



## Paul E. Meehl

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 schoolteachers, 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.

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. 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.

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.

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!).

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).

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 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.

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.

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.

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. 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 themselves, 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. 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 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. 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 psychoanalysts and social workers,” he said—but if you brought up a case informally, he was helpful.

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. 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. 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.”

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.

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 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. 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 themselves 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.

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!

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. 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.) 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.

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.

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.

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).

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.

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.

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. 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 evenhanded 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.

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.

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 ant clinicians 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!

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.

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).

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 nonredundant 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 hypercatheted 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.

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 coauthored 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).

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.

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."

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 I 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 sometimes 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 Cicchetti, 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 identify 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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