us to be present throughout the trial, which meant five weeks in court. I was sufficiently interested to consider applying for admission to the law school but decided that since I wasn't interested practicing law, and knew that if one wanted to do some teaching in the law school that was possible for non-lawyers from the medical and social sciences, it would not be worth it just to have those magic letters after my name. At times I've regretted this negative decision in watching lawyers, because it is easy for one who has expertise in an area to fantasize that he could do a better job of examining a witness than the attorney was doing, especially when counsel has not done the requisite amount of homework or preparation of the expert witness. But I managed to audit a few law school courses and while I didn't take the examinations I attended regularly, and kept up with the reading, taking courses in jurisprudence, criminal law, civil procedure, theory of the legal process, constitutional law, evidence, conflict of laws, and equitable remedies. Livermore and I wrote a paper defending the M'Naghten Rule against the standard criticisms made by social scientists and psychiatrists, some of which were rather naive and had been uncritically accepted by Judge Bazelon in the famous Durham opinion. (I understand that more recently the learned judge has become somewhat disillusioned with shrinks, and of course Durham was later abandoned by the same court.) Meanwhile, Livermore, Malmquist and I introduced a course into the Law School offerings called "Psychology, Psychiatry and the Law," which was given for ten years and taken by law students plus a few students from psychology, Criminal Justice Studies, and a few Psychiatry residents. Law students are fun to instruct, their only competitors being philosophy students. Clinical psychology students run a poor third, medical students and psychiatry residents a very poor fourth. There seems to be something about the intellectual conditions of medicine and the habits of medical school instruction—perhaps too much "doctor's authoritarianism" or what?—that dulls the intellects of students so that they are not theory oriented, and definitely neither interested in or skillful at conceptual analysis and argument. Clinical psychology students today have a dash of this medical student mentality, although they are somewhat more interesting in class. Their tool kit for intellectual controversy is narrow, mostly involving the routine objections one can make to somebody's sample not being completely random, or not using the most powerful significance test available. But when it comes to incisive or profound conceptual analysis, or a detailed critique of the way in which certain evidence does or does not strongly bear on a probandum, they just aren't very good at it. I think clinical psychology students as a group, although there are always some exceptions, are less good at it now than they were when I was a graduate student, although that may be the old oaken bucket delusion. Our paper on the M'Naghten Rule was well received, we had reprint requests from both lawyers and psychologists (and a few judges) around the country, and some very enthusiastic letters. Shortly thereafter, Livermore, Malmquist and I published an article on the justifications for civil commitment in the Pennsylvania Law Review, and a few years later had the gratification of seeing it cited in a landmark Federal case on civil commitment by a three judge panel in Wisconsin.

I think that in addition to the excitement of the courtroom scene, and the interesting conceptual puzzles presented, one appeal of forensic psychology to an academic is the application of the intellect in deadly earnest. One is playing chess for blood. There is a certain attraction, even if one is not strongly power oriented, in knowing that if you succeed in convincing the judge or jury on the rational merits of your evidence and arguments, things will happen accordingly, backed up by the full power of the state. This is not an admirable motive, but I believe it is a real one. More altruistically, to write a scholarly article that influences the holding of a federal judge and thereby *directly* affects literally thousands of mentally ill patients and millions of dollars of taxpayer money is a more clear and concrete contribution to society than most scientific research or classroom teaching. We hope that our scientific papers and our instruction of graduate students make some difference in the world, benefiting persons that we never see face to face; but the causal connection there is not quite as obvious as a law review paper that influences courts. In this respect, forensic psychology carries a punch to it for an academic analogous to the practice of psychotherapy, in that one is aware of a more direct consequence to people's lives than in our usual academic activities.

Early attainment of tenure, good salary, and professional recognition mean that a person not insatiably driven by motives of power and prestige is free to do pretty much what he wants, given the permissive mores of the academy. Arguably this can be a disadvantage, allowing dispersal of energies rather than strong focus on long-term theme-centered research programs. I detect some ambivalence here, having the feeling "I could have made more significant contributions, had I played it right." But this is an unrealistic appraisal, because my cyclothymic temperament, low boredom tolerance, and the psychological generators of the interest pattern that got me into psychology would have made such long-term concentration psychologically impossible. Also the early death of my parents, especially my father's suicide, connected as it was with excessive ambition, generated in me a somewhat easygoing approach to productivity. This is comfortable and prophylactic, but rather close to what high-achieving academics call "laziness." Life is short, and one should enjoy it as much as possible. As long as I meet my formal professional commitments, one of the joys of academia is feeling free to pursue whatever interests me. (In ethics and politics I am a moral minimalist, contractualist, and libertarian.)

