

A PROFESSIONAL AUTOBIOGRAPHY: Fortune Doesn't Matter But Good-Fortune Does

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On a winter's day in 1967, 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. I did make many mistakes along the way and my career as a psychologist has from the beginning depended heavily on chance, rather than sensible planning.

My first mistake was not being a girl. My mother gave birth to Margaret, Henry (Jr.), Bill, and Georg at two-year intervals, followed by the twins, Robert and Shirley. But Margaret died in her fifth year and Shirley was born with hydrocephalous and never left her cradle in her short life. So my appearance, nine years after the twins, was either a pure mistake (not mine!) or else a forlorn last-chance attempt for a daughter.

Dad, born in 1880, was the eldest son of a Norwegian immigrant whose father had sent him, aged 18, to America with instructions to earn enough to bring the rest of the family later. Gilman Løkka (he changed it later to Lykken, which means "the happiness" in Norse) reached Minnesota in 1872, worked in the Farmington area and then, incredibly, saved enough in one year to bring over his parents, four brothers, one sister, and three nephews! In 1879, when he was 25, Gilman married a Ella Thoreson, a Norwegian girl whose family had arrived earlier, and soon they were able to move further west, near Grafton, North Dakota, with their first-born, Henry, and to homestead a half-section of rich black soil. They tried at first to turn Henry into a Lutheran minister but he resisted and went instead to the University of North Dakota, where he perfected his English, studied engineering, joined the debate team, the literary society, and played center on the football team.

By 1905, Henry was City Engineer of Grand Forks, ND and he dined at a boarding house where, providentially, he met Frances Elizabeth Hamilton, a beautiful girl who'd come with her friend, Margaret Taylor (daughter of the *Old Taylor Whiskey* family) all the way from Frankfort, Kentucky to teach in a Grand Forks high school. Henry was smitten immediately while Frances was more cautious. But she did give him her postal address when she went back to Frankfort for the summer. That was lucky because her next job was in Deer Lodge, Montana, and Henry could pursue his courtship only by mail. He wrote her poetry and pleas while she remembered what a big, good-looking, shy fellow he was, and she finally said, "Yes", pending her father's approval. Henry promptly wrote to Papa Hamilton in Frankfort who gave his blessing whereupon, in the middle of winter, Henry swooped by horse-drawn sleigh into Deer Lodge and persuaded Frances to break her school contract and marry him there, then headed back to

Grand Forks, in December, 1911. I don't know who finished teaching her classes. Margaret was born nine months later.

In spite of my mistake as to gender, I was never treated as either an accident or a disappointment. My parents both were natural "alphas"—their children instinctively did what they were told, not fearfully but out of respect for obvious authority. I remember once, aged 13 or so, a friend of mine at school, outraged at an unfair demand of the teacher's, had protested so indignantly that the teacher gave in! So I made the mistake of trying that later with Mom, an indignant protest about one of her requirements. She looked at me briefly in mild surprise, then said, "Go to your room!"—and I went. But neither parent inquired into my affairs, asked who my friends were or what I did away from home. I think that led to my tendency to avoid seeking advice and always to rely on my own decisions.

One example was when I discovered death. It was Christmas, when I was maybe eight. My unmarried "Auntie Ann", mother's older sister who lived with us, had given me a model airplane kit, something more expensive than I thought she could afford. That night, in bed, feeling sentimental, it occurred to me suddenly that Auntie would die some day and then, horrified, I realized—*really* realized—that my whole family would inevitably die, sooner or later. I felt stricken, sobbed, and finally slept, exhausted. The next morning I still felt estranged by this awful awareness. I remember sitting quietly with some friends, thinking as they chattered how upset they would feel if they knew what I knew. I didn't speak of it, partly because of course everyone knew that people all must die—what *I* knew now was what that really meant and how one would feel when one's loved ones did die. I remained sort of unresponsive and privately depressed for several days but my parents never knew nor did I think of trying to tell them about it. Probably another mistake but I survived it.

My brothers all had been boy scouts so I too joined when I reached 12. But my troop met in the local Baptist church and the weekly meetings consisted mainly of singing hymns or reciting the Ten Commandments while standing at attention in a hollow square. I managed to get into trouble—I think it involved a childish fight with another boy—and I was required to bring my father to the next meeting where the scoutmaster and the troop's committee of elders, all Baptists, reviewed my sins and said that I would have to leave the troop. Dad and I walked home and, after a lengthy and nerve-wracking silence, he finally said: "Well, I don't think you need to worry about that."

My next mistake (although, as usual, my luck was with me in the end) occurred in 1943: I was 15, I'd never had a real date, much less a girlfriend, but I fell in love with Harriet Sarah Betts, 19 years old, a University student and the most attractive and popular girl I ever knew. The only reason we knew each other at all was that I lived at 4820 Sheridan while her home was 4923 Russell, one block away, and (this is the lucky part) the whole world was at war so that Harriet's many mannish suitors were almost all in uniform and (usually) somewhere else. That summer, when Paul Robeson came to town to play *Othello* at the Lyceum Theatre, I had nerve enough to ask Harriet to go with me to the Saturday matinee. When the streetcar brought us back to 50th and Russell, I was

feeling wonderful, but then I soon detected a large man sitting on Harriet's front steps. He was Warren Beeson, All-American center of Minnesota's champion football team, one of Harriet's admirers not yet in military uniform. Yet, although that date ended abruptly, I didn't give up hope.

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 enlisted in the navy on my 17th birthday in June and then, after boot camp, went to school to learn about electronics and radar.

Great Lakes Naval Training Station (boot camp) was interesting. We prospective radar technicians ("RTs") all ranked Seaman First-Class, a step higher than the other inductees. We had our own barracks and marched and trained together. I enjoyed the daily drilling, partly because the lowly second-class companies, when they marched near us, chanted: "Take down your service flag, Mother. Take down your stripes one, two, three. Your son is the scorn of the navy. Your son is a fucking RT!" I corresponded with Harriet regularly and I even started to write to the most attractive girl in my high school class, Joanne Edwards. But that ended embarrassingly after the second or third letter when she replied: "You must have liked that joke" (involving the 13 buttons on the pants of an enlisted man's dress uniform) "since you've told me it twice!"

Radar 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. Somehow I managed to acquire an illicit pass that enabled me to take the train home on weekends. One snowy Sunday afternoon, while Dad was driving me to the Milwaukee Depot, the Oldsmobile ran out of gas on the one-way road. Immediately, my big father, looking especially large in his woolen winter coat, was out of the car with his arms spread wide so that the next car behind had to stop. Then he smilingly explained to the intimidated driver that his son had to get back to the navy and needed to be taken to the depot. They made room for me in the back seat and, uncomplaining, delivered me to the train, although the depot was well out of their way.

(After the war, I borrowed my eldest brother, Henry's, brand new car to go out with a friend and two shady ladies on a Saturday night. Coming back from a Lake Minnetonka nightclub and just two blocks from the apartment of the girl whose folks were out of town, I managed to gently collide with another car, crunching Henry's grill and destroying the headlights (and ruining the date.) Sunday morning, when, through Dad's bedroom window, I showed him Henry's car parked out in front, he said: "Damn!" just once, then got in his car and followed me in the wounded car to Henry's house a mile away. It was Dad who went to the door and explained. I paid (gradually) for the damages, of course, but I sure appreciated the way he took charge. Dad always did the right thing.

Just two months after my enlistment in the navy, 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 gently jabbed my backside with penicillin.) Before being sent back to the 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 became 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, never having seen a ship or an ocean, 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 of Minnesota 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 Chemistry Building was crowded with real veterans and I found it both smelly and tiresome so I made sure to add other, more interesting courses. The ones I liked best were in Shakespeare (especially *Othello*, of course) and in Psychology.

I became increasingly devoted to Harriet, and saw her as often as possible. In fact, not long after my 18th birthday, as we sat by the lake, after which Harriet was named, on a lovely summer evening, the conversation lagged, and then I risked everything: “Harriet, I’m afraid I’ve fallen in love with you.” (pause) “I love you too, David!” Later, returning home from Russell to Sheridan, I distinctly remember that I sort of floated.

In my junior year I signed up for Professor 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 (I think he was gay) 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. Being now a college graduate, I promptly got a job with the Minneapolis Sewer Department operating a jackhammer. Harriet, by then *my* girlfriend, was a social worker for Hennepin County and she told me that the 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. (What if I hadn’t happened to take that RT exam and had enrolled in the University instead of the navy on my 17th birthday? Very lucky indeed!)

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 Professor Heron. I then obtained 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. In 1951, with David Premack and Bernie Arronson, I moved into an apartment just off campus. We rotated the jobs of cook, dishwasher, and house cleaner on a weekly basis. The two Davids cooked fairly well but Bernie was awful. When we invited the famous philosopher of science, Herbert Feigl, for dinner, Bernie was limited to setting the table.

