

A PROFESSIONAL AUTOBIOGRAPHY: Do the Mistakes of Youth Become the Wisdom of Old Age?

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On a winter's day, thirty years ago, my son Joseph and I were hiking through the snow to the other side of Lake Harriet to visit my widowed mother. Joe was about 10 at the time and I was recounting to him some hortatory anecdote from my own past, now long forgotten. When I finished my story, we trudged in silence for a little way and then Joe said, "Gee, Dad, it sounds as if you never did anything right!"

I thought I would take that observation as my theme for this memoir. It seems to me that my career as a psychologist has from the beginning depended heavily on chance, rather than sensible planning. I set the beginning in the early spring of 1945. I was 16 years old, a senior in high school, and a friend of mine (I've forgotten who), already 18, wanted to avoid being drafted into the army. Navy recruiters were administering an aptitude test for potential radar technicians that afternoon at my high school and, since my friend planned to take it, I thought I might as well too. Unlike my friend, I passed the test and that opened new and intriguing possibilities. With my parents' permission, I could enlist in the navy on my 17th birthday in June and then, after boot camp, go to school and learn about electronics and radar.

The school turned out to be a large commandeered high school in Chicago in which the gymnasium had been converted to a sea of three-tiered bunks and we were all issued cheap slide-rules that required frequent dosing with chalk dust to make them slide. Just two months after my enlistment, the big bombs went off in Japan and the war ended. Providentially, my childhood susceptibility to ear infections returned and I spent two weeks in a navy hospital. (I still recall being blissfully awakened twice each night by a sweet-smelling nurse who jabbed my backside with penicillin.) Before being sent back to school, I learned that the navy was becoming free with medical discharges as a quick way of reducing its ranks. Therefore, back at school (which was a bore in any case) I encouraged a repeat of the otitis media by exposing my ears to the shower and, sure enough, nine months after my enlistment, I was heading home, honorably discharged and with a 10% "disability."

What this meant was that, instead of just nine months of free education on the "G. I. Bill," I was entitled to a full four years of higher education (like all the other men who had been injured in defense of their country!) and with a monthly stipend more than double that of the ordinary veterans. I enrolled at the University in chemical engineering; my father and three of my brothers were engineers of one sort or another and I had been president of the Chemistry Club in high school, my solitary academic achievement up until then.. The Institute of Technology was crowded with real veterans and I found it rather boring so I made sure to add other, more interesting courses. The ones I liked best were in Shakespeare and in psychology.

In my junior year I signed up for Bill Heron's two-quarter course in learning theory where, in the second quarter, our assignment was to invent a theory of our own that circumvented the inadequacies of the theories of Hull, Tolman, or Skinner. It dawned on me that, while in chemistry I was just about up to Lavoisier, in psychology I was already at the cutting edge! Psychology seemed right up my alley, an exciting new endeavor where lots of obvious ideas had not yet been exploited, a field demanding rough carpentry rather than meticulous cabinet-making. Because I was, in effect, paying my own way, I had only to get the approval of the counselor who advised us veterans and he agreed that psychology was more interesting. Neither of us considered what sort of job I might aspire to with a BA in psychology.

So I changed my major and, by going to summer school each year, I managed to graduate (with a bare B-average) in the spring of 1949. I then got a job with the Minneapolis Sewer Department operating a jackhammer. My then girl-friend, later my wife, was a social worker and she told me that Hennepin County was about to give Civil Service exams for jobs in Old Age Assistance. I was always good at exams so I soon was visiting clients in nursing homes and sort of missing that healthy outdoor work with the jackhammer. But meanwhile, out in the great world, the U.S. had gotten itself involved in yet another war, in Korea. To my dismay I got a call from the draft board informing me that my number had come up. "But I'm a veteran!" I protested. "You are not classified as a veteran unless you served at least 12 months." "But, I was medically discharged! The government has sent me through college as a disabled veteran!" "All the more reason for you to pay something back in Korea." "That wasn't part of the deal!" I signed up with alacrity for graduate work in psychology but the woman at the draft board would not let me off that easily. I had to request a hearing before the board and they turned out to be more reasonable than their clerk. As long as I remained a student, I did not have to go to Korea.

In those days, one got a master's degree before applying as a Ph.D. candidate because it was felt that a year or two of direct contact with a student made for better predictions as to success in the doctoral program. Now, of course, we can predict almost infallibly just from the GREs and letters of recommendation (pause to appreciate the irony), so the preliminary Master's degree is no longer required. I got mine with Bill Heron. I then was awarded an NIMH pre-doctoral fellowship to work toward the Ph.D. in clinical psychology. Finally I was able to see myself as someone who ought to be getting good grades and the good opinion of the faculty. My advisor was Eph Rosen, the department's specialist in projective techniques, but I had no intention of getting mired in that. My plan for a dissertation was to test an hypothesis about the psychopathic personality which I think I got originally from Paul Meehl who got it in turn from Stark Hathaway.

One quality I did have as a graduate student was independence. It really never occurred to me to ask any of my professors how to plan or go about my research project and I did it all completely on my own because I assumed that was the only way. There were two exceptions: Because I was planning to measure electrodermal responses, I needed a chart recorder and I asked Meehl, who was then chair of the department, if the department would invest \$350 in a one-channel Sanborn recorder; he kindly agreed.

Months later, after I had begun to run subjects at the prison, I made an appointment with Paul at which I showed him some of the recordings and asked his advice on how to quantify them. He had never done anything like this before either and I remember feeling dumb about supposing he would solve my problems.

Harriet and I got married in the spring of 1952 and we moved into an apartment in Minneapolis. Harriet did social work with unmarried mothers while I worked on my degree. Sometime in the 1953-54 academic year I noticed in the department office several folders containing application forms for NSF post-doctoral fellowships. Although I had not yet finished my thesis research, nor even begun to write it up, I was intrigued by the remote possibility of getting one of these post-docs, especially when I discovered that one might ask for a year of study abroad. In those days, no one we knew had ever traveled abroad (except, of course, for some of the real military veterans) and it seemed like a romantic opportunity. I had passed the two foreign language proficiency tests required for the Ph.D. but only because I then had a good short-term memory and the tests were not in the least demanding. I had remained monolingual, like most Americans, so I knew that I would be embarrassed to end up in either France or Germany.

I therefore began asking colleagues whom I might study with in England. Shirley Holt, a fellow graduate student, suggested Hans Eysenck who was just beginning to make an important name for himself, at the Maudsley Hospital in London, with his theorizing about the basic factors of personality. I did some quick study of Eysenck's work, wrote up an application, and then forgot about it. On the morning of April 1st, 1954, while I was still in bed, there came a phone call from Western Union, reporting a telegram allegedly from Senator Hubert Humphrey, congratulating me on being one of just 50 young people who had been awarded NSF post-doctoral fellowships. I had lingering doubts that this might be an April Fool's plot until the written copy of the wire arrived in the mail.

About this same time a former Minnesota Ph.D. named Rundquist visited the University, recruiting for some mysterious federal agency called the CIA. I had no plans for after the year in London so I spent an hour with Rundquist learning almost nothing about what my CIA job would entail except that it took them six months to do the necessary security check. I told Rundquist I was interested and thought no more about it.

We moved out of the Portland apartment that spring in order to spend our last summer, before our trip to England, at Harriet's parents' cabin on Lake Minnetonka. Finally, on 5 Sept. 1954, we headed off to New York where we boarded a modest little steamship called the MV (Motor Vessel) Britannic. Some eight days later, we landed in Liverpool, took the boat train to London, arriving at the Royal Hotel after midnight. We had to move to the Imperial Hotel on Russell Square two days later, both hotels rather fancy in appearance but modest in accommodations, no central heating and no private baths. But the Imperial Hotel did have a very superior Turkish bath, which helped moderate the discomfort of a traveler's cold I had acquired.

One mistake Harriet and I collaborated on was in accidentally getting pregnant at that awkward time when we were planning our trip abroad. So, after finding a nice flat in Earls Court (22B Weatherby Mansions, no central heating) I called University College Hospital, thought to be the best obstetrical hospital in England. This was Grantley Dick-Read's hospital, the man who had rediscovered natural childbirth, and Harriet had read his book and liked the sound of it. "I'm sorry but we're always booked up nine months in advance" the lady said when I explained that Harriet was now about five months along. "But we have only just arrived from the U.S." I forlornly replied. "Oh, well then, you couldn't have booked, could you? We'll have to squeeze you in somehow!" So here we were, foreigners, applying to have a baby at no cost to ourselves on the then-new National Health program, at the best hospital in England, and this remarkable bureaucrat bent the rules to let us in!

They had a splendid anti-natal training program and finally, about 2 AM on Feb. 1st, 1955, we called the hospital to say the contractions were about 5 minutes apart, and the nurse said, "Well, you just make yourself a nice cup of tea, dear, and then come on in." An efficient midwife and two medical students officiated at the delivery and I was in charge of hand-holding and trying to determine the baby's gender. An anesthetic gas mask was available if Harriet wanted a whiff (she never did) and we were assured that the gas in question was the same that the Queen had used. After little Jesse made his debut, while Harriet was still on the table, they brought in a "nice cup of tea" with a bent straw so that she could get her strength back.

When I came home to our empty flat after the birth, I ran into Mr. Howe, the caretaker, outside the building and invited him up for a drink. The Howes had one son, who had become a priest, and they were very proud of him. I've always remembered Mr. Howe's toast to my new son: he said "Every man wants his son to be a better man than what his father was."

New mothers were kept in hospital for 10 days in order to have time to rest and learn how to nurse and care for the baby (they only allowed you to have one child at this hospital on the premise that you would then be able to have others anywhere.) Jesse's birth certificate is a large document that identifies his place of birth only as the Borough of St. Pancreas.

So I got busy finishing my dissertation and then, in those pre-Xerox times, sent off my only copy by sea-mail to my advisor in Minneapolis. I also began to wonder how I was going to support my young family after the NSF checks stopped coming. So I wrote to Stark Hathaway to ask if he might have any sort of job for me in Minnesota's Psychiatry Department. Stark came through with another post-doc and then I got a long letter from Rundquist at the CIA. It started out with his telling me that he'd already gotten me promoted from a GS13 to GS14 (or some such) and then broke-off in mid sentence, continuing angrily to say that he'd just learned from Stark that I was reneging on my agreement to work for the CIA. I've often wondered what my life would have been like had I ended up one of the early staff members of that invidious, insidious agency.

After two years of the post-doc back at Minnesota, the state legislature voted new funds for psychiatric research and Don Hastings, then chair of psychiatry, had to scramble to hire some researchers, the prior interests of that department, apart from Hathaway's group, having been clinical work and golf. I became a tenure-track assistant professor of psychiatry in 1957 (12-month salary of \$7,500) and my duties were solely to do some creditable research. The psychology department promptly gave me a joint appointment and I found myself in a second-rate but generous psychiatry department with nearly all of my collegial ties to a really first-class psychology department. The moral of this story is, obviously, that you don't need to scheme and plan ahead, just take things as they come, get pregnant, avoid the CIA, and it will all work out. Luck was with me all along the way!

My first publication ("A method of actuarial pattern analysis." *Psychological Bulletin*, 1956) was a paper that was widely ignored although it was in a good journal. It demonstrated that, in psychology, certain patterns of predictor variables can have high validity even though the usual multivariate prediction equations gave fairly low validity averaged over all cases. The general disregard of this rather interesting paper was my own fault. I then had the naïve belief that once an idea had appeared in print it would be read and, if it had merit, it would be taken up and used by others. What was wrong with that assumption is that, in psychology, so many published ideas are wrong and so many published research results will not replicate that one must accept responsibility for overcoming the resulting sales resistance. In 1963, with my student, Dick Rose, I published a fuller treatment of this same method, illustrating its effectiveness using published data already in the literature ("Psychological prediction from actuarial tables." *Journal of Clinical Psychology*, 1963). The idea still did not catch on and I forgot about my methods of "actuarial pattern analysis" myself since that last paper was published. Having just read it over again, it still seems like a good idea and maybe I can think of a way to use it now, 40 years later.