The result of these attitudes on my scholarly reading and writing was a more varied kind of output than most social scientists permit themselves or feel that they can get by with. Scanning my publication list, I come up with some pretty strange creatures. I find papers that I am proud of for their high-level conceptualization, but which few psychologists have read or even heard of. Examples: several papers on the metaphysical mind/body problem; an article with Michael Scriven in *Science* on the compatibility of science and ESP (Meehl and Scriven, 1956); a paper with Wilfrid Sellars on the philosophical concept of "emergence" (Meehl and Sellars, 1956); a paper on the relation between religion and mental health (Meehl, 1957a); a paper on the treatment of guilt feelings, delivered to the American Catholic Psychological Association (Meehl, 1960b); the article on parapsychology in the *Encyclopaedia Britannica* (Meehl, 1962a); a paper on Feigl's mind/body identity thesis, which some able philosophers have told me is one of the best they have ever read on this subject (Meehl, 1966); articles in law reviews on the insanity defense, civil commitment, relations of clinical psychology to delinquency (Livermore and Meehl, 1967; Livermore, Malmquist, and Meehl, 1968; Meehl, 1970a, 1971c; a paper with Feigl on determinism and freedom (Feigl and Meehl, 1974); two papers in a philosophy journal on the problem of distinguishing psychokinesis from precognitive telepathy (Meehl, 1978b 1978c; an article in the *American Political Science Review* on a paradox in voting behavior, calling into question the currently fashionable econometric analyses of why people vote as they do or why it is rational to bother voting at all (Meehl, 1977); and an article on statistical procedures for estimating the completeness of the fossil record (Meehl, 1983c). I had a lot of fun writing these and would not want to have not written them, although I confess to the paranoid thought that if you publish in certain scholarly areas without the required union card, you are in danger of going unread.

In the early 1960's Dr. Robert D. Wirt organized and chaired a conference (the "Stillwater Conference") to discuss the training of clinical psychologists and particularly to raise the question of an alternative doctorate for practitioners. The only strong advocates of the Psy.D. were Hathaway, Wirt, and myself. Reflecting on the barrage of objections by which we were met, both by the academics *and*—to my surprise—by scholarly professionals from the practitioner community, led me to write a defense of the alternative doctorate (Meehl, 1965a, 1971b). I maintain that nobody has written satisfactory rejoinders to my rebuttals of the usual objections. Though this paper exerted some influence, I decided that there was no point in fighting a losing battle. While I still defend the idea of a Psy.D., I do not myself enjoy instruction with the kind of student who is likely to take it! I sometimes think there is something odd about my mind in matters of this sort. Many psychologists don't advocate anything they wouldn't want to be a part of implementing; indeed, they tend to oppose it on ideological or theoretical grounds. I have never understood this attitude, and I believe some consider me inconsistent when I strongly favor something I would prefer not to have anything to do with. The same is true for me with regard to the distinction between theoretical interest and social importance. People are shocked, especially the liberal intelligentsia that preponderate in social science, if you tell them you are not much interested in a current social problem, and they infer that means you don't have any ethical opinions regarding it. Why should this be? There are all sorts of matters that are terribly important which one does not necessarily find intellectually interesting to think, read, talk, or write about. I am sure that garbage disposal and sanitary sewage are far more important to human welfare, my own included, than mathematical taxometrics or the mind/body problem, but I do not find the technology or economics of sewage disposal an interesting subject to discuss at a cocktail party.

Among the miscellaneous papers I have written are several labeled "methodological," and while they deal with psychology as a subject matter they are mainly contributions to the philosophy of science. In 1947 Kenneth MacCorquodale and I were having a late-night conversation (while we consumed a fifth of rye whiskey) about Hull's famous intervening variable diagram and whether those so called intervening variables were truly such in Tolman's original usage. We decided there was a confusion between intervening variables and what we unfortunately labeled "hypothetical constructs"—(they were not *constructions* in the sense of Bertrand Russell, but we didn't realize that at the time)—and we arrived at a three-fold distinction between the two classes of concepts which seemed persuasive and illuminating. We expected that on awakening in the morning the glow would have gone; as it turned out, we both woke up with a mild hangover but with a persisting conviction that we had arrived at a clarification worth calling to the attention of the profession. Much of the debate between the Hullians and their opponents involved methodological questions about what kinds of concepts were acceptable in science and what kinds were not. We published a paper (1948b) that became widely cited, and disputed, "On a distinction between hypothetical constructs and intervening variables." In 1955 Lee Cronbach and I, as a result of deliberations of the APA committee on test standards, applied this distinction to the problem of psychometric validity in a paper that is considered a minor classic, "Construct validity in psychological tests." I have also written two papers on problems of inference in the psychoanalytic session, a mixed epistemological and statistical question that has fascinated me since I was an undergraduate, but I must say that I have not made much progress in thinking it through.