My plan for a dissertation was to test a hypothesis about the psychopathic personality that 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 Psychology, 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 in the office of the minister of the Unitarian Church, our friends Jack and Cullie Mellor being the only attendants. After a modest reception with both families crowded into the Betts’s living room, we left for our newly rented apartment, with a bottle of champagne Georg had given us. Half way there, a tire went flat, so I got out the jack, opened the champagne, changed the tire, and we started our weekend honeymoon in an unusual, but very happy, way. Once again, on Monday, Harriet did social work with unmarried mothers while I worked on my degree. Our belated real honeymoon was a week’s canoe trip in August on wild Rainy Lake.

Sometime in late 1953 I noticed in the Psychology Department office several folders containing application forms for National Science Foundation 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 choice 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 at all 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 students from the various sciences 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 department called the Central Intelligence Agency. 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 Avenue 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. During the voyage we got acquainted with an agreeable young man from the South, the Reverend Dennis Kinwall, who also had won a fellowship, his to study in Scotland for the Anglican priesthood; we would see Dennis again. Some eight days later, we landed in Liverpool where, when the customs officer asked her nationality, Harriet answered "German," a mistake that was common for Americans in those days. We 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.

We soon managed to rent a nice furnished apartment (no central heat) at 22B Earl's Court Square and Harriet studied the then very different ways in which fresh meats and

produce were displayed in the shops (no supermarkets!), while I got acquainted with Eysenck and his staff and began to write up my dissertation. Harriet had sold her car just before we sailed for England and we discovered that the proceeds were nearly enough to pay for a new little two-seater Triumph TR2 roadster, if we picked it up at the factory in Coventry. Driving it back to London was memorable, on the left side of the road for the first time, the on-coming drivers angrily honking—I learned later that the Brits had not yet learned to make headlights that did not seriously flare in the eyes of oncomers. I thought the engine seemed hot and when I checked the next day I found that the oil level was zero! The company's engineers happened to be attending an auto show then, not far from our apartment, and they obligingly closed the oil drain valve, filled up with new oil, and proudly pointed out to me that "That's a good little engine that could travel all that way without oil!"

One mistake Harriet and I collaborated on (it turned out all right in the end) was in accidentally getting pregnant at that awkward time when we were planning our trip abroad. So, after finding our apartment, I called University College Hospital, thought to be the best obstetrical hospital in England. This was Grantley Dick-Reed'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, to my surprise, I was given a gown and mask and invited in to be in charge of hand-holding and trying to determine the baby's gender ("One more push, Hon."). 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. Pancras. Later that spring, Dennis visited from Scotland, filled a small bottle with incipient holy water, then went with us to a little chapel in Westminster Cathedral where he christened Jesse. We returned the favor by driving up to Edinburgh that summer to attend his wedding in St. Gile's Cathedral; I served as an usher. Denis had fallen in love with a pretty Scottish girl and I hope they were as happy as we were.

The Carry Cot

We got Jesse a bed, called a "Carry Cot", which sat upon a rather rickety stand that was easy to agitate back and forth with one's hand and we quickly found that such movement was soothing, that it prevented the usual twitches and dysrhythmic breathing that Jesse showed when first put down to sleep. The cot had cloth handles on each side that could be gripped in one hand and the whole cot swung, as on a pendulum, in a head-to-toe motion. This more vigorous stimulus feeding was effective on those occasions when mere jiggling of the cot stand proved inadequate. The head-to-toe motion prevented Jesse from rolling from side to side and it never failed to lull him into sleep, however fretful he might have been to start with.

The infant nervous system, although it is responsible for all the vegetative functions essential to life from breathing to digestion, is incomplete at birth. The nerve fibers of an adult are sheathed with *myelin*, an insulating coating that, in effect, prevents short-circuiting of the nerve impulses. Myelination is incomplete during the first few months of infant life. Erratic nervous functioning, interruptions and dyssynergies, can disturb or prevent sleep, cause gastric upset and other discomfort, and can even interrupt breathing. Such glitches in the smooth rhythmic functioning of the infant nervous system are probably responsible for some instances of Sudden Infant Death Syndrome (SIDS).

When the heartbeat or the breathing stops, normal function can sometimes be restarted by external stimulation. Even the adult nervous system often seems to benefit from external "stimulus feeding", especially in the form of rhythmic stimulation, most especially rhythmic *motion*. Anyone who has slept soundly on a train or ship appreciates this truth. Every parent knows that baby is likely to sleep happily in the baby carriage or the car seat, lulled by the motion of travel. The fetus experiences similar rhythmic stimulation as its mother walks and breaths. Perhaps stimulus feeding, especially the stimuli that are imparted by passive movements of the body, is a natural anodyne for infant distress. Anthropologist friends told me that providing stimulus feeding for babies is a universal practice among traditional societies—infants sleep with their mothers, are carried on mother's chest, back, or hip, hung from branches to sway in the breeze, handled and dandled by relatives in the evenings—a practice that probably evolved to compensate for the delayed maturation of the infant human nervous system. Motor development is so much faster in the other primates that "monkey cradles" are not necessary; infant apes and monkeys cling to mother's fur and share her motion almost from the moment of birth.

Watching little Jesse, at two months, all fed and dry and tucked on his tummy in his Carry Cot, one could actually observe the periodic misfires of his nervous machinery; he would twitch, sometimes actually "jump" as if startled. After a period of regular breathing, the next inspiration would sometimes be delayed and then come with a gasp and his eyes would open in alarm. More often than not, although he was obviously sleepy when first put down, there would be a crescendo of twitches and starts leading to a period of active crying. On the one occasion when we left him for three hours with a reluctant baby sitter, we came home to find him exhausted, sobbing uncontrollably and damp with perspiration. The foolish woman had left him in his stationary bed to "cry it out." We never left a child with a stranger after that.

We did quite a bit of traveling in our TR2 during the next few months, visiting points of interest in England and Scotland and staying nights in local inns. I have at least one vivid memory of swinging Jesse vigorously up and back on the landing outside our small room in one such inn, as several parties of bemused but polite British guests made their way down to dinner, trying not to notice the outlandish American.

Our second son, Joseph, exhibited classic infant colic during his first few months. His face grew red, his little legs strained up against his hot, hard abdomen, while he screamed bloody murder, usually between 2 and 5 A.M.---and Joey had the loudest voice I've ever heard in a child. Fortunately, by then, I had asked a local sheet metal shop to build me a sturdy rocker consisting of a frame bent out of pipe with a metal pan suspended at the corners by four arms some 20 inches long with bearings at both ends. The baby's cot fit snugly on the pan which could then be swung back and forth by hand in a head-to-toe movement, the vigor of the swings being determined by the degree of Joey's obvious distress.

It was like a miracle! By the second or third surging swing the screaming would stop, the little eyes would open as if in wonder, then the legs and body would gradually relax. In a minute or two, Joey would be stretched out prone again, the eyelids would flutter and close, and the swinging could be tapered off to a stop. It never failed! Think how lucky we were that Jesse's Carry Cot had been so rickety!

Back to Minnesota

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, my luck ensuring its intact arrival. 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.

In August of 1955, we left our roadster to be shipped to New York in a freighter and then, three weeks later, set off ourselves in the S.S. Flandre, a pleasant French vessel. When we docked, however, I learned to my dismay that our TR2 had not yet landed! Somehow we managed to get our luggage, Jesse's baby buggy, and ourselves into a (propeller driven) transport plane bound for Minneapolis. When at last the taxi delivered us to my parent's home, I gave the driver literally my last cent of cash.

We quickly set about seeking a house we could afford, grimly turning down one after another until, like a dawn after several dark days, we visited a lovely old frame house at 3139 E. Calhoun Boulevard, actually fronting on Lake Calhoun, with living room, sitting room, study, three bedrooms, garage, all for only \$15,000! My father gave me \$1,000 for the down payment and I soon had a workshop going in the basement from which to make furniture and various minor construction projects. Harriet embarked on full-time motherhood. Little Joseph arrived 18 months later and Matthew two years after that and, before long it seemed, they were all three coming home for lunch from the good grade school just three blocks away.

During the second year 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. Just as our second son, Joseph, was born, in 1957, I became a tenure-track assistant professor of psychiatry (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 publication¹ was a paper that was widely ignored although it was in a good journal. It demonstrated that, in psychology, certain patterns of predictor variables could 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.² The idea still did not catch on and I forgot about my methods of "actuarial pattern analysis" myself once that second paper was published. Having just read it over again, it still seems like a good idea and, in fact, I managed to think of a way to use it again, some 40 years later.