The Psychopathic Personality

My dissertation undertook to test whether prison inmates who met Cleckley's criteria for primary psychopathy were less fearful than normals and than other inmates. The idea was that a child at the low end of the normal distribution of innate fearfulness will be relatively hard to socialize. He will be less easily controlled by threat of punishment, less likely to develop an effective conscience, more likely to manifest an air of carefree insouciance and the other personality characteristics described by Cleckley. The study showed that, compared to non-psychopathic inmates and to normal controls, primary psychopaths

- (1) gave weaker electrodermal responses to buzzer-warnings of impending painful shock,
- (2) that they learned a complex mental maze as quickly as the controls but failed to learn to avoid those errors that produced painful shocks, and
- (3) that they appeared to be less motivated by physical or social fear on a self-report test of fearfulness.

Note that I had to:

- (1) Teach myself some basic psychophysiology and construct my own GSR sensor, using some of the electronics I had learned in the Navy;
- (2) Create my own fearfulness questionnaire, the Activity Preference Questionnaire (APQ), because the several existing “anxiety” scales measured neuroticism rather than fearfulness (my APQ was subsequently adapted by Tellegen as the Harm Avoidance scale of his personality inventory, the Multidimensional Personality Questionnaire); and
- (3) Construct out of pin-ball machine components a 20-step, 4-choice mental maze called the “Minnesota Leadership Assessment Test” that provided a painful shock for one of the three errors at each choice-point. I was especially proud of the maze because it was designed so as to provide social and self-esteem reinforcement only for correct responses so that the only reinforcement for learning the sequence of shocked errors was actual avoidance of the shock.

Because most psychological theories, like the one I was testing, can only predict the direction, not the size, of correlations or group differences, each such prediction has about a 50:50 chance of being confirmed even though the theory is false. One virtue of this study was its test of three predictions whose joint confirmation gave somewhat stronger than usual support to the original hypothesis.

The published version of my thesis (“A study of anxiety in the sociopathic personality,” *Journal of Abnormal and Social Psychology*, 1957) was subsequently reprinted in five edited collections and in the Bobbs-Merrill Reprint Series. It also initiated a series of studies by others (most notably by Bob Hare at British Columbia and, more recently, by Chris Patrick, a student of Bill Iacono’s and, hence, a kind of academic grandson of mine) which can fairly be said to constitute one of the most internally consistent bodies of psychopathological research. Curiously, I did not participate in these subsequent developments myself, having been diverted into other interests, almost all of them outgrowths of the dissertation. I did begin to teach an annual seminar in psychopathy, which helped me to keep up with that literature. I also contributed an invited article on “fearlessness” to *Psychology Today* in 1982 and I was asked to write the entry for Psychopathic Personality in Wiley’s *Encyclopedia of Psychology*, Vol. 3, (1984), and for the *Corsini Encyclopedia of Psychology and Behavioral Science* (2000).

In 1993, I was asked to contribute a paper to *Applied and Preventive Psychology*, a new APS journal, on the prediction of violence. A rather fatuous literature had recently accumulated arguing that it was unethical for psychologists to participate in, e.g., parole hearings because the data show that predictions of future violent behavior are more often wrong than right. The defect in this logic is that, while we cannot predict validly for all cases, we can be quite accurate in some cases and we can also identify the cases where prediction can be trusted. Oddly enough, this is the argument of my first publication on actuarial prediction, mentioned above, but I had forgotten that myself after 30 years. Nevertheless, I was able to dispose of that objection to the prediction of violence in short order and devoted most of this paper to a more general discussion of the causes of crime.

Finally, in 1995, I published *The Antisocial Personalities* in which I reviewed the now-extensive literature and tried to examine the psychology of crime and violence generally. One reviewer said: “I disagree with much of the book but feel it is original, variegated, intelligent, scholarly, and delightfully written” (Hans Toch, in *Aggressive Behavior*, 1997). In his featured review in *Contemporary Psychology* (1996), Bob Hogan very generously concludes: “This is an important, informative, and enjoyable book concerning the most important social issue of our day. Lykken provides a data-based, persuasive, and indispensable guide for how psychology can join the national discussion before the economists and political scientists coopt it.”

In this book I paid special attention to the “genus” of psychopathy. (I should explain here that American psychiatry had labeled chronic rule-breakers “psychopaths” first, then “sociopaths” about the time I did my dissertation—hence my use of “sociopathic personality” in that paper—but has more recently abandoned both terms from the official nomenclature in favor of “antisocial personality.” Therefore, in my book, I felt free to use “psychopath” to refer to rule-breakers with deviant temperaments, and “sociopath” to mean rule-breakers who may have normal temperaments but have never been properly socialized.) I believe that psychopaths account for most of the (comparatively trivial) crime problem in traditional societies, but for only a small part of the much larger crime problem in developed countries like the U.S. I believe this is because children in traditional societies are socialized in a manner more like that employed by our ancestors in their “environment of evolutionary adaptation.” With such child rearing, only individuals whose innate temperaments make them unusually resistant to socialization (i.e., psychopaths) continue to break the social rules.

In my 1995 book, therefore, I emphasized the type of criminal I christened “sociopaths” who I believe can be relatively normal temperamentally but whose failure of socialization can be attributed to incompetent parenting. This led me into the topic that has dominated my last few years of work, the problem of parental malfeasance and the advocating of parental licensure, discussed toward the end of this account.

The Center for Advanced Study in the Behavior Sciences

In 1955 or ‘56, when I was still a post-doc in Psychiatry, Jim Jenkins was invited to write the chapter on Individual Differences in the 1957 *Annual Review of Psychology*. Jim very generously asked me to co-author it with him and, moreover, he gave me half the responsibility and half of the space. We wrote a good chapter and I think it was the chapter that resulted in our both being invited to the Palo Alto *Center* for the 1958 academic year. For reasons I cannot now recall, I asked that my invitation be deferred to the 1959-60 year and that was granted. We rented our lovely old house at 3139 East Calhoun Blvd. to the novelist, Saul Bellow, arranged with my folks to spend the last night with them (and with our three kids, two still in diapers and Matthew only 6 weeks old) so that we could leave our house immaculate for the Bellows. Then we headed off in my father-in-law’s elderly Cadillac sedan for Palo Alto. The trip was fun, about 300 miles per day, picnic lunches along the way, inexpensive motels each night with the kids

sleeping sideways in the second bed. When we got to Last Chance, Nevada, on the edge of the Mojave Desert, the old car broke down but a kindly garage keeper let me use his space and tools to take off the oil pan and remove a bucket-full of greasy sludge. We spent that night in one of his modest motel cabins and set out bravely in the morning, across the desert, to find the furnished house we had rented sight-unseen from a Stanford faculty member (and a Marxist, judging by his bookshelves) who was also on leave.

Other psychologists at the Center that year included Jerry Blum, Tony and Diana Deutsch, Adrian Horridge, Howard Hunt, Bill Kessen, George and Jean Mandler, Gardner Lindzey for part of the time, and Karl Pribram (almost a psychologist). I spent most of that year reading about cybernetics and trying to develop the idea that psychology should abandon the physics model, searching for general laws that characterize the operation of the human psychic structure, because a principal effect of learning is to produce *structural* (rather than just parametric) *changes*. One reason psychological prediction is so crude is that each person's brain is structurally unique and obeys a set of laws that differ at least slightly from those that govern any other brain. We can aspire to make point predictions, as the physicists do, only for single individuals, that is to say, *idiographic* predictions. The later development of digital computers made all this easier to see—it is obviously difficult to make useful predictions about the behavior of a group of computers if they are all running different software.

Looking back at that period, I wish that I had been able to find a suitable colleague with whom to mull over and debate the vague disquiet that then occupied so much of my time. What I really wish is that I had been blessed with an identical twin, who would have shared my interests and intuitions, and who would have reinforced my conviction that this line of thinking was worth pursuing. I think now that my instinct then was to vaguely realize that reductionism, which had been the basis for so much scientific progress, had limitations. Science had been busy identifying the component parts of systems at each level of analysis down to the so-called elementary particles which, it turned out, had themselves component parts. What science had failed to do in any systematic way was to acknowledge that, at most levels of analysis, the interaction of the component parts yields emergent properties that are not always predictable from a knowledge of the parts. A recent issue of *Science* focuses on the study of “complex systems” and I can see that this was the line of thinking that was too vaguely bothering me during that year at the Center.

While there was little tangible (i.e., published) product of my year at the Center, some of this preparation did manifest itself later, e.g., in my interest in behavior genetics (which does study the structures that all brains have in common), in the idea of emergence (which concerns genetic factors that all brains do *not* have in common), and in my chapter, “What’s Wrong with Psychology Anyway?” written many years later for Paul Meehl’s *festschrift*. Perhaps more important, our friendship with the Kessens led to our taking summer vacations together, their three girls about the ages of our three boys, on Cape Cod, then in Colorado (it turned out they couldn’t join us for that one), then on Hilton Head, and finally on the Gunflint Trail, an important trip for us because it led us to find the log cabin on Gull Lake, where Harriet and the boys spent most of the summers

over the next several years. Those exchange visits tailed off when the Kessen triplets arrived and their lives became more complicated.

When we got home from California—driving a new Ford station wagon that replaced the tired Caddy—we found that the Bellows had split, from Minnesota and from each other—leaving our dear house a grundgy mess from top to bottom (banana peel under the rug, empty cans under the couch, dirty dishes on the shelves, etc.) and *Herzog* was being written in Connecticut describing the dissolution of their marriage. *Herzog*, which won Bellow a Nobel Prize, always seemed to me a tiresome and intellectually pretentious book. The first edition also described Herzog/Bellow changing the storm windows the previous fall and noting “a sent of wild onion in the air.” Harriet said, “I knew some idiot had been standing in my chives!” and the wild onions disappeared from the later editions.

Psychotherapy

For the first twenty years or so of my career as a psychologist, I maintained a small psychotherapy practice, seeing one or two private patients each week in my University office. I should have hated to be a full-time clinician, my days scheduled weeks in advance, but I enjoyed those occasional sessions and I learned a lot from them. There is something oddly liberating about the therapeutic relationship. Social conventions are put aside. You don't have to hold up your end of the conversation or think how to react appropriately to what the patient says; you can forget yourself entirely and focus solely on the other person, trying to understand them. The patient, who might seem like damaged goods seen in another context, seems valuable and important in this setting, complex and interesting. When almost any patient talks about their dreams, you come to realize that there is an artful dramatist inside that head, capable of spinning fanciful allegories out of mundane and often unconnected scraps of recent experience. I never felt that I was an especially gifted therapist but I'm confident that I never did any harm. Because I felt a genuine respect and empathy for my few patients, I think most of them benefited from our hours together. Here are three examples.

Donna. When I was a graduate student in the 1950s, the Psychiatry Department of the Minneapolis General Hospital boasted one full-time psychologist, a part-time psychiatrist (the chief, who came around three mornings a week to do rounds and to push the button on the electroconvulsive shock machine), and lots of very crazy patients. In the summer of 1953, the psychologist went off for a 3-month tour of Europe while I took her place, trying not to look too foolish to the veteran psychiatric nurses who really ran the place. It was there I met Donna, a tall, thin 19 year-old made by the same firm that created the then-young actress, Audrey Hepburn. It was hard to believe that Donna was in the psychiatric ward on referral from the county jail; she had been picked up with a man trying to burglarize a pharmacy for drugs. It was almost impossible to believe that Donna was a heroin addict and had spent the previous 3 months in Chicago, working as a prostitute to support her pimp and her habit.

You must picture a shy, tremulous, soft-spoken girl, demure and vulnerable, who could hardly bring herself to speak of these experiences, just as I could hardly imagine

her enacting them. The court agreed to put Donna on probation contingent on my taking her as a patient and the head of psychiatry at the university hospital agreed to have her transferred there when the summer was over. I saw her daily for the 6 weeks that she remained an inpatient, then once a week for several months, then intermittently over the next 15 years.