In the early 1960's the Psychology Department heard a series of visitors in one of the "soft areas" who reported on ongoing research programs which were excessively ad hoc. Each new ad hoc hypothesis concocted to preserve a theory from falsification generated another series of experiments, some of which panned out, others not, leading to more ad hockery, and so on. These research enterprises did not appear to be converging on anything solid, and the ad hoc adjustments were multiplying as fast as the facts, so that the situation is what philosophers and historians of science would, if they use Lakatos' terminology (Lakatos, 1970, 1974; Lakatos and Musgrave, 1970), call a degenerating research program, although sufficient to publish papers and achieve academic promotion! It seemed to me that there was something radically wrong with the whole strategy, but the thing I focused on was a point about statistical significance tests arising from the fact that in the life sciences the null hypothesis is always false. I wrote a paper in *Philosophy of Science* (1967a) pointing out that improvement in precision and sampling stability in the hard sciences subjects a theory to graver danger of refutation; if the theory is strong enough to make point or range predictions, the more sensitive the design or precise the measurements, the greater the chances of detecting a discrepancy between the facts and the theory's predictions. In the soft areas of psychology, where the theory is too weak to generate predictions stronger than directional trends, as the sample size and the reliability measurements increase, the statistical power function rises, and hence the probability of refuting the null hypothesis (which is always false) approaches unity regardless of the theory's verisimilitude. I subsequently developed this line of reasoning further in a paper (1978a) which reached a wider audience among psychologists, and even in this day of easy photocopying I received 1,000 reprint requests before I quit counting. As of this writing I have in press a long paper on the problem which will appear in the Cronbach *Festschrift* (Meehl, [1990a]).

I have been gently needled by friendly colleagues for writing more "think pieces" than empirical studies, especially in recent years. I enjoy it more, and I'm better at it, as shown by the long-term citation rates of my work in the *Science Citation Index.* Indeed, I daresay few highly visible psychologists have publication lists so preponderantly theoretical and methodological as mine. The profession does not usually view much "armchairing" favorably. Colleagues even josh me about my being a Donald G. Paterson undergraduate advisee, and then a Starke R. Hathaway Ph.D. (both of them disliking—almost despising—"mere theory") and yet writing so many more "think pieces" than empirical studies. Ben Willerman once asked me, "Paul, you are so fascinated by Freud's theory of dream work and tell us persuasive stories from your psychoanalytic practice. Why haven't you done any experiments to test it?" To which I replied (shockingly but honestly), "Ben, it's because *I don't know how*!"

In my own defense, I should point out that the published track record is misleading in this respect, for reasons largely out of my control. During the years 1948–65 I was engaged in three major research projects which occupied thousands of hours but have led to scanty publication. One was on political behavior with political scientist Herbert McClosky, psychologist Kenneth E. Clark, and sociologist Arnold Rose. We built some good instruments and collected a large body of data which have been thoroughly analyzed and are quite fascinating. But the team members dispersed or died, and our leader McClosky (now at Berkeley) became otherwise involved, so the projected book was never written. I was also working with MacCorquodale on a large-scale study of drive and reinforcement parameters in the Skinner box, and after running a couple of thousand rats, we discovered a systematic "box effect" that confounded things so badly that the intended parametric interactions were uninterpretable. The Ford Project on personality descriptors led to a wide-coverage and finely honed instrument, and we published a factor analysis of the findings. As noted earlier, through incredible inadvertence the original raw data were apparently lost—data that were qualitatively and quantitatively unparalleled, including 248 therapist ratings after 10 or more hours of contact on 791 patients, using a phenotypic and psychodynamic item pool of the highest excellence. These three bad outcomes make one wonder whether The Cosmos intended me to stick to my armchair!