¹ A method of actuarial pattern analysis. *Psychological Bulletin*, 1956

² Psychological prediction from actuarial tables." *Journal of Clinical Psychology*, 1963

The Psychopathic Personality

My dissertation undertook to test whether prison inmates who met the criteria for primary psychopathy (expounded by Hervey Cleckley, M.D. in the several editions of his classic, *The Mask of Sanity*) 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 would 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. I asked the appropriate personnel of the three Minnesota prisons to consider all the inmates they had already classified as “psychopaths” using their usual vague clinical standards, and to rank them using Cleckley’s criteria. The most highly ranked formed my Primary Psychopaths group while the most lowly were the Secondary Psychopaths. My dissertation showed that, compared to the secondary psychopaths and to normal controls, primary psychopaths:

- (1) Gave weaker electrodermal responses to buzzer-warnings of impending painful shock,
- (2) Learned a complex mental maze as quickly as the controls but failed to learn to avoid those errors that produced painful shocks,
- (3) 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 sensor for measuring electrodermal changes due to palmar sweating, 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 Auke Tellegen as the Harm Avoidance scale of his personality inventory, the Multidimensional Personality Questionnaire); and
- (3) Construct out of pinball-machine components a 20-step, 4-choice mental maze labeled 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.³ One virtue of this study was its test of three predictions whose joint confirmation gave somewhat stronger than usual support to the original hypothesis.

³ The chance that this confirmation will be *statistically significant* is, of course, much lower than 50%.

The published version of my thesis⁴ 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.⁵ 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 does have useful validity. Oddly enough, this is the argument of my first publication on actuarial prediction, mentioned above. Therefore, 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*⁶ 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".⁷ 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 co-opt 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 work—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)

⁴ A study of anxiety in the sociopathic personality, *Journal of Abnormal and Social Psychology*, 1957

⁵ Predicting violence in the violent society. *Applied and Preventive Psychology*, 2, (1993) 13-20.

⁶ Lykken, D.T. (1995). *The Antisocial Personalities*. Mahwah, NJ: Lawrence Erlbaum Assoc.

⁷ Hans Toch, in *Aggressive Behavior*, 1997

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, in traditional societies, children are socialized in a manner more like that employed by our ancestors in their “environment of evolutionary adaptation,” in which the extended family plus other members of the local tribe all freely participated. 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, Prof. Jim Jenkins in Psychology 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 who was then giving a series of lectures at the University and being psychoanalyzed by Paul Meehl. We 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 also was on leave.

Other psychologists at the Center that year included Jerry Blum, Tony and Diana Deutsch, Howard Hunt, Bill Kessen, George and Jean Mandler, Gardner Lindzey for part of the time, and Karl Pribram (almost a psychologist). Another colleague, Dutch psychologist Adrian de Groot, an expert on the psychology of chess and a chess master himself, simultaneously played and defeated 20 chess duffers like myself. He was not allowed even to see one chess board presided over by two of the Center Fellows who

thought themselves to be relatively accomplished players. Well into the game, after these two announced the next move they had decided on, de Groot pointed out that their proposed move was impossible! Although they had the chess pieces arrayed before them while he had only his mental image to rely on, they got it wrong while he got it right. De Groot himself had played—and been easily beaten—by the future grand master, Bobby Fisher, when Fisher was a boy of twelve. De Groot was careful to point out, however, that even by that early age Fisher had played many thousands of chess games and had derived from this experience a vast mental armamentarium of chess positions and strategy.

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—after some thirty years of doing twin research—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. Two, to me deeply mysterious, papers in the Proceedings of the National Academy of Sciences⁸ indicate that even theoretical physicists now recognize emergent phenomena and admit that their Theory of Everything has limits.

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, in the idea of *emergence*, 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, then on Hilton Head,

⁸ R. Laughlin, D. Pines, et al., PNAS, 97, pp.28-31 & 32-37.

and finally on the Gunflint Trail, on the Canadian Border north of Lake Superior. This last was an important trip for us because it led to our finding the log cabin on Gull Lake at the north end of the Trail, where Harriet and the boys spent most of the summers over the next several years, while I visited on weekends. Those exchange visits tailed off when the Kessen triplets arrived and their lives became more complicated.

Those Gunflint summers were especially delightful. We purchased our cabin from Willard Watters, one of the most remarkable men I ever met. He'd come up to the area on an old motorcycle from Iowa in about 1930, aged 19, and worked, summers building resort cabins for a local entrepreneur, and winters running a trap line to catch beaver and other furry critters. Willard saved enough to buy his own plot of shoreline on Gull Lake, later listed on the maps as "Watters Point", and that winter he began cutting black spruce logs, which he hauled home across the ice of the lake. Then, in the spring, he removed the bark from those straight logs and constructed, single-handedly, our handsome cabin, with a basement and the first indoor bath and toilet in the area. During WWII, Willard worked at a defense plant in Minneapolis and learned to fly so that he could bring a two-passenger float plane back with him to the Gunflint and make money giving rides in summer and, in winter, tending a distant trap line near Lake Saganaga, accessible only by his airplane, which he then equipped with skis instead of floats. He built four more cabins, sold three to tourists like us, and moved with his wife and two sons into the fourth, leaving the first and best for us to buy.

Willard's two boys were the ages of our sons for whom they provided a healthy and interesting peer-group change each summer for the next several years. With Willard near by, I never had to worry about my family during the times I had to be in Minneapolis. When I was able to join my family, Willard could always show me how to do or fix whatever needed doing or fixing. When I needed two logs to stabilize a workshop on my property, Willard, who was maybe 5'8", put one on his shoulder and headed off while my brother, Georg, and I, both about 6'2", managed painfully to carry the other, one on each end. Willard began exploring Florida in the winters and found a piece of scrubland through which he thought the state must soon build a highway. He offered me a chance to share in his investment but I had neither the money nor, foolishly, the faith. The highway was built and, after several more careful and clever investments, Willard retired a millionaire. Sometime in his late eighties, Willard decided that his time was up; he walked out into the back yard of his Florida home, put a pistol barrel in his mouth, and pulled the trigger.

When we got home from the Center and California in 1960, 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 grungy mess from top to bottom (banana peel under the rug, empty cans under the couch, dirty dishes on the shelves, we couldn't believe it!) 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

scent of wild onion in the air.” Harriet said, “I knew some idiot had been standing in my chives!” The wild onions disappeared from the later editions.

Three years later, I made another mistake. Now a tenured Associate Professor of Psychiatry and Psychology, I thought we ought to look for a larger and fancier house. One of my graduate students gladly paid my asking price for the good one we were leaving and that provided the down payment on the one I picked out in a posh area near Lake of the Isles. It had numerous bedrooms and baths, a bar in the basement with room for an elegant billiard table I had my eye on. The small back yard of our “new” house was a disaster area. I fenced it in, dug it all up, laid out a brick patio, and then, on impulse, planted some vegetables in the now rich-looking dirt, tomatoes and corn, squash and onions, lettuce and green peppers. We thought the kids would enjoy watching developments but we enjoyed them even more. We left that house within two years because it proved to be in an upper-Yuppy, cocktail-party neighborhood where children were alternately ignored and indulged and not the kind of peer group we wanted for our sons. The last straw was when a neighbor kid threw an ice scraper at four year-old Matt’s head, causing profuse bleeding, and the kid’s mother, a witness, told Harriet: “Boys will be boys!” So we moved again, this time to a house Harriet selected and where we lived on happily for the next 40 years. The only positive I could reasonably claim from my expensive mistake was my discovery of the joys of gardening.

Our first house having fronted on Lake Calhoun, our second just a block from Lake of the Isles, and our third stood on a corner lot just two short blocks from Lake Harriet in south Minneapolis. This house’s sunny side yard seemed an ideal cite for another vegetable garden! Using 2 by 8-inch boards, I constructed seven 4 by 8-foot rectangles for raised beds with narrow paths between. During one rather strenuous week, I dug up all the dirt within each rectangle, then filled each one to the top with rich black dirt hauled from a truckload I had ordered dumped nearby. Those beds continued to do their summer’s job, year after year, reinvigorated every spring with cow manure. We grew strawberries, raspberries, gooseberries, and currants, several varieties of tomatoes and lettuce, snap peas and cucumbers, green beans and carrots, sweet and hot peppers, red cabbage and broccoli, onions, squash, and herbs and catnip for the cats. We always had several Brussels sprouts plants that were left until Thanksgiving when they were cut down with an ax and the frozen sprouts picked off to be part of that family feast. And now all three of our sons know and practice the pleasures of growing one’s own tomatoes.

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 be entertaining; 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 ward. It was there I met Donna, a tall, slim 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 lovely, 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.⁹ 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. However, the progressive desensitization that later cured my stage fright problem (see below), worked equally well with Ralph.