During those years, Donna completed a kind of Rake's Progress in reverse, from prostitute and heroin addict, to becoming the star turn at a local lesbian bar, to a serious relationship with a Black Army lieutenant, and finally to a reasonably stable marriage with a young musician. There was much backsliding along the way, binges of wild self-indulgence, impromptu romances, and unplanned trips with new acquaintances. I would not hear from her for months at a time and then I would hear a faint, frightened voice on the phone: "Dr. Lykken? Can I see you?" I would pry out of her a summary of what she had been up to this time and always the protagonist of those wild adventures seemed unconnected to the farouche and vulnerable girl who was reluctantly recounting them. It was hard to believe that the person I had come to know was capable of doing the things that other Donna did; my Donna could barely talk about them, much less do them. I saw the other Donna just once, when she dropped in for an unscheduled social visit; it was the only time I saw her laugh or heard her swear. Having burned thus fitfully but at both ends, Donna's candle guttered out; she died of uterine cancer when she was 36.

Ralph. Soon after I became an assistant professor of psychiatry, a man I'll call Ralph was referred to our University Hospital for *regressive shock therapy*, a radical and dangerous procedure now long since abandoned. Ralph was in his early 30s and his problem was that he had been afraid to leave his parent's house except, for short walks in the dead of night, for some ten years. Ralph had a bad case of *agoraphobia* or fear of crowds and open places. I was asked to do the psychological workup on Ralph before his course of shock treatments was begun. I got interested in his case and asked to be allowed to try to help him in a less drastic manner.

After several conversation sessions to gain his confidence, I began giving Ralph some homework assignments. "This afternoon, Ralph, I want you to walk downstairs, five flights, and go to the front door of the hospital. Then you can come right back." The next week he had to make the same trip each day but on the elevator with other people. When Ralph had mastered the elevator, I assigned him to one of the regular afternoon patient walks, around the block with one of the attendants in charge.

Within a month or so, Ralph was ready to set out on his own, first short walks, then longer ones, then a walk to the drugstore, then an actual purchase at the drugstore. A bridge runs across the Mississippi River not far from our hospital and Ralph was very nervous about bridges. It took at least a week of trying before Ralph made it across but he was so proud! By this time, Ralph had come to realize that he *could* do it, that he was already freer of the limitations of his illness than he had been for years, and that he could go further still. The last I heard from Ralph, he had an apartment of his own, he had a job, and he also had a girlfriend. It is important to see that my role in this was rather like

that of a parent with a shy child. I was interested in Ralph's problems, I gave him gentle pushes in the right directions, and I was as pleased as he was with each success.

If Ralph had turned up 30 years later, in the 1980s, he would have been recognized to be suffering with "panic anxiety" thanks largely to the work of psychiatrist Donald Klein.¹ Ralph would now be treated with drugs like Xanax that forestall panic, but he would still need someone like me to urge him to explore his world in ever-increasing circles in order to convince himself that devastating attacks of overwhelming panic were no longer lurking around the next corner.

Craig and Sally. This "hippie" couple solicited my help for a kind of marriage counseling. Craig and Sally had been living together for about four years and seemed to me to be genuinely in love. But they had a problem. They belonged to a kind of a commune whose members held advanced and liberated views about life, the environment, the evils of war, the virtues of recreational drugs, and about love and sex. Both Craig and Sally subscribed to the party line, which meant, among other things, that love between two people should not rule out spontaneous sex with other people. But Craig and Sally were not getting along; Craig often seemed surly and resentful, and Sally couldn't understand why.

After meeting with them once together and once with each alone, I offered my diagnosis: "You kids live in a world that's foreign to me, but I know that if I were Craig, I couldn't help feeling jealous about Sally's making out with other men, and I think it would make me feel hurt and mad and generally miserable. I know you two think that people shouldn't feel this way, but I think people *do* feel this way, especially men, because that's the way we are made. We can't really help it." I had feared they might make fun of my old-fashioned views, but Craig's eyes teared and his voice choked while Sally reacted with maternal solicitude: "I don't have to do those things, Baby; I didn't think you cared!" It was one of my few triumphs as a therapist.

Teaching.

Apart from one or two seminar reports, I had never done any teaching prior to becoming a member of the faculty. My first assignment came when Paul Meehl, now chair of psychology, asked me to take over his senior-graduate course in clinical psychology. Paul was perhaps the most gifted of several really fine teachers on the psychology department faculty and his two-quarter clinical course was his own invention and a prize package. I remember walking over from the Medical School side of the campus to my first lecture. En route, I thought of some sort of diagram that would illustrate a point I wanted to make. After the preliminaries were finished—filling out the class cards, handing out the syllabus—I started my lecture and soon turned to the blackboard to sketch the diagram I had thought of coming over. The sketch rapidly completed, I looked at it and realized I had no idea of what I had meant to illustrate by

¹ See for example, DF Klein & JG Rabkin (Eds.), *Anxiety: New research and changing concepts*. New York: Raven Press, 1981.

this silly looking drawing! I recall standing there silently for a long moment, looking at the blackboard, thinking that I might have to leave the room, perhaps leave town. I don't remember what in fact I did to resolve that panic, except that I didn't leave the room.

The rest of the course went well enough—I think I probably deserved about a B- over all. I always wrote out my lectures in advance and then tried to give them only half-reading. I recall one incident when I had given on Wednesday what I thought was a very clear and penetrating analysis of some sticky problem, and then on Friday hands were raised by several students who confessed that they hadn't grasped what I was driving at last time. Because I had not brought Wednesday's notes with me, I had to give that lecture over again *ad libitum* with much poorer phrasing, less apt examples, not nearly so smoothly as before. But this time they all smiled and nodded and understood! Not long after that, Harriet and I signed up for a course of contract bridge lessons taught at the Campus Club by a local bridge maven named David Clarren. We got to know David and his family socially later on and liked them all. David was a master bridge player (he'd won the Vanderbilt Cup among other honors) but he seemed to be a bad teacher because his verbal skills were limited and he almost never said anything quite right. Soon I realized, however, that this failing meant that his students had either to give up and tune out or else really work at mentally correcting his mistakes and those who made that choice were active, participant listeners and learned a lot!

But I never really became wholly comfortable teaching. The problem was that how I did on a given day depended on how I felt and varied from a rare low of C- to an occasional high worth an A+. Fortunately my job description never required much teaching, just an occasional course or seminar that I volunteered for.

As I shall explain later, in the mid-1960s I had been working with the two-flash threshold (TFT, see below) when Peter Venables in England published an elegant-looking experiment in which he found a positive correlation (R) between TFT and skin conductance level (SCL) in normals and a strong negative R in schizophrenics. We knew there was something wrong with this so my student, Mike Maley, and I undertook a constructive replication. The VA Hospital let us test a group of schizophrenic patients who had been removed from medication a week or two earlier, and also a group of non-psychotic, non-SC patients as controls. The latter group gave the expected negative correlation but the schizophrenics showed little relationship between the two variables.

I generated what I still think is a reasonable explanation for the SC data but which differed entirely from Venables' interpretation. Therefore, in 1968, I got an NIMH fellowship to spend another year in London, this time with Venables, trying to find out the cause of our differing results. About that same time, however, when I was already a full professor, I had acquired a persistent case of stage fright! Venables was then professor at Birkbeck College and kindly provided me an office and, of course, an invitation to talk to their staff and students about what I was up to. Due to my stage fright problem, I had to refuse and that was really difficult. The British expect Americans to be brash and self-confident and the idea that an American who was actually a professor of psychology should suffer from stage fright raised a lot of eyebrows. I realized I had to deal with this problem.

I read in the Times that people from the Dale Carnegie group were offering a course, one night a week for eight weeks, in a London hotel and I signed up. When I went to sleep on the tube en route to about the sixth weekly lesson, I realized that I was making progress. In that lesson, we had to get up in front and give an impromptu talk on a subject selected by the instructor at the last moment and, by God, I had no problem with that.

My real graduation exercise was a bit more of a challenge. Toward the end of the Dale Carnegie course, I got a nice invitation from Hans Eysenck to come speak to the staff and students at the Maudsley Hospital, and I realized that this would be the real test. During my post-doc with Eysenck in 1954-55, he and his wife, Sybil, had been very generous and friendly to Harriet and me, having us over for dinner several times, evenings which turned into long, friendly arguments about psychology, arguments which I always lost. When I returned to Minnesota, I got a request from Edgar Boring, who had founded *Contemporary Psychology* and was still editor, to review Eysenck's new book, *The Dynamics of Anxiety and Hysteria* and I accepted gladly, expecting to admire the book. Unfortunately, Eysenck, who was a brilliant critic of other people's work, was like most of us a lot less critical of his own ideas. So my long, featured review (1959) was very critical. Eysenck replied, more in sorrow than in anger, and Boring thought my rejoinder was sophomoric and smart aleck. Especially my final sentence, which said something like: "I'm not sure how to respond to Eysenck's comment about my youth, except to say that I wasn't born yesterday, Professor."

Therefore, now, ten years later, I was going right into the lion's den! But Hans was gracious and welcoming (with his 600-item bibliography, in four languages, plus some 50 books, he could afford to forgive and forget) and my talk went perfectly well. Later on, as I developed a bit of a reputation and was invited to give talks at scientific meetings or lectures to the Woman's Club, I always did well—my presidential address at the Psychophysiological Society meetings in 1981 received all sorts of plaudits—but I continued to feel uncomfortable about such assignments until they were successfully completed.

Psychophysiological Research

My thesis research got me interested in psychophysiology and especially in the problems of measuring the electrodermal response or EDR. In 1959, I published a study of different types of electrodes used in EDR work and the silver-silver-chloride electrode, which I showed to be the best, has become the standard in the field. Other papers, in 1961, 1966, 1968, 1971, and 1972, further explored the nature of the EDR.

One methodological contribution that I think was useful was the concept of range correction. The range over which a subject's measured skin conductance will vary is a function of the electrode area and of the density and activation levels of the sweat glands in that area, i.e., it is influenced by instrumental and physiological sources of variance having no psychological significance. Therefore, it is often useful to partial out this extraneous variance by obtaining estimates of each subject's maximum and minimum

skin conductance level, and then to express his experimental level as a proportion of his individual range of variation. In the case of the wave-like EDRs, where the minimum is always zero, one obtains an estimate of the largest EDR the subject is capable of producing and then expresses his EDR to the experimental stimuli as a fraction of that maximum.

With two graduate students, Bob Strahan and Ralph Miller, I did some very satisfying studies of the electrophysiology of skin and the sources of apparent skin resistance and skin potential. My two assistants bravely allowed me to remove a patch of epidermis from their shoulders, by repeated stripping with scotch tape, so that we could measure the recovery of polarization capacity and potential difference over the ensuing days of healing. (Neither Bob nor Ralph remained in psychophysiology for some reason?) I was my own subject during the summer of 1970, stripping skin off my ventral thighs and attaching numerous electrodes through which I applied voltage square waves, observing the waveforms of current on an oscilloscope.

That experiment, published in *Psychophysiology* in 1971, taught me all I thought I needed to know about measuring electrodermal phenomena and constituted the end of a kind of a wandering methodological effort that had begun when I first made acquaintance with the EDR (then called either the galvanic skin response (GSR) or the psychogalvanic response) when I began my dissertation work in 1953. My 1971 paper with Venables, which was designated as a *Citation Classic* by the Institute for Scientific Information in 1990, outlined the method of measuring and interpreting the EDR that has also become the standard in the field.

My former student, Bill Iacono, was elected president of the Society for Psychophysiological Research for the 1996-97 term, a happy event that was not unrelated to my being awarded, at the 1998 annual meeting of SPR in Denver, the Society's Award for Distinguished Contributions to Psychophysiology. What made that occasion especially memorable were the presentation remarks by Don Fowles who sounded as if he had read this autobiography and who obviously had read quite a few of my papers. About that same time, I was asked to write the entry on psychophysiology in the *Corsini Encyclopedia of Psychology and Behavioral Science* (2000).

Polygraphic Interrogation

Again adventitiously, in 1958, I became interested in polygraphic interrogation. Two medical student assistants assigned to me that summer were so efficient that they completed in 3 weeks a project I had thought would keep them busy until Fall. Because of the polygraphic equipment in my laboratory, they had asked me about lie detection and so we decided to do an experiment along those lines.

I had assumed all along that actual lie detection was impossible. Natural selection led our species to acquire language and then the ability to use language to deceive, because both of these talents are adaptive. But Nature did not equip us with some sort of

Pinocchio's nose, an involuntary reaction that accompanies lying but not truth-telling, because that would clearly have been maladaptive.