But I cannot deny that my personality also plays a role in this think-piece preponderance. My cyclothymic temperament leads me to become bored with most subject-matters after a while. There is also an element of passivity in me that perceptive clinicians come to discern but that is missed by persons who are struck by my verbal fluency and high social potency, especially in the domain of intellect. (Perhaps this is why I enjoyed psychoanalytic therapy more than RET, although the latter is more cost-effective.) At heart I am more of a knowledge-absorber, knowledge-integrator, and knowledge-transmitter than knowledge-producer. I read more widely than most psychologists and enjoy nonpsychology reading far more than the strictly "professional" stuff. For example, during my dozen years as a Lutheran, I read over 300 treatises on theology. When I was on the Law School faculty I read more books and articles on jurisprudence and the appellate decision process than any of my law colleagues had done (e.g., none of them had suffered through Roscoe Pound's six-volume *Jurisprudence,* but I did). I have enough scholarly expertise in philosophy of science to teach a graduate course in it, and my philosopher colleague Herbert Feigl once said that any time I wanted to switch fields he could write me a strong letter of recommendation as a philosophy professor. Now all this "Renaissance man" syndrome may be good or ill—the bright students rather like it for a change—but one cannot do it without sacrificing time from empirical research. I have chosen to do so, despite experiencing twinges of scientific guilt about it. (I was pleased to be officially appointed Adjunct Professor of Philosophy because that put an institutional seal of approval on my armchair doings.) Certainly it is not a safe model for a young psychologist to emulate, and I am careful to point that out to those who identify too strongly with me.

These psychodynamic and external happenstance factors are not the whole story, as they are strongly confluent with two rational considerations that (I like to think) play the main role in my preference for writing "think-pieces." The first rational consideration is that a scientist should do what he is good at, and I am better at conceptualizing than at experimenting. My synthetic-creative talents are only somewhat superior to most psychologists (cramped by the dustbowl empiricist flavor of my Minnesota training?); but my analytic powers are, I believe, exceptionally strong, and well cultivated through long association with top-caliber philosophers of science. Knowledge is advanced in several ways, and it has been my experience that there are many more psychologists who are capable of performing a clever and replicable experiment than there are high-level ideators who can create a novel concept or deeply analyze a familiar one, especially one in controversy. Living off the taxpayer, I feel it appropriate to do what I am best at, especially since (1) it's rare, and (2) I find it more fun.

The second rational consideration is more important, less narcissistic, but somewhat controversial. (For younger readers of these autobiographies, it could be morale lowering and bad career advice—but we were asked to be as frank and revelatory as seemed fitting.) By age 35 or so, I had come reluctantly to the sad conclusion that *most empirical research in psychopathology on theoretical matters is nearly worthless, that it does not prove much of anything interesting, one way or another* (as the Dartmouth Conference of 1950 had, alas, convinced me of the weakness of the "grand learning theory systems," a view that is now commonplace). This was not a snobbish dismissal of what others were doing; my research files were full of studies—both on rats and on patients—which were clearly publishable but were never written up.

Schizophrenia provides a good example. In my APA presidential address (1962b) I propounded a neurological-genetic theory of schizophrenia that was pretty heretical, especially among clinical psychologists. During the ensuing decade I took some friendly criticism from colleagues about not having published empirical research on this theory. They assumed I was content to have concocted an interesting theory (they were 60 percent correct about that) and was not even *trying* to research it. But I was. In the decade surrounding that 1962 lecture, I had a half-time R.A. and was attempting to study the schizotypal personality in several ways. We conducted numerous statistical analyses of large samples of VA hospital patient histories, built a Q-sort for the Hoch-Polatin syndrome, analyzed MMPI and checklist data on my private practice cases, studied psychosomatic and other nonpsychotic symptoms and traits in schizophrenic veterans, collected self-descriptive "good" and "bad" adjectives on schizophrenic and borderline cases, studied MMPI "test misses," identified a strong "cognitive slippage" factor in the Ford Project item pool, tried to replicate the old Worcester findings as to vestibular nystagmus, entered Roget's *Thesaurus* to locate possible schiz-related low-frequency adjectives, studied MMPI shifts on remission from a schizophrenic episode, constructed a nonpsychotic schizotype-specific MMPI key, etc. A lot of thought, time, and work went into these projects, and most were publishable, but we never published them. Why not? *Because while they were mildly interesting and largely consistent with my views, they did not strongly corroborate or refute my theory or anyone else's.*