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

⁹ See for example, DF Klein & JG Rabkin (Eds.), *Anxiety: New research and changing concepts*. New York: Raven Press, 1981.

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 saw them together again and 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, then 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 nervously 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 no longer had any idea of what I had meant to illustrate by 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-minus 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 Peter Venables in England published an elegant-looking experiment¹⁰ in which he found a positive correlation (R) between two physiological variables in normals, but a negative R in schizophrenics. We knew from our own prior work that the true correlation in normals is the opposite of Venables' finding, so my student, Mike Maley, and I undertook a constructive replication.¹¹ The Veterans Administration 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-schizophrenic, non-psychotic patients as controls. The latter group gave the negative correlation that we expected but the schizophrenics showed little relationship between the two variables.

I generated what I still think is a reasonable explanation for our data, which differed entirely from Venables'. 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 problem with *stage fright!* Venables was then Chair of Psychology 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 *London 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. Advertised as a course in the art of public speaking for young business people, all my classmates were there for the same reason I was, to moderate their innate stage fright. The Carnegie formula was straightforward progressive desensitization and it works! When I went to sleep on the tube en route to about the sixth weekly lesson, I realized that I was making good progress. In that lesson, we had to get up in front and give an impromptu talk on a subject selected at the last moment by the instructor and, to my relief, I had no problem with that.

¹⁰ Venables, P. (1963). *Journal of Psychiatric Research*.

¹¹ Lykken, D.T. & Maley, M. (1968). *Autonomic versus cortical arousal in schizophrenics and non-psychotics*. *Journal of Psychiatric Research*, 6, 21-32.

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 (and later paced for an hour or two at home, trying to win them back.) When I returned to Minnesota, I got a request from Edwin Boring, who had founded *Contemporary Psychology*, a journal of book reviews, and was still its 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 review¹² 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, testifying before committees of Congress or as an expert witness in courtrooms all over the U.S. and Canada, 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.

That second year in London meant a lot more than my skirmish with stage fright. We had driven to New York in a new Ford Thunderbird sedan, which we then brought with us on the *SS France*. Upon arriving in Southampton, we drove (now on the left) to London where, in spite of having no prior arrangements, we managed to find, rent, and move into a charming house before nightfall! We got the boys into schools that they liked and, during the holidays, we took some delightful trips. We visited Lisbon and Granada, drove our car through France, the Low Countries, Switzerland, and from Venice to Rome. Coming back from Rome, wanting some lunch, I turned off the autostrada toward a town called Firenze and we were then delighted to discover that we'd come in fact to Florence. On another occasion we spent a week floating down the Rhine and sampling its wines. Our most exciting trip involved flying from London to Kenya, where an ancient little DC3 flew us from Nairobi on to Mombasa on the coast of the Indian Ocean. After a few days acclimating in a quiet resort, we joined some English tourists in three eight-passenger VW minibuses for a fascinating week touring the game parks, the Serengeti plain and the fabulous Ngorongoro crater, home of most of those African animals you've heard of, plus many that you haven't.

¹² Lykken, D.T. (1959). *Contemporary Psychology*, 4, 377-379.

In August of 1969, we sailed happily home in the *Queen Elizabeth* (how lucky we were to make these trips before jet planes replaced those enjoyable ocean liners!) After our drive back to Minneapolis, we found our Lake Harriet house, which we had rented to a law professor, to be clean and neat and even the garden in tolerable condition.

Psychophysiological Research

My thesis research got me interested in psychophysiology (the study of physiological reactions that signal psychological events or states) and especially in the problems of measuring the skin conductance response or SCR, changes in the electrical conductance of the skin of the palms and soles. These changes are due to sweat gland activity, which, in these thick-skinned regions, has the purpose of increasing tactile sensitivity and making these grasping surfaces less dry and slippery. In 1959, I published a study of different types of electrodes used in SCR 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 SCR.

One methodological contribution that I think was useful was the concept of *range correction*.¹³ The range over which a subject's measured skin conductance will vary is a function of the electrode area and of the nature of the skin and the density and activation levels of the sweat glands in that area, i.e., sources of variance having no psychological significance. Therefore one should partial out this extraneous variance by obtaining estimates of each subject's maximum and minimum skin conductance level (SCL), and then to express his experimental level as a proportion of his individual range of variation. In the case of the wave-like SCRs, where the minimum is always zero, one obtains an estimate of the largest SCR the subject is capable of producing and then expresses his SCR to the experimental stimuli as a fraction of that maximum. Like most of my contributions to this immature science, this one was pretty obvious.

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*¹⁴ 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

¹³ Lykken, D.T., Rose, R., Luther, B., & Maley, M. (1966). Correcting psychophysiological measurements for individual differences in range. *Psychological Bulletin*, 66, 481-484.

¹⁴ Lykken, D.T. (1971). Square-wave analysis of skin impedance. *Psychophysiology*, 7, 262-275.

SCR (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,¹⁵ which was designated as a *Citation Classic* by the Institute for Scientific Information in 1990, outlined the method of measuring and interpreting the SCR that has also become the standard in the field.

I'm proud to say that my former student, Bill Iacono, now a McKnight Distinguished Professor of Psychology at Minnesota, 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 Prof. Don Fowles, from the University of Iowa, 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).

The Education of Our Children

Our eldest son, Jesse, had a lively time at Washburn High School, on the debate and speech teams, editing the *Poplars* magazine and the editorial page of the *Grist*, serving as Student Council President, and meeting Veneta, his (eventual) wife-to-be. Jesse then enrolled at the University of Minnesota, but he also started as a stage-hand/extra at the Tyrone Guthrie Theatre where he soon became entranced by dreams of a stage career. A year or three were wasted, at least in my stodgy, paternalistic view, acting in various dinner theaters, but then he settled down and completed, not only his BA in political science, plus three years of graduate work in criminal justice studies, but also his JD at Hamlin Law School, this last not because he wanted to be a lawyer but just to show me that he could. He is now an executive in his county's corrections department.

Joseph's 9th grade science teacher told us that he'd taught Joe everything he could and that we should seek some more advanced opportunities. Joe had a paper route at that time and, as luck would have it, the *Star Tribune* offered its carriers a Saturday meeting with representatives of two swank eastern boarding schools that were apparently seeking to broaden the socio-economic range of their student bodies. The school that appealed most to Joe was *Phillips Exeter Academy*, President Kennedy's alma matter, in Exeter, New Hampshire and *Exeter* was happy to accept him. We were not poor enough to qualify for one of the newspaper's scholarships but those classrooms furnished with Harkness tables, with seats for just 12 students plus the instructor, were irresistible. At the end of Joe's second or junior year, *Exeter* told him that he was ready for college and they actually arranged for him to be interviewed (and accepted) at Princeton on his way home. His mother said, however, that she wanted him home for a while; he could get his BS in physics right here at Minnesota and then go do his graduate work wherever he liked. And that's what Joe did, three years of home at Minnesota, then on to MIT and his

¹⁵ Lykken, D.T. & Venables, P. (1971). Direct measurement of skin conductance: A proposal for standardization. *Psychophysiology* 8, 656-672

PhD and string theory. Joe is now a theoretical physicist at the Fermi National Accelerator Laboratory and Chair of the Division of Particles and Fields of the American Physics Society.

Because he was a Merit Scholar, Matthew got numerous invitations to matriculate at various colleges and they included an invitation to apply to this unknown junior college in California where the tuition, board, and room were free. Our first thought was that this must be some sort of cult endeavor, an Ayn Rand training program possibly, but when we looked at the library in *American Colleges and Universities*, there was Deep Springs College, right next to Dartmouth. But the median ACE or SAT scores of its freshman class were higher than Dartmouth's, higher than Harvard's or MIT's, or any other college in the book. The centerpiece of the application form consisted of ten challenging essay questions and, because he had put off applying to the last minute, Matt had several busy days. His efforts were successful and Matt soon set off, still only 16 years old, for two of the most memorable years of his life.

Deep Springs really is a cattle ranch as well as a college. It is also a farm, growing hay for the cattle and vegetables for the Boarding House, where the 20 students and 10 staff have their meals family style. In addition to herding the cattle and working in the fields, students help in the kitchen, run the library, milk the dairy cows, care for the pigs and the horses, do needed mechanical or other repairs. The half-dozen faculty members live in modest houses on the ranch and provide a high-level liberal arts curriculum. Class sizes are typically five or fewer and teaching is mainly tutorial. The school year consists of six 7-week terms from late June through the next May. There is a month's holiday around Christmas and another two-week break in June. At the end of these two interesting years, Deep Springers can be virtually certain of admission to the college of their choice. Matt finished up at Minnesota, married a girl met through a Deep Springs classmate, honey-mooned all the way around the world, then off to Harvard Law. He is currently Vice-President (Tax Law) of Baxter Pharmaceuticals.