So I invented instead the guilty knowledge test (GKT) in which one tries to determine if the suspect has knowledge of the crime that only a guilty suspect ought to have. The basic idea is to compare his physiological reactions to the alternatives of multiple-choice questions about details of the crime. Suspects who consistently respond differently to the correct alternatives are likely to know things they shouldn't if they were innocent as they claim to be. We set up two mock crime situations, a murder and a theft. One of my assistants greeted the subjects at the door and walked them thru one or both or neither of two mock crimes, then delivered them to the other assistant who tested them on both crimes.

This experiment worked like a charm. No innocent subject was misclassified and nearly all the guilty subjects were correctly identified. Our study of the validity of the GKT was published in 1959 and subsequently reprinted twice. One of the summer-school students we recruited as a subject was a Hungarian refugee who had served with the anti-Soviet resistance and had been interrogated twice by the KGB without having his cover story broken. We detected which of our two mock crimes he had committed in a painless 30-minute GKT interrogation and he was greatly impressed with our superior technology.

Another study, published in 1960, showed that the GKT was highly resistant to faking or counter-measures. These two papers made me one of the few people with scientific credentials to have published on polygraphic interrogation and led ultimately to a kind of quasi-scientific sideline that has occupied some 25% of my professional life.

In 1974, I published "Psychology and the lie detector industry" in the *American Psychologist*, the house organ of the American Psychological Association (APA). This paper was the first widely read analysis and critique of lie detection and I began to be approached for advice by lie detector victims or by their lawyers for my services as an expert witness. As is my wont, I failed to keep accurate records of these occasions but I can say with confidence that, over the ensuing years, I testified more than 50 times in state, federal, and military courts as well as in Canadian courts. My first such case was in Phoenix where a young Navaho man, an employee of the tribal council, had been accused of rape by a woman he had met in a motel bar. The alleged victim was so squirrely that the DA told my guy that he would drop the charges if Sam could pass a lie detector test. Sam would have to stipulate in advance, however, that the results could be used against him at trial should he fail the test.

Sam's lawyer had taken the precaution of having his client tested privately first and he agreed to the stipulation only after Sam had passed that first test with flying colors. Sam, of course, failed the police test and that was when I was called in. There had been virtually no scientific studies of the lie detector at that time and Sam was saved primarily by a failure of communication between the polygrapher and the detectives. There were three "relevant" questions on Sam's test—Did you force your way into Mary's motel

room?—Did you threaten Mary with a knife?—Did you rape Mary?—and Sam responded strongly to all three. But Mary had reported that she invited Sam into her room to use the toilet, hence I could point out to the jury that his strong responses to all three questions were most likely due to their common reference to Mary and her charges against him, not to deception. Sam was found to be not guilty.

I must say that I enjoyed the expert witness business, all those different courtrooms, different lawyers, different cases. My fees ranged from paying my own travel costs, to just being reimbursed my expenses as in Sam's case (although he did send me a small Navaho rug that was later burgled), to substantial fees when the state was my employer. I am aware of only two cases where my side lost the decision but there may have been more. My testimony was always basically the same, whether testifying for the prosecution (when the defendant had passed a friendly lie test) or for the defense—I did not know if the defendant was guilty or innocent but I could say with confidence that the results of the polygraph test were without probative value.

In 1975 I published a dumbed-down version of my *American Psychologist* article in *Psychology Today* and an invited piece on the same subject in *Modern Medicine*, and I'm rather proud to say that Martin Steinmann, a professor of rhetoric at Minnesota, reprinted all three of these papers in his *Words in Action* (1979) as examples of how to present the same ideas effectively to different readerships. I subsequently contributed invited pieces on the lie detector to some 15 magazines (e.g., *Law Enforcement*, *Physician and Patient*, *Jurimetrics*, *Society*) or newspapers (e.g., the *Los Angeles Times*, *Washington Post*, *USA Today*). I also published some 17 articles or book chapters on polygraphic interrogation in the psychological or scientific literature (e.g., *Nature*), most of them invited. I wrote the entry on Lie Detection for the *Encyclopedia of Science and Technology* (1980, McGraw-Hill), and for Wiley's 1984 *Encyclopedia of Psychology*.

In 1981 Lewis Thomas, an eminent biologist who wrote columns for *Discover* magazine, produced a dumb one reflecting on the implications of the "fact" that humans have evolved an involuntary specific lie response that can be detected by the polygraph. I could hardly let that pass and so I wrote a response that *Discover* published as a one-page article. Once again, I'm pleased to say, this effort was reprinted as an example of effective argument, in Tibbets & Tibbets' *Strategies of Rhetoric* (Scott-Foresman, 1982).

My first book, *A Tremor in the Blood: Uses and Abuses of the Lie Detector* (McGraw-Hill), was published in 1981, the same year that I served as president of the Society for Psychophysiological Research, which helped dignify the scientific status of the arguments presented in the book. In consequence of all this writing, I was often asked to testify before legal or governmental bodies considering the lie detector. Sometime in the early 1970s, I testified before a committee of the Minnesota legislature in support of a bill, subsequently passed, that prohibited polygraph screening of employees in Minnesota and, in 1977, before the Subcommittee on the Constitution of the U.S. Senate's Judiciary Committee, also in support of legislation to prohibit pre- and post-employment polygraph testing of employees in the private sector. That bill failed but was reintroduced ten years later and signed into law. In 1979, I testified before the

Subcommittee on Oversight of the House of Representatives' Permanent Select Committee on Intelligence, on the use of polygraphic screening for counter-intelligence purposes. Once again, in 1983, I testified before the Senate's Committee on Governmental Affairs, concerning Reagan's proposal to expand use of polygraph testing for "national security" purposes. I later testified before a small committee of the President's Foreign Intelligence Advisory Board (of which Ross Perot was then a member). I have to say that my testimony in Washington had no apparent salutary effects. However, that bill to ban most pre- and post-employment polygraph screening, the Employee Polygraph Protection Act, was signed into law in 1988. By this time, the APA had taken a strong position on the lie detector and their support of this bill was important.

In 1983, the CIA was pressuring the British to start using the polygraph to screen their civil servants in the security agencies and the prospective screenees were properly concerned. I was invited to come to London to talk to the Society of Civil and Public Servants, Supervisory and Executive Grades, and to a number of British MPs. Once again I can't be sure about what if any impact my comments and information had on subsequent policy decisions. I feel sure that many British noted the subsequent scandals in which CIA officers, like Aldrich Ames and Harold Nicholson, skilled at beating the lie box, were found to be Soviet moles. In 1987 I lectured on polygraphy at the 38th International Course in Criminology at the University of Montreal. My last significant public appearance relating to this topic was in 1988 when I was invited to debate the noted defense attorney and polygraph advocate, F. Lee Bailey, at the Interservice Military Judge's Seminar at Maxwell Airforce Base in Alabama. Once again, I cannot claim to have had much impact on the military's use of polygraphy, although I thought I won the "debate."

I was very pleased, at the 1990 meetings of the American Psychological Association in San Francisco, to receive the APA's award for Distinguished Contributions to Psychology in the Public Interest, based primarily on my work as a critic of the lie detector. Awards of this kind generally result from the initiatives of a few well-disposed colleagues (rather than from a spontaneous outpouring of admiration from the body at large) but I was no less happy to get this one, in part because I thought it would add clout to my subsequent fulminations on this topic. I was therefore surprised, not to say irked, when Ray Fowler, executive editor of the *American Psychologist*, refused the conventional publication of my acceptance speech on the grounds that it was too polemical.

In their 1993 decision in *Daubert*, the Supreme Court rewrote the rules for the admission of scientific evidence in federal courts (and in state courts that follow the federal lead). Instead of relying solely on whether the type of evidence had achieved general acceptance in the relevant scientific community, judges now were to decide for themselves, usually after holding evidentiary hearings, whether the scientific technique was testable for validity, whether its validity had been adequately demonstrated, and whether in the instant case the technique had been properly used according to accepted scientific standards. By this time, I had begun to pass along all requests for me to testify

to my former student, Bill Iacono. Bill had published some of the best research on polygraphy, he made an excellent witness, and he needed the money. Bill began getting frequent calls to testify at Daubert hearings, where he usually prevailed.

Because of the *Daubert* decision, West Publishing, the prominent St. Paul legal publishers, commissioned some California law professors to edit what turned out to be a two-volume book for judges and lawyers, containing chapters on most of the sorts of scientific evidence currently in dispute. Each chapter was written by a recognized expert. I was approached to write a chapter on the lie detector in collaboration with David Raskin, the leading polygraph proponent who had scientific credentials. Raskin was then a professor of psychology at the University of Utah where he managed to hang Ph.D.s on about five additional polygraph enthusiasts who, collectively, constitute nearly the entire subset of “scientists” who claim that the lie detector is valid (I call them the Raskals). I had testified against Raskin in a number of cases and considered him to be wholly unscrupulous; there was no way in which he and I could collaborate on anything. In the end, therefore, *Modern Scientific Evidence: The Law and Science of Expert Testimony* (1997), contained a long two-part chapter on the lie detector, comprising the case for, by the Raskals, and the case against, by Iacono and Lykken. (Because Bill had taken over my testifying activities, and because he was now arguably the leading scientific critic of the polygraph, and also because he did most of the work on our chapter, it seemed to me appropriate that he be the first author.) Bill and I managed to anticipate and to refute most of the Raskal’s arguments and I believe that our chapter will play an important role in finally weeding the lie detector out of American jurisprudence.

The *Daubert* decision still left a significant role for the opinions of the relevant scientific community and the Raskals had attached great significance to two prior surveys of the opinions about the lie detector of members of the Society for Psychophysiological Research (SPR). One was a telephone survey done by the Gallup organization at the behest of some client wanting to get the polygraph admitted into court. The second was a mail survey conducted by one of the Raskals. Neither survey was worth much (the mail survey got a 30% response rate) and Bill and I determined to do a better one. Debbie Rasmussen, an able undergraduate student of Bill’s, studied up on mail survey techniques, while Bill, with a little help from me, organized the questions to be asked.

To improve validity and also encourage response rate, we included descriptions of the various techniques we wanted to ask about, quoting the Raskal’s own words. Any competent psychologist could evaluate the plausibility of these methods or the quality of the research but only if they first know what the methods are or how the research was done. Debbie handled all the mailing and data recording and we got responses from more than 90% of the SPR members solicited. We also sent similar surveys to the persons honored as Fellows of Division 1 (General Psychology) of the APA, achieving a response rate of better than 75%. The two groups very closely agreed with each other and we found that from 75% to 95% of both groups agreed with our views rather than those of the Raskals. A summary of these results was included in our chapter in *Modern Scientific Evidence* and the complete survey was published in the *Journal of Applied Psychology* in 1997

Early that same year, Michael Hennelly, who had been an editor with McGraw-Hill when they published *A Tremor in the Blood*, contacted me to say that he had moved to Plenum Press and was wondering if I wouldn't like to prepare a new edition of *Tremor* incorporating whatever was new in the field since 1981. I liked the idea and, in 1998, the Plenum edition appeared with a lot of new material, new examples, some lively illustrations, and the like. In the first edition, I had tried very hard to impart an air of scientific impartiality, letting the facts make the case. I was apparently quite successful in this because a number of professional polygraphers, including Norman Ansley, former head of the polygraph section at NSA and the editor of *Polygraph*, asked me to autograph their copies of the book. After all the battles of the 15-plus intervening years, however, I found I could no longer do this with a straight face so I let myself say what I thought from the Introduction on. *Tremor* received a very favorable review in *Nature* in 1998 but, for unknown reasons, *Contemporary Psychology* did not get around to soliciting a review until 2002. Happily, that review also was complementary.

Canada, Israel, and Japan are the only other countries that became seriously infected with the American lie detector virus. But now the Israeli National Police are beginning to use The GKT and, in Japan, where police polygraph examiners all have undergraduate (or higher) degrees plus extensive training in forensic science, the GKT is virtually the *only* method used. And, examiners with the Michigan State Police are currently making arrangements to switch over from trying to detect lying to trying (and succeeding) to detect guilty knowledge. I think these developments give me the most satisfaction.