Meanwhile, as this discouraging truth was becoming clear to me, Popper's *Logic of Scientific Discovery* had appeared in English (in 1959), and his emphases on strong tests, noninductivism, falsification, etc., were leading topics of discussion in the Minnesota Center for Philosophy of Science. I finally concluded that the whole social science tradition of testing weak theories by H_0-refutation was a methodological mistake, and I found that Popper, Lakatos, and some local statisticians agreed with me. I realized sadly that if a clinical student needed to "learn about schizophrenia" in a hurry, I would have him read and reread Bleuler's 1911 classic, then spend 100 hours talking with recent and chronic schizophrenics, then read the research on schizophrenia genetics, and finally the research on schizophrenic soft neurology. But I would *not* have him waste his precious time reading the hundreds (no, thousands) of research studies conducted by psychologists, whether psychometric or experimental. The work is usually inconclusive or trivial, sometimes both. This vast and dismal literature rarely tells us anything we didn't know (when it "refutes" clinical experience, who believes it?) and has not, in my opinion, told us anything really important about the disease nor helped appreciably to settle any of the controversies concerning it. Seeing this, I resolved not to make any more empirical efforts until I had (1) developed my theory further, (2) found a few schizotaxia indicators in the literature that show *replicably large* separations, and (3) found or invented taxometric methods capable of testing numerical point predictions from a strong genetic model. As of this writing, these three conditions have finally been met, and I am codesigner of a research project that we believe will definitely corroborate or refute my conjecture that schizophrenia is a low-probability ($p < .10$) decompensation of a soft neurological integrative disorder (schizotaxia) which is inherited as an autosomal dominant of 100 percent penetrance. We believe we can now *answer* these questions, but it has not been possible to do it until the last decade or so.

My methodological skepticism about conventional significance testing has meanwhile engendered some good think pieces about that dangerous topic (Meehl, 1967a, 1978a, [1990a]), and the recent literature indicates that they have begun to have an impact. My current thinking and writing are oriented to formulating a positive methodological program (I call it "neo-Lakatosian") to replace the conventional H_0-refutation strategy. If I can make even a small advance in this direction, that, plus my earlier destructive criticism of the received doctrine, will be worth a dozen or two average-quality empirical studies that I might have done instead—and that might only have added to what Lakatos called the "intellectual pollution" of the social sciences (Lakatos, 1970, p. 176n).

If I have a McDougall "master-sentiment," it is that of rationality, emphasizing *critical open-mindedness.* I have been rather little moved by desires for power, money, or helping (collective) mankind. My professional-status motive, the academicians' *n Recognition,* is fairly strong, but I think weaker than in most high achievers I know, witness my long-standing nonattendance at APA conventions, the declining of almost all speaking invitations or book-chapter opportunities, and general "nonpoliticking." (Given these attitudes and habits, it is odd that I was elected APA president, and I wouldn't stand a chance today. Publishing in both "hard" and "soft" areas, or "pure" and "applied," was important during the postwar period, witness the names Hilgard, Sears, Mowrer, Cronbach, Osgood, Miller, Hebb, Lindzey.) While critical of many societal arrangements and deeply cynical about politicians, I am not a world-improver (exception: I was a passionate and, for me, active opponent to the Vietnam War). My undergraduate socialism stemmed primarily from the (mistaken) opinion that a socialist economy would be more efficient, rather than from compassion for the poor, hatred of the rich, or the usual academic's hostility to businessmen. Voltaire said that in contemplating human affairs, those endowed with an excess of feeling are moved to weep, those with an excess of intellect, to laugh. I am clearly of the second sort.

The overarching value of being open-minded, shutting no cognitive doors, entertaining even strange possibilities (fusion of *n Cognizance* and *n Play)* I see as stemming from a combination of parental precept + reward + modeling ("one should be *fair*"), Mr. Smith's science class, early reading of authors like Bertrand Russell and science journalist Albert Edward Wiggam, and my "teenage logicians" peer group—all converging upon a genetic makeup that included high *g,* low *n Dominance,* low *n Affiliation,* and a certain kind of "passivity" (contemplation over action). While this fair-mindedness obsession, an Allportian *radix,* has helped me to make scholarly contributions, it has its bad side. Example: I spent time and money (when I hadn't much of either early in my career) learning Rorschach with Samuel Beck and Bruno Klopfer and then doing a lot of it for a while, because I wanted to be sure the Minnesota skepticism about projectives wasn't biasing me. I finally realized that the useful yield of incremental validity did not warrant regular use of these instruments, at least as administered by me. I could better have learned that early from the research literature, plus the anecdotal fact that the "masters" Beck and Klopfer, while clinically perceptive men, were in reality *not* all that impressive when interpreting blind. The plain fact is that I wasted a lot of time making sure that I was not being "intellectually unfair" about projectives.