Polygraphic Interrogation

Again adventitiously, I became interested in polygraphic interrogation. I had given mock lie detector tests to the prison inmates in my thesis research, just to interest them and ensure that those invited would participate. Then, in 1958, two medical students, assigned to me that summer as research assistants, 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 through 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.¹⁶ No innocent subject was misclassified and nearly all the guilty ones 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 1960¹⁷, 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 occupied some 25% of my professional life for the next 30 years.

In 1974, I published "Psychology and the lie detector industry" in the *American Psychologist*, the house organ of the American Psychological Association (APA)¹⁸. 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 in state, federal, and military courts in most of the 50 states (including Alaska twice but not, alas, Hawaii), and also in several provinces of Canada. 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 Sam that he would drop the charges if 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

¹⁶ Lykken, D.T. (1959). The GSR in the detection of guilt. *Journal of Applied Psychology*,

¹⁷ Lykken, D.T. (1960). The validity of the guilty knowledge technique: The effects of faking. *Journal of Applied Psychology*, 44, 258-262.

¹⁸ Lykken, D.T. (1974). Psychology and the lie detector industry. *American Psychologist*, 29, 725-739.

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, handsome 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 private 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 Steinman, 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 30 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 and 2000 *Encyclopedias 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.¹⁹ 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).

I took a sabbatical year in 1980-81 and Harriet and I spent a delightful autumn at Deep Springs College where I taught an introduction to psychology in exchange for board and room. In that relaxing environment, I began my first book, *A Tremor in the Blood: Uses and Abuses of the Lie Detector* (McGraw-Hill). *Tremor* 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. By this time, not even polygraphers were claiming that there is a specific lie response and they depended instead (as they still do) on the *Control Question Test* or

¹⁹ Lykken, D.T. (1981). To tell the truth. *Discover* (p. 10), February.

CQT. This involves two or three Relevant or “did you do it?” questions, together with “control” questions such as: “Prior to last year, did you ever tell a lie to a person in authority?” or “Have you ever taken something of value that didn’t belong to you?” The examiner “explains” that these questions are to determine whether the suspect “is the sort of person” who might be guilty of the crime referred to by the Relevant questions. If the suspect shows greater physiological disturbance to the Relevant than to the “control” question, he is diagnosed as deceptive.

A genuine control question, in the scientific sense of that term, would be one expected to have the same impact as the Relevant questions if, in fact, the subject can answer them both truthfully. For example, if you are accused of sexually abusing your neighbors’ small child, then a reasonable control question might involve asking if you also abused another child, having previously led you to believe that another neighbor had also brought charges against you. If you were much more disturbed by the Relevant question than by this one where your “No” answer is known to be true, then your “No” to the Relevant question might reasonably be diagnosed as deceptive.

But such genuine control questions are not used (professional criminals would soon learn that the second “charge” was a fake.). Here is a typical example of the “control” question test that is still in wide use. A nurse in Yakima, Washington, was charged by the new wife of her ex-husband of sexually abusing her own 4 year-old son. Because there was no real evidence of abuse, the district attorney offered her the same deal that Sam had: “If you can pass a polygraph test, we’ll forget this thing but you have to agree in advance that, should you fail the test, we can use that result in evidence against you.” Frantic to be freed of this outrageous allegation, she agreed---and failed the test. Called to testify for the defense, I thumbtacked the defendant’s polygraph chart to an easel in front of the jury box together with the numbered list of the questions she had been asked. I showed the jurors how the examiner had marked the chart in pen where he had asked the numbered questions and that most of them were followed by changes in the nurse’s blood pressure, in her breathing patterns, and in the sweating of her palms.

I showed them where the examiner had asked her his “control” question: “Have you ever committed an unusual sex act?” and there was definite physiological reaction to that question. But, where he asked, “On the date of May 14th, did you take Johnny’s penis in your mouth?” that was followed by a much larger reaction. The nurse was clearly more bothered by the accusation that she took her little boy’s penis in her mouth than she was by the question about “unusual sex acts.” And I explained that this was why—the *only* reason why—the examiner concluded she was lying about abusing little Johnny. The jurors’ eyes widened at this and they quickly brought back a verdict of not guilty.

From 1959 to 2000, I explained the invalidity of the lie detector in two books and more than 40 articles, invited chapters or editorials, even the entries for *Polygraph Tests* in two encyclopedias. 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.²⁰

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

²⁰ Lykken, D.T. (1991). Science, lies, and controversy: An epitaph for the polygraph.

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 had use for the money. Bill began getting frequent calls to testify at Daubert hearings, where he consistently 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 four-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.²¹ (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 Raskals’ own words. Any competent psychologist could evaluate the plausibility of these methods or the quality of

²¹ Iacono, W.G., & Lykken, D.T. (1997). In D.L. Faigman, et al, (Eds.) *Modern scientific evidence: The law and science of expert testimony*. St. Paul, MN: West Publishing Co.

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.

We old folks complain a lot about our failing memories but, as a Positive Psychologist, I try to look on the bright side. The fact is that I can now read books I've read before with even greater enjoyment, because while I remember almost nothing about the plot or characters, I know that I will enjoy this read because I remember enjoying it before. One example is that, in the first edition of *Tremor*, I tried to illustrate how my Guilty Knowledge Test could be used in police work by writing a 39-page detective story entitled *The Body on the Stairs*. I left that out of the second edition (I can't remember why) but I recently came across the text and started to read it. Fascinating! I couldn't remember how it came out (my own story!) and I really enjoyed it!

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. In fact, I just received from Takehiko Yamamura, PhD, a copy of his new text on polygraphy in Japan, all written in Japanese but with my photograph in the introductory chapter. Moreover, 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.

There is no credible scientific evidence indicating that failing the lie detector indicates deception. There *is* good evidence, however, that guilty suspects can *pass* the lie test if they augment their physiological reactions to the "control" questions by

covertly biting their tongue or clenching their toes after answering. Yet our federal government—the FBI, CIA, NSA, the military services, the new anti-terrorism agencies—employ hundreds of polygraph examiners and now, alas, this pseudoscience has spread throughout Canada and most of Europe. It is especially discouraging when, not just the uneducated, but the actual leaders of government continue to believe in disproven mythologies.

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 terminates 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 paper²² 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.²³

A similar study with human subjects appeared in 1972 (showing that I needn’t have mistreated those poor rats 10 years earlier) and another paper on this topic was published in 1974.²⁴ 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

²² Lykken, D.T. (1959). *Psychophysiological Measurements Newsletter*, 5, 2-7.

²³ Lykken, D.T. (1962). *Preception in the rat: Autonomic response to shock as a function of length of the warning interval. Science*, 137, 665-666.

²⁴ Lykken, D.T., Macindoe, I., & Tellegen, A. (1972). Preception: Autonomic response to shock as a function of predictability in time and locus. *Psychophysiology*, 9, 318-333.

“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 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*.²⁵ 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?"²⁶ 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, and length of hospitalization 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, a psychoanalyst who was then Director of the Psychiatry Department's research unit, generously agreed to take me on as a psychoanalysand 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

²⁵ Lykken, D.T. (1968). Statistical significance in psychological research. *Psychological Bulletin*, 70.

²⁶ Lykken, D.T. (1991). What's wrong with Psychology anyway? In D. Chicchetti & W. Grove (Eds.), *Thinking Clearly About Psychology. Vol. 1*. Minneapolis: University of Minnesota Press. pp. 3-39.

that we had a friendly collegial relationship outside of the analytic hour. One thing I did get 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*, 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. Having been a happy, wonderfully effective full-time mother for a dozen years, Harriet was ready to start applying her abilities to other problems (although her boys were never "problems.") She was a member of the original Steering Committee of the Minnesota Conference of Concerned Democrats. In October 1967 the urgings of this group precipitated Senator Eugene McCarthy's decision to run for the presidency, as an antiwar alternative to incumbent president Lyndon Johnson. As DFL chairwoman of the 13th Ward in Minneapolis, Harriet oversaw a record turnout for the March 1968 ward caucus, and the election of a unified slate of antiwar delegates to the state DFL convention, herself among them.

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-plus% 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 the eminent psycholinguist and social activist, Noam Chomsky, and to participate with him and my family in an enormous and generally peaceful anti-war march, which ended outside the American Embassy. A three-deep rank of (unarmed) London bobbies held 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 a dumb 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 Union, 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, unnoticed by me, he left but returned with another plain-clothed cop, 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 about 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 in. 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 armed police 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 burley 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 and dark recesses of the basement. These were confiscated together with the spirits found within the closed first-floor liquor cabinet. My son, Matthew, aged 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 marijuana 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 fingerprinted 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 Harriet 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 Harriet 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. (Thank God we didn't yet have Polly Peachum or Slick Willy; they would not have tolerated those cops manhandling Jesse and I think I would have tried to kill the cop who shot my dog.)