Preception

One incidental observation during my thesis research was that it probably is not possible to get reliable measures of individual differences in autonomic conditioning from human subjects. When presented with a series of tones (the to-be-conditioned stimuli or CSs) each of which terminate in a painful electric shock (the unconditioned stimuli or USs), the CS does not gradually acquire the power to elicit the strong autonomic response that the US innately produces. The conditioned stimulus or CR does not show a smooth growth curve over trials as described by Pavlov—not at least in human subjects. Because the shock is so aversive, one tries to predict its next occurrence, forming hypotheses in sequence until the obvious correct one has been verified. For many subjects, one CS-US pairing is all that is required. For this reason, I was quick to appreciate Bob Hare's improved "count-up" paradigm in which the subject is told that he will see or hear the digits one through ten in sequence and that he will experience the shock on the count of ten. Then all subjects know what to expect and the amount of autonomic arousal observed during the count-up provides a relatively reliable measure of individual differences in fearful apprehension.

But why do subjects try to predict the successive occurrences of shock in my original design? It seemed obvious to me that most people feel that a brief aversive stimulus is somehow less disturbing if its occurrence can be accurately predicted. In 1959, I published my first paper on what I christened the "preception" phenomenon, the

mammalian ability to augment or attenuate the subjective intensity of an expected stimulus when the nature and time of occurrence of that stimulus can be accurately predicted. In 1962, *Science* published my study showing that the laboratory rat will give much smaller and less variable EDRs (recorded from the foot pads) to mildly painful tail shocks when those shocks are administered following a half-second warning tone than when the tones are either coincident with the shocks or precede the shocks by longer intervals, making the shocks less exactly predictable.

A similar study with human subjects appeared in 1972 and another paper on this topic was published in 1974. We were able to show that this negative preception talent varies widely from one person to another and that it is rather strongly heritable. I have never tried to study “positive preception,” the augmentation of predictable weak stimuli which the subject wants to experience or detect, although I feel sure that the century of study of sensory thresholds must have provided proof that detection is better when the subject knows what the stimulus will be like and when it will occur. I think I must admit that this preception idea has not yet had much of an impact on my field, partly again because I haven’t really pushed it. But I think preception is a real mammalian ability that will someday be generally acknowledged. Preception is related to our remarkable ability to habituate to repeated, meaningless stimuli, even when they are strong and/or painful. Habituation takes some time to develop but, when it has developed, it is economical of cortical resources; we don’t have to consciously anticipate each recurrence in order to attenuate its impact enough so that it does not intrude on consciousness. Preception does require conscious anticipation but it works right away, attenuating the first (or, at least, the second) stimulus in a series, before habituation has had time to develop. One likely peripheral mechanism for at least negative preception would be the spinal gating system described in 1965 by Melzack and Wall.

Schafer (e.g., *Behavioral and Brain Sciences*, 1985) has researched what he calls the “neural adaptability” index (NA), which is the ratio of the amplitudes of average evoked potentials (AEPs) elicited by predictable auditory click stimuli, produced by the subject pressing a button, divided by the AEP amplitude when the clicks are presented randomly. Schafer found high (negative) correlations between the NA ratios and the subjects’ IQ. Because these results also deal with individual differences in the ability to use stimulus predictability to modulate the CNS response to the stimulus, it would be interesting to determine whether our preception scores also are related to CNS efficiency or IQ.

The Significance Test Controversy

When I was a graduate student circa 1950, I had a job for several months in the Student Counseling Bureau analyzing the returns from a “After High School What?” survey that one of the counseling faculty had administered to 57,000 seniors in Minnesota high schools. In the basement of Eddy Hall, I would run boxes of IBM cards, each bearing the responses of one student, through the IBM sorting machine. A few years later, when I was on the faculty myself, Paul Meehl and I used those data for our unpublished “crud factor” study in which we showed that, in psychology, everything is related to everything else, at least a little bit. We cross-tabulated all possible pairs of 15

categorical variables on the questionnaire and computed Chi-square values. All 105 Chi-squares were statistically significant and 96% of them at p less than 10^{-6} . Thus, we found that a majority (52%) of Episcopalians “like school” while only a minority (47%) of Lutherans do. Fewer ALC Lutherans than Missouri Synod Lutherans play a musical instrument.

What this silly-sounding study implies is that Group A is bound to differ from Group B on Variable X so that, if your theory predicts that $A > B$, you have about a 50:50 chance of confirming that prediction empirically—at least if you have a large enough sample—even if your theory is dead wrong.

Meehl used these data as illustrations in a 1967 paper in *Philosophy of Science*. He pointed out that the physical sciences, whose theories are strong enough to permit point predictions (Group A will average 125% of Group B’s score, rather than merely $A > B$), use significance tests in a way that is obverse to the way they are used in the soft sciences. Psychologists say, e.g., that X and Y will be correlated positively and, if that much proves true, then we try to “reject the null hypothesis” by showing that the correlation is so far above the zero or null point, that there is less than one chance in 20 (or more) that the true value of the correlation (which our obtained value estimates) could be as low as zero.

One unhappy consequence of this way of proceeding is that our conclusions become more suspect as our experiment gets better! If we use good, reliable measures of X and Y, then we are more likely to detect the (almost inevitable) correlation between them, and the larger our sample, the more likely it is that this detected correlation will be statistically significant, i.e., have a small enough sampling error and be far enough from zero to believe it really is not zero. A cheap, crappy experiment with poor measures and a small sample that can report a statistically significant result is therefore regarded as more persuasive than a good, big study!

In physics, on the other hand, the object is to *accept* the null hypothesis, which now is the point value predicted by the theory. A challenging experiment, with careful measurements and many of them, will be more likely than a poor one to detect deviations from the experimental prediction (the null) and therefore, if a really good experiment cannot invalidate the prediction, the theory has survived a real test.

About the same time, in 1967, I happened to read an article in a psychological journal in which a psychologist named Sapolsky proposed that some psychiatric patients unconsciously believe in the “cloacal theory of birth” which is that babies are started by mouth and born through the anus. This creative thinker predicted from his theory that people who believe in the cloacal theory will (a) tend to have eating disorders (over-eating if they want to get pregnant or anorexia if they don’t), and (b) they will tend to see frogs on the Rorschach inkblot test. A test of this prediction in Sapolsky’s hospital showed that patients who were frog responders also showed a much higher incidence of eating disorders according to the nurses’ notes.

I thought this alleged study would be a good sacrificial lamb for a paper on significance testing so I asked 20 colleagues whether they believed in this “cloacal theory” idea. The median probability they attached to this theory’s being true was a generous 0.01, which I interpreted to mean, “I don’t believe it.” I then revealed the highly significant results of the “experiment” and asked again about the probability of the theory but the responses didn’t change. Thus my 20 colleagues, who would normally profess to believe in the statistical methodology of our field, plainly rejected it when the results were too—implausible.

One reason why I doubted Sapolsky’s theory was that few patients would know what a cloacum was or that a frog has one. Moreover, biologically sophisticated patients are likely to also know that the frog’s eggs are both fertilized and hatched externally, so that its cloacum is in no way involved!

With this as a base, I wrote an essay on statistical significance testing in psychology and its problems and consequences, a paper that included the first and best analysis of the notion of replication to appear in our literature, and published it in the *Psychological Bulletin*. Briefly, *literal* replication is probably impossible, even by the original experimenter; *operational* replication means following the first author’s experimental recipe, i.e., the details he thought relevant enough to include in his description of what he did; and *constructive* replication, the most valuable kind, involves taking just the first author’s empirical hypothesis and testing it in whatever way you think is best. If an operational replication is well conducted but it fails, then the first author’s results may have been due to an artifact or sampling error, or they may have resulted from some defect in his experiment that he failed to detect or acknowledge. Similarly, if the operational replication works but the constructive one does not, then—unless your own methods were defective—the first author’s empirical generalization was too broad. Those were the pre-Xerox days and people sent for reprints of articles they thought were important. I got more than 600 reprint requests for this paper, an almost unprecedented number, and it was later reprinted in several collections and cited as a “Citation Classic” by SSI.

Years later, Meehl asked me to give three lectures on this general theme to his annual graduate course in Philosophical Psychology and, years later still, I used my lecture notes as the basis for my contribution to Paul’s *festschrift*, a chapter I entitled “What’s Wrong with Psychology Anyway?” Quite a number of academic colleagues from around the country have told me that they have been assigning this chapter in their graduate seminars. One minor but interesting point about operational replication is this: If a researcher tells you the model number of some instrument he used or, like Sapolsky, lists in a table the diagnoses, age, sex, length of hospitalization, etc. of each individual patient whom he tested, then that researcher doesn’t understand what he is doing. What you list in your methods section should be limited to what you believe or suspect might have been relevant to getting the results you got—period.

The War in Vietnam and My "Disorderly House"

In the early 1960s, Buss Glueck, then Director of the psychiatry department's research unit, generously agreed to take me on as a psycho-analysand for training purposes. For about a year I occupied the couch in his office, three hours a week, but nothing much happened. I liked and respected Dr. Glueck but somehow I never developed any useful transference and I expect this was due to the fact that we had a friendly collegial relationship outside of the analytic hour. The main thing I got from Buss Glueck was an introduction to the weekly newsletter published by the greatly respected and wonderfully independent journalist, I. F. Stone. Thanks to the *Weekly*, my wife, Harriet, and I realized in 1965 what a travesty the Vietnam War had turned out to be and what a stream of lies and cover-ups was issuing about it from Washington. Along with numerous like-minded citizens, we joined in support of Gene McCarthy's anti-war presidential campaign in 1968, by which time Harriet had become co-chair of our Democratic ward club.

My own contribution was typically academic and relatively ineffective, although interesting. With the help of my family, I distributed questionnaires about the war to several hundred households in our neighborhood, promising feedback of the results to all those who mailed in their responses. The questionnaire was a mix of items concerned with attitudes toward the war, toward the Johnson administration, views about the plausibility of the "domino" theory and whether and how our intervention was affecting the Vietnamese people and our own national interests, together with questions assessing the respondent's factual knowledge of the history and current status of American involvement. The results were fascinating and my promised summary of them for the participants gave me an opportunity to demonstrate that many widely-held beliefs were in fact mistaken. Fewer than 40% of the respondents had a reasonable grasp of the facts and they were about equally divided between "hawks" and "doves." The remaining 60% were largely ignorant of the facts and their attitudes fitted what they thought they had learned from our government's propaganda; they were the "followers."

During our 1968-69 year in London, I had the opportunity to meet Noam Chomsky and to participate with my family in an enormous and generally peaceful anti-war march which ended outside the American Embassy, a three-deep rank of London bobbies holding back the more fervent protestors just by linking arms in a human fence that gave a bit but didn't break. On our return to the U.S., anti-war marches had become a favorite diversion for the "good guys" and I recall one in which Matthew, then about 10 years old, was riding on my shoulders carrying an American flag. I had insisted on bringing the flag on the grounds that it was bad policy to yield this important symbol to the war-buffs but we proved to be the only participants who had one. As Matt and I walked under the railroad bridge, a group of scoffing workmen on the bridge called down to us: "That flag is in the wrong parade!"

In May of 1970, the U.S. Senate had just voted 51 to 49 to proceed in building a multi-billion dollar anti-ballistic missile installation in North Dakota. Several young women asked us to lend our house for an evening meeting at which to raise funds enough

to let them go to North Dakota and protest this foolish investment by planting flowers on the proposed site. We agreed and they invited people to the meeting by notes or phone calls, and they also circulated a flyer on the University campus. That flyer mentioned that there would be a "cash bar" at the gathering, although I was not aware of that until later. I bought four cases of beer, three of soft drinks, and bags of potato chips as refreshments. The designated time was the evening of the Saturday when Minnesota's most ambitious peace march was planned, to wind from the campus of the University to the Capitol in St. Paul. My son, Jesse, then 15, and I went on the march (as usual, all the best people were there) and then returned, a bit tired, for a quick meal prior to the evening meeting.