This unity-thema of critical open-mindedness (plus more extensive reading in intellectual history than most psychologists indulge in) has sometimes made me receptive to possibilities that are strictly taboo among scientifically trained intelligentsia. ("Taboo" is not too strong a word here, my experience shows.) The ideology of scientism (as a metaphysic, an epistemology, and a group-shared faith) proscribes certain substantive concepts as well as extrascientific ways of knowing. Colleagues find me paradoxical (some would say inconsistent) here, because while 1 don't understand or trust unscientific ways of knowing, I do entertain substantive notions that are anathema to almost all American psychologists. Example: I am inclined to think there is something to telepathy, and if forced to bet a large sum one way or another, I would wager affirmatively. My friends invoke the "rational conservatism" of science (which, in general, I accept as a sensible *policy*) and tell me this is being too open-minded for my own good. Example: Despite its Teutonic metaphysics, Wagnerian bombast, dogmatism, and numerous factual errors, Oswald Spengler's *Decline of the West* is, I believe, a work of genius containing profound truths about culture and history and disturbingly diagnostic of our present society. Most scientific historians view Spengler as nothing more than a mystical and fascistic crank, so I was pleasantly surprised when a recent issue of *Daedalus* counted the *Decline* among the ten most important historical works of our first half century.

The most shocking heresy to which critical open-mindedness has led me is skepticism about the received doctrine of organic evolution. Students and colleagues react to my (rare) overt expressions of this view with a mixture of disbelief and amused tolerance, the flavor being, "Well, Meehl is a very bright and reflective man, so we will just have to put up with some funny ideas from him now and then." Some attribute my grave doubts about neo-Darwinism to a Lutheran upbringing—quite wrong, as my minimal childhood religious exposure was to a tepid, liberal Methodism, my parents and the clergymen I met being comfortable evolutionists. But I find it quite useless to explain to people that my objections to evolutionary theory are philosophical and scientific, not religious. I have had no denominational connection for a quarter century, and presently hold no theological opinions. I believe Kant's third great question is unanswerable, but if pressed to speculate about the untestable, I would opt for a kind of nonethical polytheism—a doctrine hardly suitable for spiritual support or edification!

An autobiography in this series is no place for polemics about a nonpsychological theory, but since my aberrant views about evolution have been a matter of some curiosity, speculation, and gossip, it is perhaps permissible to list here, without argument, my scientific objections, which are (1) the improbable "chance" origin of the genetic code, (2) the mutual dependence of DNA and complex cellular organelles ("chicken-or-egg" problem), (3) the joint teleology of structures like the vertebrate eye or the neural wiring for the bee's communicative dance, (4) the central "improving" role of random mutations when the thousands of known examples are uniformly disadvantageous or at best neutral, and (5) the absence of transitional forms in the fossil record. There has been increasing concern about these terrible conceptual and empirical difficulties among scientists in recent years (see, e.g., Denton, 1985, and references cited therein) but no real doubts as to the theory itself. Nor will there be any, because evolution is unique among scientific theories in having no imaginable scientific alternatives. Hence it will be held by educated denizens of our culture, regardless of its theoretical implausibility or empirical counterevidence. For my part, I don't believe macroevolution by accumulated random mutations ever took place, and I regret that I won't be around a thousand years hence to see whether the verdict of history vindicates me. Whether my deviant views on this question, held for some 40 years now, have significantly lowered my credibility as to other scholarly matters I do not know.