An inventory was made of these and other confiscated items but I cannot now locate it. Harriet said 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 for weeks, 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 the Faegre-Benson law firm (ironically, Mr. Benson, decades earlier, had been my father's patent and business attorney.).

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 that 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 claimed 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 such unbridled misconduct nor did they offer any clue as to how these private papers came to be transported to the Courthouse or offered 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 testifying, 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 script 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.

Having won the lawsuit and ended the war, Harriet went on to prove that she, at least, was an amazingly effective activist. She joined the North Star chapter of the Sierra Club in 1967, and by the early 1970s was working full time on environmental issues. In 1974 she founded the chapter's Wildlife Task Force, serving also as Legislative Chair, Conservation Chair, and as a member of the Executive Committee. She also worked professionally for four years as field representative for the national organization, Defenders of Wildlife. Harriet's first victory for wildlife was a bill passed in 1975 protecting the lynx in Minnesota. Her efforts in subsequent years led to legislation protecting bobcats, eagles, badgers, and bears. She also played an essential role in persuading the University of Minnesota's Veterinary School to establish in 1974 its now famous Raptor Center, which rehabilitates each year some 800 eagles, hawks, owls, and falcons that have been injured in the wild.

Over two decades, Harriet was a leading activist helping to protect Minnesota's timber wolves. During this time she led the coalition called Help Our Wolves Live (HOWL). She helped initiate the 1978 court action against the U.S. Fish And Wildlife Service, forcing them to comply with their own rules for wolf management. She worked tirelessly to demythologize the wolf, answering hundreds of letters written to HOWL by schoolchildren. Harriet was a legend in the local environmental community for her efforts on wildlife issues, as well as towards protecting Minnesota wetlands and the Boundary Waters Wilderness. In 1984 she received the Sigurd Olson Award, naming her as Environmentalist of the Year by the Sierra Club North Star Chapter. The Minnesota Wilderness and Parks Coalition named her a Minnesota Environmental Hero in 1996.

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 surprisingly 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 electrodermal measures and, in 1966,²⁷ we reported 11 studies in which the TFT had been correlated with concurrent measures of skin conductance level (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, as mentioned above, 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 patients from the Veteran's Administration (VA) hospital, schizophrenics who had all been removed from medications for a week or more before testing so that many of them 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, to my shame, 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

²⁷ Lykken, D.T., Rose, R., Luther, B., & Maley, M. (1966). Correcting psychophysiological measurements for individual differences in range. *Psychological Bulletin*, *66*, 481-484.

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, as explained earlier, I joined Venables in London trying to explicate the differences in our findings. One of his students, John Gruzelier, was entrusted with running the study at a local hospital while I, to my shame, focused my attention on other things. 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.

Computers

In addition to overcoming my debilitating stage fright, my principle accomplishment during that 1968-69 sabbatical in London was to learn how to operate Venables' laboratory computer and to begin at once seeking grant money to obtain one for my own lab. My PDP-12 was roughly the size of a refrigerator, cost several times my annual salary, and contained a high-speed memory of 16 kilobytes! Its permanent memory consisted of reels of DECTape. I spent a busy week at the Digital Equipment Corporation's east coast factory learning how to program it digitally and also in FORTRAN. A year or two later, I obtained another refrigerator-size cabinet containing, wonder of wonders, a 12-inch magnetic disk capable of storing up to one million bytes! My son Joe, then about 14, had his first hands-on computer experiences with that great machine. Limited as it was, it could run experiments and analyze results. We could feed in raw recorded EEG and it would produce histograms of the frequency distributions. It presented the Raven Matrices to our twin subjects, timing how long they pondered each item and scoring the result. I was fond of that PDP-12 and felt very advanced as the only

member of our faculty who was then computer-literate. Now, of course, my grandchildren race about the Internet and leave me in the dust.

Research with Twins

Nearly any experiment one might want to do with human subjects will be more interesting and yield more valuable results if one does it with twins. Twins are plentiful and easily recruited as experimental subjects; they know that they are special and they take special interest in whatever it is that is being studied. In spite of being special as twins, however, twins are more representative of the general population with respect to ethnic origin, socio-economic background, educational level, and similar variables, than any other group to which psychologists have easy access.

This representativeness is even more true of the families of twins. My colleagues and I have collected personality and other data from thousands of middle-aged adults, people from rural, small-town, and urban areas, with educations ranging from grade school to advanced degrees. How can one recruit such a sample?—by recruiting twins. The parents of twins find it natural that researchers would be interested in their offspring and they readily accept that one might also want data from the parents for comparison purposes.

Twin data are valuable for choosing among alternative methods or units of measurement. One of the reasons why psychological research too often fails to replicate is because our measures are full of error variance or fail to carve Nature at her joints. But any measure that shows high correlations between *monozygotic* (“identical” or MZ) cotwins deserves to be treated with respect. If X and Y are alternative ways of measuring some variable of interest and if X yields an MZ correlation appreciably higher than Y does, then X is almost certainly the better measure. MZ twins can be regarded as *parallel forms of the same individual* and that is extremely handy for the researcher.

There are still other advantages in studying twins but clearly the most important of all is that twins are Nature’s gift to those of us interested in genetics, in the relative importance of, and the interplay between, *nature* and *nurture* in determining the diverse ways in which we differ from one another, psychologically and biologically. MZ twins start out as a single fertilized ovum that divides into two identical cells, which then divide again and again en-route to creating a single human fetus but, for reasons still unknown, about 4 times in every thousand cases, the embryo splits, usually within the first week, into two equal halves, which each then continue to divide and develop into two fetuses that are genetically identical.

(Except for female MZ pairs, interestingly. Females get an X chromosome from each parent, while males get one X from Mom and a Y from Dad. But two X chromosomes would be troublesome if both remained active so, very early in fetal development, one of the Xs is, in effect, turned off in each fetal cell. But it will be Dad’s X that is inactivated in some cells and Mom’s in the others and which remains active in which cells is

determined by chance! Therefore, women, bless them, can each honestly claim to be unique, even if they are “identical” twins!)

Dizygotic (“fraternal” or DZ) twins, in contrast, result when the mother happens to make two ova available at the same time, to be fertilized by two different sperm. Just like ordinary siblings, DZ twins each receive different random halves of each parent’s genetic complement, some 30,000 genes from each, which pair-up to create a genetic blueprint almost certain to be at least slightly different from any seen before in human history. Most of those 30,000 gene pairs are *monomorphic*, the same for all normal humans, because they are the part of the blueprint that makes us human rather than an ape or a lizard. But thousands of gene loci in the blueprint are *polymorphic*, meaning that anywhere from two to twenty or more slightly different genes may occur in that locus, each producing somewhat different effects in the final product. The paired genes that mainly determine eye color, for example, occur in two forms. If both of your eye-color genes are of one type, your eyes will be predominantly blue (other genes influence tints, etc.) while if either of the genes is of the other type, your eyes will be brown (the brown gene is said to be *dominant* over the blue gene.)

Just like ordinary siblings, DZ twins can be of the same or opposite sex, and they may share from most to just a few of their polymorphic genes: I have a photo of one young-adult DZ pair in which one looks like a long-haired swinger while the other looks like his cotwin’s middle-aged uncle. On average, however, DZ twins share about 50% of their polymorphic genes while MZ pairs share 100%. The average pair of first-cousins, by comparison, share 15% of their polymorphic genes.

Research With Twins 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.²⁸ Because they are more interesting and more representative than college sophomores, the most common source of subjects for psychological experiments, I had begun working with twins in the early 1970s. 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.

MISTRA

About that 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,

²⁸ Lykken, D.T. (1982). Research with twins: The concept of emergensis. Presidential Address, 21st Annual Meetings of the Society for Psychophysiological Research, Washington, D.C., 1981; *Psychophysiology*, 19, 361-373.

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 research assistants 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.²⁹ 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, Auke Tellegen, and I began the Minnesota Twin Registry in the mid-1980s.³⁰ We identified from birth records all twins born in Minnesota from 1936 through 1955, located (most of) the surviving intact pairs, some 5,000 of them, and recruited them, and their spouses or other family members, to fill out various lengthy questionnaires and return them by mail. Over the next dozen years, we accumulated quantities of data about their families, their education, their occupational history, their personalities, their recreational and occupational interests, their social, religious, and political attitudes, and the like. I will review a few interesting findings below.

Matt and I later 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"³¹ and these 430 twin pairs were added the Registry data base.