The young women arrived early, set up their literature on a card table, together with a couple of wicker baskets for contributions. Jesse suggested that we should charge for the beer and pop to augment these contributions and he made a sign: "Beer, 50¢, Pop 25¢, Power to the People!" and placed it next to a third basket. The guests started arriving just after 8:00. They included a Methodist bishop and his wife, a violinist with the Minnesota Orchestra, a well-known portrait painter, a Quaker pacifist, a social worker, the wife of a psychiatrist neighbor, a prominent member of the Women's International League for Peace and Freedom, the Director of the Minnesota Chapter of the American Civil Liberties Association, and about a dozen other dangerous characters. They were all made welcome, offered beer or pop or coffee, and encouraged to mingle. Unbeknownst to me, one young visitor was a plain-clothes policeman, a Morals Squad officer named Gordon Haertel, who had been sent to investigate this gathering. He accepted a bottle of beer from Jesse, was not asked for any payment, but proceeded to place a marked \$5 bill in the basket. Sometime later, he left but returned with another man, who also "bought" a beer with a marked bill. Because guests were never asked to pay for their drinks, these two marked bills comprised most of the money later found in the basket placed by Jesse's sign.

At approximately 11:30 PM, I happened to be standing with some people in the kitchen and in view of the back door of the house. The door suddenly opened and six or eight uniformed police officers burst into the house. My first reaction was to suppose that there was some neighborhood emergency and I stepped forward, identified myself as the owner of the house, and asked how we could help. "You're under arrest!" was the astonishing reply. At the same time that this warrantless intrusion through the back occurred, a somewhat larger group of officers entered through the front door. We 19 residents and guests were told that we were all under arrest and required to assemble in the living room and most of us obeyed, there being now 19 armed policemen in the house to enforce this order. Jesse refused, saying that it was his house and the police had no right to order him around. I still recall the outrage that I felt, watching two burly policemen wrenching my son's arms up behind him and boosting him bodily out of the kitchen.

The house was searched from top to bottom. One officer came up from the basement with a plastic tube of yellow powder and handed it meaningfully to his sergeant. The wise sergeant tasted the powder, then handed it back. "That's sulfur from a kid's

chemistry set," he (correctly) opined, "Put it back." But they did not put back several cases of fine Bordeaux wine I had stacked carefully in the far recesses of the basement. These were confiscated together with the spirits found within the closed first-floor liquor cabinet. My son, Matthew, then 10, was reading in bed when an officer entered his second floor bedroom and looked around. We asked Matt what he'd thought when the cop came in. "I thought we were being raided." "What did you do when he left?" I went on reading." Matt always was a good reader.

The neighbors had, of course, been made curious by the presence outside of several police cars and two paddy wagons. My next-door neighbor asked me later why the police had spent so much time examining the window ledges with their flashlights. A possible explanation emerged two days later in a phone call from a man who said he was a police officer who disapproved of the raid. He told me that Haertel, the detective who first entered the house, had been instructed to plant some marijuana on a windowsill but had not done so and thus incurred the wrath of his boss.

When the police started herding the guests into the paddy wagons, I asked that sensible sergeant if it was really necessary for my wife and Jesse to be jailed, with Matthew upstairs in bed and Joe, age 12, baby-sitting (ironically) for Roger Sahr, my old friend and attorney. The sergeant agreed that Harriet and Jesse could just be ticketed. I was given special treatment. Together with one young male guest whom I didn't know, I was taken to a squad car, made to stand spread-eagled with my hands on the roof while being patted down (I turned out not to be armed), and then we were driven downtown to the courthouse. We were taken up the jail elevator and, at the top, the young man indicated that he required a toilet so he went off with one officer who shortly returned alone. "What happened?" the second cop asked. "He tried to make a break so I had to lock him up," was the reply. My heart sank, thinking that unfortunate young man must have had drugs in his pocket. It turned out that was correct for the young guest was in fact the Gordon Haertel who had been told to plant some drugs in my house but had defaulted. This whole scene had been staged so that I would not know he was really part of the raiding party.

The 17 arrestees were booked and finger-printed and then allowed one phone call each. I called Roger Sahr who, being a civil litigator, had to consult with the County Attorney about how to get me released. When he drove me home, about 2 AM, I invited him in for a drink, only to find that my house was now drinkless. It was also missing a large box full of other items, selected by the chief raider, one Kenneth Tidgewell, while my wife sat in the living room and watched him work. These 40 pounds of confiscated papers included:

1. Name lists of the members of three parent-teacher organizations to which my wife belonged
2. Reprints and manuscripts of articles containing suspicious terms like "schizophrenia" or "antisocial personality."
3. Several sheets of typing paper with hand-written lists of names of professional baseball players and marked up with cabalistic signs and numbers. These were

the fruit of a game Jesse had invented in which the actions of a baseball game were determined by tossing coins.

4. The list of people who had worked with Harriet on her MCLU court-watching project.
5. Materials we had collected about bull terriers. We had been taken with this breed in England and were planning to acquire one ourselves.

An inventory was made of these confiscated items but I cannot now locate it. Harriet recalls that, in the end, Tidgewell simply swept into the box most of the remaining mail and papers on the desk or the adjacent buffet.

It turned out that Harriet and I were charged with "keeping a disorderly house" while our guests were said to have been "participating in a disorderly house." A charge of selling liquor without a license was later added against me. The raid and its aftermath produced a flood of media reaction. It was front-page news in the Twin Cities, the *New York Times* carried several smaller articles, and friends told me later of reading about my arrest in the Paris edition of the *Herald Tribune*, one friend visiting in Crete and the other on a plane over Scandinavia. The only negative media attention came from a local radio talk-show host, the darling of all local right-wing zealots. As a result we got some unsigned hate-mail and offensive phone calls—one, that woke me early in my bed, was from some woman: "Good morning, Mr. Communist. How do you feel this morning?" Click! But the friendly and supportive mail and phone calls far out-numbered the other kind. I remember one from an elderly lady who said: "I'm sorry for your trouble, but as long as this had to happen to somebody, I'm glad it happened to you because I'm sure you will know how to deal with it."

At the first hearing on the charges, in a packed courtroom, the city attorney told Judge Leslie, "This is just another disorderly house case, Your Honor, like many you have seen before." "Not like this one, Mr. Vavreck, not like this one." Roger Sahr, my attorney, alluded to the fact that the house had been entered and searched without a warrant and Mr. Vavreck responded that the raiders had consulted him and that he'd told them that they didn't need a warrant. Roger rose and said, "Mr. Vavreck has just made himself a co-defendant in the lawsuit that will follow this affair." Vavreck rather hurriedly disclaimed all responsibility for the raid, insisting that it was entirely police business and that he had not ordered or condoned it. Roger rose again and said something that almost made the whole experience worthwhile. He said, "I should like to point out for the record that a similar speech was made 2,000 years ago by Pontius Pilot."

The charges were, of course, dismissed and the police then attempted to return the box of confiscated papers. (We learned later that Tidgewell had offered that box to the local FBI office.. It was reassuring to hear that the FBI refused to accept his offer.) We insisted that the box be inventoried in the presence of the court and then sealed, to be opened later as evidence for our lawsuit. Because Matthew Stark, then president of the MCLU, was one of the arrested guests, that organization took over planning our response. It seemed that most of the better law firms in the area offered to handle our lawsuit on a pro bono basis. In the event, our counsel consisted of two young men, one a solo

practitioner who had done prior MCLU work, and the other a Rhodes Scholar associate of one of the city's largest law firms.

We brought suit in federal court against the city and the police officers individually, under the Civil Rights Law, 42 U.S.C.(1983), alleging an action "taken under color of state law" that resulted in "deprivation of rights, privileges, or immunities secured by the Constitution or by federal law." Judge Neville heard the case without a jury. The police witnesses insisted that they had had neither knowledge of nor interest in the political nature of the gathering, although when I remarked to Tidgewell on that night that they were behaving like Nazi storm troupers, Tidgewell replied; "Don't call me a Nazi. I don't call you a communist although I know you are one." The police testified their search of the house had been limited to looking for other guests, e.g., in the dark room on the far side of the basement where my French wine was stored. They testified that they (i.e., Tidgewell) had not confiscated any papers but, rather, that Lykken had raged around the room, picking up stacks of his own papers and throwing them in the box in which the police had placed only those materials relevant to the arrest. They did not explain why those 19 armed officers had been so intimidated by this harmless professor that they permitted this unbridled misconduct nor did they offer any clue as to how these private papers came to be transported to the Courthouse or reported to the FBI.

One of the high points of the trial came when the late Professor Mulford Q. Sibley, a political scientist much admired by the Lykken family, came to testify as to the local political climate at the time of the raid. Mulford was a tall, Ichabod Crane sort of man and rather shy. He must have planned out the nature of his testimony prior to his appearance. But the defense objected strongly to his appearance, and there ensued a long negotiation about what he could and could not comment upon in his testimony. I remember watching him during this discussion, thinking how disconcerted I would have been to have my plans discarded and the rules rewritten at the last minute. But Mulford got up at last, "affirmed" rather than "swore" the oath (he was a Quaker), and proceeded to give a clear, incisive, and beautifully coherent picture of the climate of the times, entirely within the strictures set for him just before he took the stand. We were very proud to have him on our side.

Judge Neville's ruling also was clear, concise and unambiguous. He found for the plaintiffs, emphasizing the flagrant violation of their Fourth Amendment rights, and awarded compensatory damages of \$500 to each of the 19, plus an additional \$3,000 in punitive damages to each of the Lykkens. (Jesse bought a VW Beetle with his award.) Once again there was a flurry of newspaper attention from the *NY Times* to the *LA Times* plus more pundit columns of opinion. A local attorney objected to the City's decision to pay the damages for the police defendants—they should have to pay the fines themselves—and brought an action to the State Supreme Court. With sympathy for his arguments, the Court concluded that the city had the right by statute to pay those costs.

The main consequence as far as my professional position was concerned was that I was elected a member of the University Senate's Consultative Committee, the chief committee of the faculty governance system, which meets with the President and has

some oversight responsibilities. I had not previously participated in faculty governance at all and there is no doubt that I was elected because the faculty now recognized my name and thought, perhaps prematurely, that I might be a useful activist.

The Psychophysiology of Schizophrenia

As mentioned earlier, in the 1960s I got interested in critical flicker-fusion frequency (CFF), the maximum frequency of repeated light flashes in which a person can detect flicker rather than a continuous light, and also the two-flash threshold (TFT), the minimum interval between a pair of flashes that permits the observer to detect that there were two, rather than a single flash. The TFT is in a sense the limiting case of CFF; a subject who can detect the double flash with a TFT of 60 msec can see flicker in a 1-sec train of flashes whose interflash interval is only about 20 msec. Animal researchers had shown that the TFT is longer when the animal is sleepy or mildly sedated but shorter after giving a stimulant drug or after electrically stimulating the ascending reticular activating system in the brain stem. My student, Dick Rose, showed in 1966 that there is a high negative correlation between the TFT and scores on my APQ fearfulness inventory and I found that, sitting alone in an acoustic chamber where I could control the inter-flash interval with a potentiometer, if I set the interval just below my own TFT while relaxed and then coughed or moved suddenly (any self-stimulation that would produce an electrodermal response), the next few flashes would be seen as double, i.e., my TFT would decrease briefly as my arousal increased.

I had been looking for a demonstration experiment to illustrate the virtues of range correcting skin conductance measures and, in 1966, we reported 11 studies in which the TFT had been correlated with concurrent measures of SCL, skin potential (SPL), or both. In that paper, we reported that SCL correlated $-.44$ with TFT but $-.67$ after range-correction, and that SPL also correlated about $-.63$ with TFT after range correction (and about $+.75$ with range-corrected SCL).

In 1963, however, Peter Venables had reported high *positive* correlations of SPL with TFT in normal subjects but high *negative* correlations in schizophrenics. Our prior work made us certain that Venables' normal data could not be correct, so my student, Mike Maley, and I did a constructive replication of Venables' experiment using 20 VA schizophrenics who had all been removed from medications for a week or more before testing, so many of these patients were getting quite agitated. Our control group consisted of 16 VA psychiatric patients with anxiety or depression diagnoses, none of them having evidence of psychosis or schizophrenia. As expected, we found that the non-schizophrenic controls showed high negative correlations between the TFT and SCL and SPL, both range-corrected. The TFT also correlated $+.67$ with CFF but, oddly, the CFF had only weak correlations with the electrodermal variables.