Writing this autobiography has turned out to be more fun than I had anticipated, providing an opportunity to collect my thoughts about the psychologist's enterprise and my modest role in it, and fond remembrances of persons and tasks. The pleasure is tempered by realizing the ephemerality of much of what goes on in our field, and some ambivalent regrets about how I have conducted my professional life. Although I do not see myself as a highly ambitious person and I believe that I have rarely done any work *mainly* with visions of social acclaim, like everybody I enjoy narcissistic rewards. I think the profession has delivered such ego pellets to me somewhat more than I deserve, in terms of lasting major contributions, but this is the kind of thing that the subject of an autobiography is probably not the best person to assess. I some-times think that professional recognition came to me too early "for my own good," if that makes any sense. My work on the MMPI, latent learning, and methodological questions was becoming fairly widely cited by the time I was in my late twenties; I became chairman of one of the top psychology departments in the world at the age of 31; I was president of the Midwestern Psychological Association at 34; recipient of APA's Distinguished Scientific Contributor award at 38; and APA president at 42. Since 1968 I have enjoyed the prestigious academic title of Regents' Professor at Minnesota. I've been elected to the American Academy of Arts and Sciences and received the Bruno Klopfer Distinguished Contributor Award in personality assessment. In 1987 I was elected to the National Academy of Sciences. I have a respectable tally in the *Science Citation Index* (some articles 40 years old are still being cited) and a few in the *Humanities Citation Index.* So, as regards "professional success," I have had my share of it, and earlier in life than most. My book on prediction is considered a minor classic, although I wish it had a greater impact than it has on clinical practice, from the standpoint both of helping patients and of saving taxpayer dollars. Colleagues perceive me as having contributed to a more quantitative/objective approach to clinical work, but I am unable to detect much impact in most clinical settings. An exception would be the actuarial (now increasingly computerized) interpretation of MMPI profiles; but even there the careful validation and empirical comparison of programs has lagged uncomfortably behind their proliferation. I have a theory of schizophrenia (currently being revised) that has received favorable attention, although my diagnostic checklist for schizotypy has not attained wide use.

An influence harder to trace, but perhaps more important in the long run than anything I have published, are the Ph.D. candidates I have turned out over the years, an average of one per year for the 42 years I have been on Minnesota's graduate faculty. I am proud to have served as advisor to such contributors as Alexander Buchwald, Dante Cicchctti, Richard Darlington, Robert Golden, Harrison Gough (my first Ph.D.!), Will Grove, Donald R. Peterson, Leonard Rorer, William Seeman, and George Welsh. As a teacher I influenced many students who were not my own doctoral candidates, such as Grant Dahlstrom, Harold Gilberstadt, Ben Kleinmuntz, David Lykken, Philip Marks, William Schofield, and Norman Sundberg. The distinguished behavior geneticist Thomas J. Bouchard, Jr., currently chair of our department, is an "academic grandson" of mine, via Harrison Gough at Berkeley.

One gratification in being a college professor is to realize that at least hundreds, sometimes thousands, of persons one has never met have been shaped, helped, and inspired by the lectures, articles, and textbooks of one's students. I have the same feeling when I reflect that there are clinical facilities scattered here and there over the world in which the care of mental patients has become more efficient because the practitioners have been influenced by my writings (however slightly!) or by academic teachers and clinic supervisors who are in the academic line of descent from Paterson through Hathaway through Meehl through Meehl's students.

I have led a secure and leisurely life with a minimum of the financial anxieties and the daily irksome episodes that are part of the human condition outside the academy. I formed a definite vocational goal to be a college professor at an early age, and I have never regretted that decision. But I have sometimes regretted the *field* I went into, because of its low yield of solid scientific intellectual satisfactions. Those branches of psychology that tend to show the most respectable properties of cumulative and quantitative science are not the ones that interest me or got me into the profession. While I have never had any illusions about being a genius or near genius, I am aware that I'm a pretty bright man, and from time to time I find myself thinking *if* I had gone into some other field, like genetics, I would have not merely had a respectably productive academic career and enjoyed myself at it, but I might have been one of those rare nongenius highbrights who makes a major scientific breakthrough. Of course, one knows the statistical odds are against that, even for people in the IQ bracket 175–190, and there is also the element of sheer luck, unless one is possessed of unusual focused persistence, which I am not. The weak (but not zero) "social worker" side of my nature required at least some degree of activity in direct, face-to-face helping, such as experienced by psychotherapists, physicians, social workers, clergymen, and lawyers.

Apart from the egocentric question of whether I could have achieved something "bigger" had I not become a clinical psychologist, there is another factor that leads me to say that despite a pleasant life, interesting companions, and more than the usual share of acclaim by one's fellows, I am at age 68 a somewhat disappointed man. I find this difficult to explain to younger colleagues and graduate students, and I think the reason is that the cognitive orientation of young people is more realistic—perhaps I should say saner—than was true when I was a student and young faculty member. The decade between the mid-1930's and the end of World War II was characterized by high optimism about the expected progress of clinical psychology, including optimism about integration of three great traditions, from the experimental laboratory, psychometrics, and psychodynamics.