²⁹ Bouchard, T.J., Lykken, D.T., McGue, M., Segal, N., & Tellegen, A. (1990). The sources of human psychological differences: The Minnesota Study of Twins Reared Apart. *Science*, 250, 223-228

³⁰ Lykken, D.T., Bouchard, T.J., McGue, M., & Tellegen, A. (1990). The Minnesota Twin Registry: Some initial findings. *Acta Geneticae Medicae et Gemellologiae*, 39, 35-70.

³¹ Lykken, D.T. (2000). The causes and costs of crime and a controversial cure. *Journal of Personality*, 68, 559-605.

The Minnesota Twin/Family Study

Finally, in about 1986, Bill Iacono, Matt McGue, and 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. We recruited male twins aged either 11 or 17, to come to the University with their parents for an initial day-long 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, psychophysiological traits, and even DNAs. 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 has now been extended yet again to include a parallel assessment of a sample of adopted children, and in other ways. I feel confident that these projects, which will continue well into the current new millennium, are producing a database of wide-ranging relevance and importance, and that they 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 (and to the project manager, Kevin Haroian, who first started managing things in my lab some 20 years ago) but I am glad to be able to claim credit for providing at least the initial impetus.

Emergenesis

In 1974, with Auke 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 a 1982 paper with Iacono and Tellegen. The MZ twin correlations for the proportions of the resting EEG frequency spectrum that occupied the traditional frequency bands (Delta, Theta, Alpha, and Beta), are high and about double the value of the DZ correlations. But 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 1981 SPR presidential address³² in which I introduced the idea of *emergensis*, and most extensively in my 1992 paper with Tom Bouchard, Matt McGue, and Auke Tellegen.³³

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 opposable

³² (Society for Psychophysiological Research) Lykken, D.T. (1982). Research with twins: The concept of emergensis. *Psychophysiology*, 19, 361-373.

³³ Lykken, D.T., Bouchard, T.J.Jr., McGue, M., & Tellegen, A. (1992). Emergenesis: Genetic traits that may not run in families. *American Psychologist*, 47, 1565-1577.

thumb, are constructed by polygenes working *configurally*, like workers on an assembly line, each doing a different and essential part, 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 (*i.e.* traits on which normal people differ) also are constructed *configurally* rather than additively.

For example, using Tellegen's MPQ Well-Being scale, we measured happiness levels, on some adult twins twice, 10 years apart.³⁴ Those retest correlations were quite modest (e.g., 0.55), which indicates that people can experience fairly large but transitory deviations in their happiness levels, as a consequence of winning the lottery or having a crippling accident, of being promoted at work or just feeling kind of sick, bored, or blah, so that on any given occasion of measurement they may score above or below their average or baseline value. Therefore, when MZ twins are measured only once, they will correlate less strongly than if their average happiness levels could be measured directly. But we found that the MZ twins correlated with each other over that long interval nearly as strongly as they correlated with themselves over the same interval, indicating that the stable component, or what I call the happiness set-point, is very strongly determined genetically. For DZ twins, however, 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.

A useful insight concerning emergence came from our earlier work on correcting electrodermal variables for individual differences in range of variation. Members of MZ twin pairs have very similar *uncorrected* skin conductance levels (SCLs), while DZ twins are virtually uncorrelated. After range-correction, however, the MZ pairs remain as similar as before while the DZ pairs now correlate about half as strongly as the MZs. Raw SCL is determined by (at least): the subject's current level of arousal, the reactivity of that subject's sudomotor system, the average reactivity of that subject's volar sweat glands to sudomotor innervation, the density of sweat glands in that subject's volar skin, and the conductivity of that subject's dry epidermis. Moreover, raw SCL is a function of the *product* of these several variables, each of which is determined in part by a different group of polymorphic genes. Sharing all their genes, MZ twins will display similar raw SCLs (or SCRs). DZ twins, in contrast, are likely to differ significantly in one or more of these components and a significant change in any multiplier will yield a significant difference in the product (the emergenic trait value), whereas the same change in any addend would cause only a slight change in the sum (the polygenic-additive trait value).

If subjects are presented with a series of loud sounds or painful shocks, their SCRs to the stimuli will gradually diminish or *habituate*, some subjects much more rapidly than others. The slope of this habituation curve is an emergenic trait, MZs correlating about .70 and DZs near zero. The Raven Matrices, an excellent measure of *fluid* intelligence,

³⁴ McGue, M., Bacon, S., & Lykken, D.T. (1992). Personality stability and change in early adulthood: A behavioral genetic analysis. *Developmental Psychology*, 29, 96-109.

produces emergenic scores when the items are presented without a time limit. The average time the subject does spend on each item is correlated about zero for DZ pairs but about 0.45 for MZs. When the number of Raven items solved correctly while untimed is divided by that subject's average time-per-item, then the DZ correlation for this ratio becomes about half the MZ value. Thus, problem-solving *power* is emergenic while speed-of-processing is not.

In addition to the happiness set-point, other traits of personality, as well as some interests and even certain social attitudes, are emergenic. On the trait of *Social Potency*, our MZT and MZA twins correlated 0.65 and 0.67, respectively, while the DZ pairs correlated only 0.02. A higher-order personality factor we labeled *Well Adjusted* produced similar correlations. In the area of interests, *Arts and Crafts* and also *Husbandry*, an interest in making, building, or repairing things, gave correlations of about 0.60 in both MZT and MZA twin pairs but only -0.07 in a combined group of DZ pairs. 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 genius³⁵, are all emergenic traits.

I also believe, incidentally, that emergenicity 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 Pimlico, and then at the Belmont, 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—or between his colts and fillies--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 numerous foals like himself and their advanced capabilities would have rather suddenly spread, producing a qualitative leap forward in evolutionary properties.

From Psychiatry to Psychology

After the NIDA grant for the Twin/Family Study was first approved, my relationship with the then current Chair of Psychiatry, Paula Clayton, began to deteriorate. I had

³⁵ Lykken, D.T. (1998). The genetics of genius. In A. Steptoe (Ed.), *Genius and the mind: Studies of creativity and temperament in the historical record*. Oxford: Oxford University Press.

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 insisted he should establish a “distinguished chair” for 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 why he had been hired) 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 by far 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 explain vividly what I was grieved about. The other option was for me to transfer to the Psychology Department 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.

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 50+ 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 at least 25% to more than 80% 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.

The Inadequacy of Single Measurements

We have also shown, although the message has not yet been widely comprehended, that the stable component of many psychological traits is genetically influenced much more strongly 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 each of us unsystematically over time. Therefore, a single measurement may find Twin A feeling good-natured while Twin B is feeling irritable for some 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 level, *i.e.*, their *irritability 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 lasting effects of experiences not shared by both twins. The truth is, however, that the MZ twins, who correlate today only 0.50 on happiness, may correlate 0.80 in respect to the mean of 10 happiness measurements taken semi-monthly. 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 mainly the time-to-time ups and downs in the current but temporary happiness *level*. It would be difficult to obtain numerous repeated measurements of traits, taken months or more apart so that variations about the set-points are random from one time to the next. But the MZ twin correlation for the mean of many such measurements can be estimated from the *cross-twin cross-time* correlation (R_{CT}), divided by retest or *within-twin cross-time* correlation (R_{WT}), obtained from just two measurements taken sufficiently far apart. In the case of an emergent trait, such as happiness, this between-twin, cross-time correlation is close to zero for DZ twins.

Romantic Infatuation

Romantic love, which anthropologists once thought had been invented by French poets in the Middle Ages, is now known to have characterized virtually every traditional society of which we have records. The other great apes do not experience infatuation because they do not need to pair-bond. The baby chimps cling to their mother's fur and she can provide for their care and sustenance without any help from the unknown father. But when our ancestors began producing those big-headed, altricial³⁶ babies that needed several years of constant carrying and oversight, more than the mothers could manage on their own, some sort of attachment had to be invented to persuade the fathers to help out. It turns out that, over all known societies that permit divorce, the modal length of

³⁶ *Altricial* species are helpless at birth.

marriage for those couples who eventually split is just four years; the fast-setting superglue of romantic infatuation lasts just long enough for Junior to be sturdy on his feet

Auke Tellegen and I have shown,³⁷ 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 it is not predictable from the characteristics of either the Chooser or the Chosen! Although MZ twin pairs tend to choose similar clothes, jobs, vacations, and so on, the spouses of MZ pairs do not resemble each other more than the spouses of DZ pairs. Asked how he/she felt about the cotwin's spouse when first met, both MZs' and DZs' ratings were as often negative as positive. Asked how she felt about her husband's cotwin when she first met her lover's family, the wives of male MZs were as likely to find their husband's clone to be unattractive as attractive. Because men, in their primitive way, judge women too much by their physical appearance, the husbands of MZ twins ranked their wives' look-alike cotwins somewhat more positively than negatively, but only slightly. Thus, having identical genomes does not cause twins to seek similar mates, nor does it lead the lover of one twin to feel similarly attracted to the MZ cotwin. With the confidence of science, therefore, we can tell you that the person whom you will fall in love with is the one you're standing next to when Cupid's arrow strikes.