The schizophrenic subjects presented a different pattern. First, the TFT and the CFF correlated only $-.13$. Second, the correlations between the two cognitive variables and the electrodermal variables were weak and positive in sign. I proposed an interesting hypothesis to explain these results but one that has never been tested. I pointed out that

both Venables and we had used between-subject correlations to estimate the average *within-subject* correlation over time, a maneuver frequently employed by investigators who often seem not to realize quite what they are doing. This procedure depends upon the very risky assumption that the function relating the two correlated variables is homogenous across subjects. Suppose, however, that X and Y are linearly related — $X = a + bY$ — for schizophrenics as well as for normals, but that the schizophrenic sample is heterogeneous with respect to, say, the parameter *b*. Then it could well happen that, although the within-subject covariation of X and Y is as high in schizophrenics as in normals, the between-subject correlation might be zero or even reversed in sign.

Suppose, that is, that SC levels represent the degree of energy mobilization in the subject at the time while the TFT represents the level of cognitive arousal that this energy expenditure has achieved. If one feature of the illness is to reduce the increment in X produced by a given increment in Y (i.e., to reduce the size of *b*), and if the patient group is heterogeneous in respect to *b*, then our findings might be understood. The test of this hypothesis would be to measure SCL and TFT on repeated occasions in the same subjects and then look at the within-subject correlations. I would predict that these values might be as high among schizophrenics as among normals. Then the parameter, *b*, might qualify as another useful psychophysiological indicant of the illness.

In 1969, I joined Venables in London trying to explicate the differences in our findings. One of his students, John Gruzeleir, was entrusted with running the study at a local hospital while I, to my shame, focused my attention on other things (see above). The Gruzelier study was published later in the *Archives of Psychiatry* but I have to say that I never trusted the data, which clarified nothing really. I don't mean to impugn Gruzelier's ability or honesty but rather to simply acknowledge that I should have paid much closer attention to that project and run at least the first half of the subjects myself.

In the middle 1970s, Phil Holtzman had recently reported his finding of defective smooth eye-tracking in schizophrenia. I was intrigued enough to rig up a simple pendulum in my lab and bring over two or three reasonably intact schizophrenic patients from the hospital to see if they could sit quietly while smoothly following the pendulum with their eyes. We measured eye movements electro-oculographically (EOG) by means of electrodes to the right of the right eye and left of the left eye. (The eyes are like little batteries so that the electrodes record a smooth sine wave if the subject is smoothly following the pendulum.) Sure enough, these patients showed exceedingly erratic tracking.

My student, Bill Iacono's, thesis project was a study of eye tracking in normal twins, showing both the wide variation among "normals" in smooth tracking ability and also the high heritability of this proficiency. Bill went on to study this phenomenon in schizophrenics and in their relatives and has made major contributions in this area.

Research with Twins

This heading was the title of my presidential address to the Society for Psychophysiological Research (SPR) in 1981 and signaled a change in focus of my own thought and research from psychophysiology to behavior genetics. I had begun working with twins in the early 1970s on the principle that any research one might think of doing with human subjects is likely to be more interesting if you do it with twins. Those first twin subjects were mostly college or high school students who were recruited in a rather haphazard fashion. With an NIMH grant, I studied fearfulness, preception, habituation, and related topics with these twins.

Emergenesis

In 1974, with Tellegen and one of his students, I published a study of the heritability of EEG parameters in twins. This work was replicated and extended in my 1982 paper with Iacono and Tellegen. Whereas the MZ twin correlation, for the proportions of the resting EEG frequency spectrum that occupied the traditional frequency bands (Delta, Theta, Alpha, and Beta), is high and about double the value of the DZ correlation, the MZ correlation for the mid-Alpha frequency or *Phi* was equally high (+.80) while the DZ correlation was near zero. I suggested that Phi might be determined by a configuration—rather than a sum—of polygenic influences. This theme was further developed in my 1982 presidential address in which I introduced the idea of *emergenesis* and most extensively in my 1992 paper with Bouchard, McGue, and Tellegen.

Most behavior geneticists were used to dealing with metrical traits, like stature or IQ, which are polygenic-additive, meaning that each of the polygenes contributes additively to the total result. But most monomorphic traits, like the human eye or the 5-fingered hand, are constructed by polygenes working *configurally*, like workers on an assembly line, so that any missing or defective gene is likely to produce a qualitative, rather than a mere quantitative, change in the outcome. I think it has become quite clear by now that many polymorphic traits also are constructed *configurally* rather than additively.

For example, McGue and I measured happiness levels, using Tellegen's MPQ-Well Being scale, on some adult twins twice, years apart. The MZ twins correlated with each other over that long interval nearly as strongly as they correlated with themselves, indicating that the stable component, or what I call the happiness set-point, is very strongly determined genetically. But for DZ twins the cross-twin, cross-time correlation was about zero, indicating that the happiness set-point is apparently emergenic and that means, in turn, that while this trait is strongly determined by genetic factors it does not tend to run in families. This is because the parent is unlikely to pass on the entire intact configuration in the random half of that parent's genes received by the offspring. I believe that quite a few other traits, including important ones like leadership, parenting ability, perhaps talent for teaching and for salesmanship, and especially human genius (Lykken, 1998), are all emergenic traits.

I also believe, incidentally, that emergeneses may account for some of the sudden leaps ahead that seem to have characterized the evolution of species, the so-called "punctuated equilibrium." I've made the argument in several places that the running

talent of the great thoroughbred racehorse, Secretariat, was emergenic. By his time, most of the heritable additive variance had been bred out of the great racehorses and further small improvements were attributed to better training, veterinary practices, and so on. Then along came this great red stallion who broke the records at the Kentucky Derby, at the Belmont., and then at Pimlico, not by a whisker but by seconds. Put out at once to stud, he produced some 400 foals by the very best mares, yet almost all of them were disappointments. Finally, in 1988, one of his sons, Risen Star, won two of the Triple Crown races but even he could not have raced with his dad.

Long ago, on the Steppes of Russia, a Secretariat would have been the dominant stallion of his herd, would have serviced many mares, some of them his own offspring. Although he couldn't pass along his essential gene configuration in half-helpings, such mating with his own daughters would have greatly improved the chances of passing the intact configuration to the foal. Sooner or later, one such qualitatively-advanced stallion would have begun to produce foals like himself and their advanced capabilities would have rather suddenly spread, producing a qualitative leap forward in evolutionary properties.

In 1978, I published a reasonably comprehensive methodological paper on the diagnosis of zygosity in twins. This paper was fairly widely cited but now we know that, although neither twins nor their parents can be trusted to know the zygosity accurately (because many obstetricians still seem to believe that all MZ twins share a single placenta), a few simple questions put to the parents in person or by mail can yield about 95% accuracy. For more accurate work with smaller twin samples, DNA testing has become as cheap and more reliable than the earlier comparisons of blood antigens and other proteins.

MISTRA

About this time, Tom Bouchard came upon a newspaper article concerning a pair of MZ twins who had been separated in infancy and who had just found each other at age 39. Tom wanted to bring these men to Minnesota for extensive testing as a kind of case study and, knowing that I was already doing twin research, he invited me to collaborate. I was doubtful that a case study of one MZA pair would be worth the trouble but Tom was indefatigable. He obtained funding, arranged for medical and dental examinations, psychiatric interviews, and extensive psychological testing using multiple instruments. My only contribution was to run the twins each through the half-day set of psychophysiological measures we had already set up for our then-current twin study, plus a computer-administered intelligence test involving the Raven Matrices and the Mill Hill vocabulary test.

Tom had already arranged for professional psychometrists to simultaneously administer WAIS IQ tests to the twins, in different rooms and, on a third day, his RAs administered still another battery of ability tests. This is illustrative of Bouchard's philosophy of assessing all important areas in several different ways. This first pair of

MZA twins ('A' for 'apart') had many strange similarities and they, with Tom, received considerable national media attention. The result was that Tom began to hear of other reared-apart twins and, before long, the famous and important Minnesota Study of Twins Reared Apart (MISTRA) was off and running. I credit Tom and MISTRA with having played a major role in swinging the pendulum of both public and professional opinion back from the radical environmentalism that had been dominant since the 1930s.

The Minnesota Twin Registry

Knowing that we should require large samples of garden-variety twins reared together, with which to test and extend hypotheses generated by the MISTRA findings, Bouchard, Matt McGue, and I began the Minnesota Twin Registry in the mid-1980s. We identified from birth records all twins born in Minnesota from 1936 through 1955, located (most of) the surviving intact pairs, some 4,000 of them, and recruited them to provide questionnaire data by mail. Upon my retirement in 1998, I turned over the Registry to my young colleague, Bob Kreuger, who I know will make good use of it.

The Parenting Project

Matt and I also recruited a large sample of Minnesota-born 30 year-old male twins. This was for a study of the relative importance of parenting and genetic factors in determining the extent of antisocial behaviors admitted by these men. This Parenting Project provided useful data for my invited paper in the *Journal of Personality* on "The Causes and Costs of Crime and a Controversial Cure" and these 430 twin pairs were added the Registry data base.

The Minnesota Twin/Family Study

Finally, in about 1985, I obtained a 5-year grant from the National Institute on Drug Abuse (NIDA) to begin a longitudinal study of risk factors for substance abuse in young male twins. With Bill Iacono and Matt McGue, we recruited male twins aged either 11 or 17, to come to the University with their parents for an initial assessment with the intention of bringing them back at 3-year intervals. Because virtually anything might be a risk factor for substance abuse, this project permitted us to assess everything we could think of, from psychiatric history to abilities and personality traits, the assessment of peers and of interests, of parenting attitudes and practices, teacher ratings, etc., etc. After several years, I wisely turned over administration of this project to Iacono and McGue (who, unlike me, are both talented administrators) and McGue proceeded to get a grant from the National Institute on Alcohol Abuse and Alcoholism (NIAAA) to do a parallel study with female twins. This very large, very important Minnesota Twin/Family Study is now being extended yet again to include a parallel assessment of a sample of adopted children. I feel confident that this project, which will continue well into the current new millennium, is producing a data-base of wide-ranging relevance and importance and that it will come to be regarded as a landmark contribution, not just to the problems of substance abuse, but to psychology broadly. The credit for this will belong to Bill and Matt but I am glad to be able to claim credit for providing at least the initial impetus.

From Psychiatry to Psychology

After the NIDA grant was first approved, my relationship with the then current Chair of Psychiatry, Paula Clayton, began to deteriorate. I had decided that the best place to house this big project was in space made available in Elliott Hall by the then-Chair of Psychology, Tom Bouchard. Paula thought this decision was “disloyal,” among other things. She, meanwhile, had split with her husband, Charlie, and formed a new relationship with one Bob Rose, a psychiatrist then at a Texas university. Paula informed the Medical School Dean that she would resign unless he established a “distinguished chair” for her new lover, Bob. Paula being the only female department head in the medical school, the Dean (foolishly, I think) acquiesced to her demand.

Still not satisfied, Paula insisted that an international search be made to fill this chair (so as to avoid embarrassing Bob by letting the world know he had been hired to be Paula’s pet) but, of course, she wanted to be sure of the results of that search. She appointed James Halikas, her former student whom she had hired a few years previously, to head a search committee. Dr. Halikas wrote a job description for the new position based on Bob’s vita, a description that opened a window through which only Bob could crawl. Finally, Paula called a meeting of the full professors to vote on Bob’s appointment, attended it herself with her secretary taking notes, and called on another of her former students and recent hires to make the motion. I spoke next, explaining that I would consider that voting for this motion would make one an accessory-after-the-fact to a fraud upon the University and the taxpayers. The motion won by 6 to 4 but Paula wasn’t happy. Within a week she had transferred my secretary to work for Bob and within a month I was notified that she was giving me the statutory minimum raise although Leonard Heston and I were then the best-known and most widely cited members of the department. I then met with Paula and outlined two options: One was that I would file a grievance and begin by holding a press conference to make public her behavior. The other option was that I would transfer to Psychology and take with me my grants, my laboratory equipment, and also my salary item. Paula (and the Dean) accepted option #2 and I moved to Psychology on the first day of 1990.