When I talk to students about this "integrative optimism" prevailing among faculty and students, say, in 1941 when I entered graduate school, I get the impression that our attitude 45 years ago strikes them as terribly naive on the part of reasonably bright people. In a way it was. But think of the great books that appeared in the decade 1935–45. We had Dollard's *Criteria for the Life History,* Thurstone's *The Vectors of Mind,* Miller and Dollard's *Social Learning and Imitation,* Allport's *Personality: A Psychological Interpretation,* Murray's *Explorations in Personality,* Dollard, Doob, Miller, Mowrer, and Sears's *Frustration and Aggression,* and Hull's *Principles of Behavior.* (I have omitted the most important single book of that period—namely, Skinner's *Behavior of Organisms*—because only a few of us at Minnesota appreciated its earthshaking significance.) These "great books" of that decade were produced by first-class intellects with quite different biases and interests and little overlap in research technique, but it was possible for a person who was neither stupid nor hysteroid to see in them the signs of rapid advance and intellectually satisfying integration. Thurstone was telling us how to identity the individual differences factors of the mind; Hull was mathematicizing the laws of learning; the Yale group were translating Freudian concepts into learning theory and doing ingenious experiments to show reaction formation and displacement in the rat. While I don't suppose any of us had the crazy idea that psychology was practically on the threshold of becoming like chemistry or physics, these exciting developments did make it reasonable to think that it wouldn't be very many years before a large integrative job between the clinic, the laboratory, and the mental testing room would be accomplished.

It didn't turn out to be that way *within* the "grand theories" of the three great traditions, let alone the integration across them. We have settled for more modest theoretical aspirations, and even with that resetting of sights, the record of psychology as a cumulative quantitative science, especially in the "soft" areas, cannot be considered impressive by anyone familiar with the state and history of chemistry, physiology, or genetics. I do not want to blow up this change in the academic subculture into some sort of personal tragedy for me or my contemporaries, which it certainly was not, although I have known a few psychologists who suffered a major identity crisis, severe enough to include psychiatric symptoms, when they "lost the faith" they were reared in by their mentor, whether Skinner, Hull, Rogers, or a second-generation disciple of Freud.

Looking back, I think that one of my generation's mistakes was to take one kind of scientific theory, what may be called the "functional-dynamic," as *the* model for all science, forgetting that there are other kinds of theories in the sciences which may be labeled as "structural-compositional," theories concerning what something is made of and how its parts are arranged, and "developmental-historical" theories that narrate how some system or entity formed and grew (cf. Meehl, 1986d). Secondly, after taking the more exact physical sciences as our sole theoretical paradigm, we further thought in terms of "grand theories," theories which as my friend Paul Feyerabend says are "cosmological," in the sense that they say something about everything there is and everything that happens, whereas there are many interesting, complex, and intellectually respectable mini-theories in other sciences (e.g., the theory of capillary attraction that one learns in a high school physics course).

A person with mixed cognitive and helping needs prefers to have an intimate connection between theoretical understanding and the helping process, which I managed only in the relatively short period in which I was treating patients classically, and even then with the nagging background thought that what I found interesting and scientifically defensible didn't necessarily relate closely with how much I helped the person. I am more likely today to rely on leverage from the "relationship" and a mixture of common sense, intuition, and bits and pieces of psychodynamics than I am to proceed with some "grand strategy," as when we say, "Whatever happens, your task is to interpret," or "Whatever happens, your task is to reflect acceptingly the client's current phenomenology," or "Whatever happens, your job is to reinforce healthy responses and extinguish unhealthy ones." I am not criticizing practitioners who find it possible to live by these monolithic principles. They may be more effective than I am by doing so, even if they are not theoretically correct. Except for certain pervasive attitudes of skepticism and flexibility that I attribute to my basic science training in psychology, much of what I studied to pass my Ph.D. prelims is not closely related to what transpires in an interview. I have learned to live with that fact, but the point is that when I was a graduate student I assumed that by the time I reached my present age we would have figured it out! I am resigned to this intellectually unsatisfactory state of affairs, and today it rarely makes me uncomfortable in my work—but I am not pleased with it.

These cognitive deprivations aside, I can say that I have had a pleasant and sometimes exciting life as a psychologist. I doubt that I could, in fact, have done a better job or made more important contributions in some other field, and there are fields of science for which my talents and temperament make me totally unsuited (e.g., experimental physics). My advice to young persons (other than "pick your grandparents wisely") is to have intellectual fun, because I am convinced that being turned on by the life of the mind is the most important factor, other than brains and energy, in making even such modest contributions to a field of knowledge as I have made.

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