A Professor of Psychology

This transfer to Elliott Hall was the best move I’d made in years. Now the people that I respected and wanted to work with were right down the hall rather than on the other side of the campus. I count in my bibliography some 25 to 30 articles or chapters growing out of all this twin research, most of them co-authored with Bouchard, Iacono, McGue, or Tellegen and others. We know now that virtually every psychological trait that we can reliably measure owes from 25% to 75% of its variance across people to genetic differences between people. We know that MZ twins reared apart will be nearly as similar psychologically in adulthood as are MZ twins reared together. For this and

other reasons, we know that being reared together in the same family does not tend to make children more alike (except if the family environment is very bad or possibly if the parents are remarkably charismatic and influential.) We know that a number of important polygenic traits are emergenic, with high MZ but low DZ correlations so that, while genetic, these traits tend to run in families only weakly.

We have also shown, although the message has not yet been widely comprehended, that the stable component of many psychological traits is much more strongly genetically influenced than is currently realized. The customary analysis of twin data assumes, erroneously, that the degree of similarity between MZ cotwins that is due to their sharing the same genome must be fixed and therefore assessable with a single valid measurement. In fact, however, many (probably most) psychological traits—traits like happiness, irritability, aggressiveness, fearfulness, even various aptitudes and interests—vary within a subject unsystematically over time—happiness varies, for example, according to the slings and arrows, etc. Therefore, a single measurement may find Twin A feeling good for some reason while Twin B is feeling bad, for some different reason. If we make such one-shot measurements on 100 or 1,000 pairs of MZ twins and compute the within-twin correlation, it will surely under-estimate those twins' genetically-determined similarity in respect to their average happiness level, i.e., their *happiness set-point*. Customary twin data analysis uses these one-shot twin correlations and then attributes the unexplained variance to some combination of measurement error and *unshared environmental influences*. That is, it is assumed that the (presumably stable) trait is partially determined by genetic factors and also substantially determined by unshared experiences. The truth is, however, that the MZ twins', who correlate today only .50 on happiness, may correlate .90 in respect to the mean of 10 happiness measurements taken semi-monthly. That is, when retested over long periods, MZ Twin A's score at Time 1 may predict Twin B's score at Time 2 almost as well as it predicts Twin A's own score at Time 2. The "unshared environment" has not determined 50% of the variance in each twin's happiness set-point, because most of that variance is determined genetically. The unshared environment has determined merely the time-to-time ups and downs in the current but temporary happiness level. In the case of an emergenic trait, such as happiness, this between-twin, cross-time correlation is close to zero for DZ twins.

Romantic Infatuation

Tellegen and I have shown, with the help of the Registry twins, that romantic infatuation, which largely determines who marries whom, is a nearly random event, rather like imprinting in goslings, and that is not predictable from the characteristics of either the Chooser or the Chosen. We know that certain important and inter-related social attitudes, such as conservatism, traditionalism, religiosity, and authoritarianism, have very strong genetic roots. These findings, suggesting that romantic relationships begin rather adventitiously but (as shown below) persist or terminate under rather strong genetic control, were reviewed in an invited chapter (Lykken, 2002a). These are important contributions and I have again been lucky in my opportunities and in my colleagues, so as to have been able to play a part in making them.

Divorce

In 1992, Matt McGue and I published a paper reporting that risk for divorce is strongly rooted in genetic predilections. If your parents were divorced, the risk for your marriage increases by about 50%. If your spouse’s parents were divorced, that yields another 50% increase in your risk, independently of whether your parents divorced. If your fraternal twin has been divorced, your risk goes up about the same degree. But if your MZ twin has been divorced, then your risk goes up about 250%! McGue and I argued that these data obviously show that divorce risk is very strongly genetic in origin. Like crime, divorce is heterogeneous; people get divorced for many different reasons but most of them involve traits of temperament, traits like impulsiveness, aggression, danger-seeking, and so on—all traits that themselves have strong genetic roots.

But a clever radical environmentalist, like Leon Kamin or the late Stephen J. Gould, could easily generate a different post hoc interpretation. If I am having problems in my marriage, as everyone does from time to time, then if my parents had solved their problems by divorce, that option would seem more plausible and acceptable to me. If my DZ cotwin or sibling had gone through a divorce, then once again my resistance to marriage dissolution might be weakened. And everyone knows how close MZ twins tend to be, how they expect to react the way their cotwin does, to like the same movies or vacations, and so on. Surely it is not surprising that my inclination toward any given course of action will be considerably increased if my MZ cotwin has made a similar choice. Maybe Matt and I were premature in claiming we had demonstrated a genetic basis for risk for divorce?

However, since that paper was published, Tom Bouchard passed on to me the marital histories of the twins in his landmark study of twins separated in infancy and reared apart. These people were unacquainted with their biological parents or with their cotwins when they first married and when they first split.

| | <u>MZA Pairs</u> | <u>DZA Pairs</u> |
|--|------------------|------------------|
| N of pairs where both had married: | 50 | 45 |
| Pairs concordant for no divorce: | 30 | 19 |
| Pairs concordant for divorce | 10 | 5 |
| Pairs discordant for divorce: | 10 | 21 |
| RISK if cotwin is <u>not</u> divorced: | 10/(60+10)= 14% | 21/(38+21)=36% |
| RISK if cotwin <u>is</u> divorced: | 20/(20+10)= 66% | 10/(10+21)= 32% |

Among the 50 MZA pairs in that sample, where both twins had married, if the cotwin had never divorced, the risk for the target twin’s marriage was only about 14%. **But**, if the cotwin *had* divorced, then that risk leaps up to 66%! The corresponding data for the 45 pairs of DZA twins were 36% and 32%, a trivial difference and in the “wrong”

direction! These data don't agree with Kamin and Gould but they don't agree with McGue and Lykken either. These data seem to indicate that risk for divorce is an emergenic trait that it is strongly genetic but also configural so that it runs weakly if at all in families!

We can reconcile these results with those of McGue & Lykken, using a bit of the Kamin and Gould sort of post hoc reasoning. We can say that there is in fact considerable family influence on risk for divorce and that this explains the effect of parental or sibling divorce. Behavior geneticists have been insisting for some time that it is very hard to find *any* evidence for a significant effect of shared family environment. But if parental or sibling divorce raises one's own risk for divorce by 50%—and if this is unlikely to be due to shared genes, since divorce risk is emergenic—then that is assuredly a significant shared-family effect. It would be interesting to collect divorce data on adoptees. Suppose your divorce risk goes up substantially if your adoptive parents divorced while you are growing up, or if your unrelated, adoptive sibling gets divorced later on—that would clinch the argument for a strong shared-environment effect.

What would be left to explain is why the risk for MZA twin marriages is more strongly predictable from the fate of the MZA cotwin than it is for twins reared together. For MZ twins reared together, the risk goes from 12% to 45% when the cotwin divorces while, for MZA twins, it goes from 14% to 66%. But any social scientist with a Ph.D. ought to be able to rationalize those data. MZ twins reared together need to learn to differentiate themselves from one another, they develop from necessity the ability to see themselves as individuals, not bound to follow one another's path.

For example, we know of a pair of male MZTs who were so similar that it was uncanny. They were attending a small Lutheran college in Iowa during the Vietnam War when the parents received simultaneous letters from each of them. This was itself a surprise since they usually wrote a joint letter. The first missive said: "I've been thinking that I need to learn to get along on my own, so I've decided to join the Marines." The letter from the other twin said essentially the same thing! Once they discovered that they had made the same decision, the twins flipped a coin to see which one would wait a month or two so that they would not end up in the same Marine company. Most MZT twins do decide, sooner or later, to try to learn to emphasize their individuality. The divorce risk of MZT twins still goes way up if the cotwin divorces, but it doesn't go up as much as it would if they had never known they had a twin and if they had never practiced trying to be independent of or different from their cotwin. Q.E.D.

Happiness

When Tellegen and I published our paper on the genetics of happiness (*Psychological Science*, 1996), I inserted a sentence that I regretted as soon as I saw it in print. I said that, since the happiness set-point seems to be determined genetically, and the variations around that set-point are determined by the slings and arrow of outrageous fortune, then perhaps trying to be happier is like trying to be taller, and a waste of time and effort. The truth is, I believe, that one can bounce along usually above one's set-point of subjective

well-being. So I wrote a book to correct this mistake. It is called *Happiness* (New York, Golden Books, 1999). Unhappily, the publisher went bankrupt just as my book came out so it was never advertised and it has not yet (as of 8-1-02) been reviewed!

My surname in Norse means “the happiness” and I had hoped that my agent would find a publisher in Norway for this book so that the title page would read:

LYKKE
av
David Lykken

But it turns out that educated Scandinavians all read English so well that translation would not be worthwhile

The Causes of Crime and Parental Licensure

In doing the research for my 1995 book, *The Antisocial Personalities*, I discovered the magic number 70. It turns out that about 70% of incarcerated delinquents were reared without fathers. Similarly, fatherless rearing characterizes about 70% of adolescent runaways, school dropouts, teen-age pregnancies, and, I believe, most other examples of social pathology. I also discovered *Licensing Parents* by Jack Westman (1994), a child psychiatrist at the University of Wisconsin and the following propositions became apparent to me:

1. Children “reared by” biological parents who would never be qualified as adoptive parents by any reputable adoption agency are at high risk to be deprived of their birthright of life, liberty, and the pursuit of happiness;
2. Most social pathology results from the malfeasance of such parents who constitute fewer than 10 percent of biological parents;
3. Society at large has a moral obligation to attempt to rescue these children by removing them as early as possible from the custody of malfeasant parents;
4. Up until the so-called “sexual revolution” of the 1960s, a kind of parental licensure had been traditional in western society: because of the strong taboo against illegitimacy, most people believed that marriage was a prerequisite to parenthood and a license was required to get married;
5. Reasonable licensure requirements for prospective biological parents would include: maturity (e.g., age 21), marriage (acceptance of a legal commitment), economic self-sufficiency (no one has the right to expect me to finance their family except as a result of unforeseen misfortune), no disabling physical or mental defect (one should not accept a responsibility one cannot meet), and no conviction for a crime of violence;
6. A child born to any un-licensable mother should be removed from her custody at once and put up for permanent adoption; I would also empower the family-court judge to confine the prospective mother to a nursing home where she would receive adequate nutrition and medical care, if she was found to be at risk for

- substance abuse or venereal infection, and I would require the biological father to be identified and made liable for the costs of the confinement;
7. Both men and women who participate in a second unlicensed pregnancy should be required to submit to a long-term contraceptive implant.
 8. At least 90 percent of prospective parents would be able to obtain a license as easily as, say, a passport. Some licensable parents would turn out to be malfeasant but imperfectability characterizes all social measures. Parents who prove to be malfeasant once should lose subsequent licensability. This should include one or both parents who divorce while any child is less than 13 years old.

David Lykken's words and thoughts should occupy our minds constantly.

- Yvonne B. Moore, Burnsville, MN.

I began advocating these doctrines in 1995 in articles in *Law & Politics*, *Newsday*, *The Chronicle of Higher Education*, *Society*, *Child Psychiatry and Human Development*, and *Psychological Inquiry*. The *Law & Politics* article stimulated a letter to the editor, in the April, 1996 issue, the first line of which is quoted at the start of this section. I felt that what Ms. Moore had to say was both sensible and very well put. I outlined the parenting proposition in more detail in an invited chapter, "The Case for Parental Licensure", in a book on psychopathy and crime edited by Millon et al. (1998). More recently I was asked to submit an article to the *Journal of Personality* on a topic of my choice as part of their "Distinguished Contributor" series and, to the editor's obvious dismay, I submitted a different version of this same argument under the title, "The Causes and Costs of Crime and a Controversial Cure." This came out in 2000 with three essay critiques by Robert Sampson, Sandra Scarr, and Judith Harris, together with my response, and I have the author's usual conviction of having won the argument.

At the annual meetings of the American Psychological Association, in San Francisco in August, 2001, the APA was to give me their award for "Distinguished Scientific Contributions to the Applications of Psychology." Having had cardiac bypass surgery on 2-29-2000, I was not eager to attend yet another APA convention, so I skipped it but my award address, "Parental Licensure," was published in *The American Psychologist* on November, 2001.

Currently, I attend the occasional research meeting or seminar with my colleagues, write the occasional review, enjoy the successes of my offspring, genetic and academic, and take my bull terrier, Slick Willie, for shorter and shorter walks.

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