These findings, suggesting that romantic relationships begin rather adventitiously, together with those about divorce, discussed below, were reviewed in an invited chapter.³⁸ We also know that certain important and inter-related social attitudes, such as conservatism, traditionalism, religiosity, and authoritarianism, have very strong genetic roots. These are important contributions and I have again been lucky in my opportunities and in my colleagues, so as to be 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.³⁹ 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 own 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.

³⁷ Lykken, D.T. & Tellegen, A. (1993). Is human mating adventitious or the result of lawful choice? A twin study of mate selection. *Journal of Personality and Social Psychology*, 65, 56-68.

³⁸ Lykken, D.T. (2002). How relationships begin and end: A genetic perspective. In H.T. Reis, M.A. Fitzpatrick, & A.L. Vangelisti (Eds.) *Stability and change in relationship behavior across the lifespan*. New York: Cambridge University Press.

³⁹ McGue, M. & Lykken, D.T. (1992). Genetic influence on risk of divorce. *Psychological Science*, 3, 368-373

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 might 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.

Here are the basic data:

	<u>MZA Pairs</u>	<u>DZA Pairs</u>
N of pairs where both had married:	57	48
Pairs concordant for no divorce:	36	20
Pairs concordant for divorce	10	5
Pairs discordant for divorce:	11	23
RISK if cotwin is not divorced:	$11/(72+11)= 13\%$	$23/(40+23)=37\%$
RISK if cotwin is divorced:	$20/(20+11)= 65\%$	$10/(10+23)= 30\%$

Among the 57 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 13%. **But**, if the cotwin *had* divorced—even before the pair had ever met as adults—then that risk leaps up to 65%! The corresponding data for the 48 pairs of DZA twins were 37% and 30%, 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, taken alone, would 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 (although, shhh! the best explanation for this difference may be just sampling error). 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. Q.E.D.

These divorce data suggest that there may be other traits that are emergent even though the DZT correlation is about half of the MZT value. This could happen when the phenotypic trait is strongly influenced by shared family experience. For example, McCourt et al⁴⁰ administered Altemeyer's Right Wing Authoritarianism scale (RWA: Altemeyer, 1988) to a large sample of reared-together, middle-aged twins, getting the following twin correlations

Reared Together Twins:

	MZT Twins	DZT Twins
Number of Pairs:	423	434
Within-Pair Correlations:	0.63 (0.57-0.68)	0.42 (0.34-0.49)

Thus, it would at first appear that RWA is heritable as a polygenic-additive trait. Because DZ twins share, on average, at least half of their polymorphic genes, they should be at least half as similar, in respect to such polygenic-additive traits, as are the MZs, who share all their genes. But these DZs were even more than half as similar as the MZs. This might be due to the fact that husbands and wives tend to be nearly as similar in RWA as those MZ twins are, and such assortative mating increases the genetic similarity of the offspring of those parents. Another possibility is that growing up together in the same family (and the same neighborhood) tends to make DZ twins more than half as similar as MZ twins in the extent of their authoritarian attitudes. It makes sense, of course, that this centripetal influence would be stronger for those DZ pairs whose genetic difference is larger than for those who happen to be genetically similar.

However, McCourt et al also administered the RWA scale to 77 pairs of reared-apart twins from MISTRA. These pairs had the same genetic similarity (although perhaps not as much parental similarity) as did the twins reared together but they were not influenced by one another nor by the same parents while growing up.

⁴⁰ McCourt, K., Bouchard, T.J. Jr., Lykken, D.T., Tellegen, A., & Keyes, M. (1999). Authoritarianism revisited: Genetic and environmental influences examined in twins reared apart and together. *Personality and Individual Differences*, 27, 985-1014.

Reared Apart Twins:

	MZA Twins	DZA Twins
Number of Pairs:	39	38
Within-Pair Correlations:	0.69 (0.48-0.82)	0.00 (-0.31-0.33)

Here those high MZA correlations corroborate that RWA is strongly influenced by genetic effects, yet the similarity of the DZA twins is not significantly different from zero. One interpretation of these two sets of results would be that RWA is *emergenetic* but also subject to a strong centripetal environmental influence in sibs reared together. If my DZ twin's genotype had been strongly authoritarian, I might not have become as sweet-tempered and liberal as my genotype inclined me to be. If he had been a daredevil, then I might have climbed higher and been less cautious than my genes suggested.

Happiness

When Tellegen and I published our paper on the genetics of happiness⁴¹, 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 arrows 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 above—or droop along below—one's set-point of subjective well-being. So I wrote a book to correct this mistake. It is called *Happiness*.⁴²

Soon after this book was published, I got a letter from a Scandinavian in Wisconsin informing me that my surname in Norse means “the happiness”! I then hoped that my agent would find a publisher in Norway for this book so that the title page would read:

LYKKE
av
Lykken

But it turns out that educated Scandinavians all read English so well that translation would not be worthwhile. However, the book has recently been translated into Chinese and the Peking University Press is preparing to publish it. Now, if only one of every thousand Chinese citizens would buy a copy, that would make me very happy (at least for a while).

In writing this book (which I enjoyed: what a good way to begin one's retirement!) I developed my *effectance theory* of subjective well-being (SWB). Many people, including many psychologists, had not thought of happiness as a trait at all, but rather as a *state* similar to pleasure or pain. These two states influence one's happiness, of course: indeed they are Nature's tools for controlling our behavior, keeping us safe and healthy

⁴¹ Lykken, D.T. & Tellegen, A. (1996). Happiness is a stochastic phenomenon. *Psychological Science*, 7.

⁴² Lykken, D.T. (1999). *Happiness: The Nature and Nurture of Joy and Contentment*. New York: St. Martin's Griffin

and reproducing. With all the talk these days about building life-like robots, I have not yet heard anyone propose to make them subject to pain and pleasure. But could any robot be truly lifelike if it could not fear injury or failure or feel joy and delight anticipating the achievement of some goal?

So displeasure, of whatever sort, motivates us to undo or escape from its source, and we avoid those behaviors that we fear might cause us pain or regret, that might diminish our subjective well-being. Pleasures, in contrast, enhance our current well-being and, when we are not preoccupied avoiding things we hate or fear, our behavior choices tend to be those we think will yield pleasure, or at least contentment. But, is happiness just a state, just the sum and difference of the good and bad things you experience from day to day?

Think of SWB as a lake on which your ship is sailing and the higher the lake level, the better you feel. That level seems to vary from person to person for genetic reasons. It also varies within people from time to time for physiological reasons. Mine is higher if I've had a good night's sleep, lower if I'm feeling stressed out. The individual SWB also varies as a consequence of recent events, going down when something unpleasant happens and up when something good happens, but it doesn't *stay* down or up. I can droop along under my personal SWB if I allow some of those happiness thieves to obtrude, if I let myself stay mad or anxious or if I let a spell of the blues lead me into thinking blue thoughts. But I can bounce along *above* my personal SWB by seeking activities and experiences that I enjoy. The problem is that these positive experiences cannot permanently raise my set point but only produce a temporary wave-like increase that soon recedes back to where I started. Therefore, I have to vary the input, ideally to become an "epicure of experience", like a gourmet at a buffet, alternating a little of this with a little of that, all things I like to do, never too much of just one.

Some people, like children at their first buffet, go just for the desserts, the entertainments. Desserts have their place but the mainstay of any happiness diet is productive effort, developing and exercising skills, doing something that needs doing, that is worth doing, and especially doing it well. All children (and many adults) have dreams of glory, and of what does this glory consist? It is not candy and ice cream and unlimited television watching but, rather, it is the adulation of the multitudes for some spectacular achievement. What most children (and most adults) fail to realize, however, is that the satisfaction of even the most spectacular achievement fades after a while and new accomplishments are needed to keep that SWB bouncing up into the higher reaches of the plus zone.

This human trait we have of enjoying productive labor, making things, having an impact on the world around us, was adaptive in the primordial stone-age environment as it is today. Those ancients who kept busy, both at the routine activities of hunting and gathering and nurturing the young and also in novel ways, developing new ideas, new methods, novel skills, were more likely to pass their genes along and become ancestors. One sees this *effectance motivation* even in infants whose abilities to impact their world is limited to knocking things over, breaking and banging and making a mess. Why do teen-agers break windows, carve their initials, paint graffiti, and commit similar acts of

vandalism? The “evil demon” theory is wrong or at least inadequate. Vandalism is a way of having an impact, exerting control, and one reason well-socialized youngsters are less inclined to vandalism is that well-socialized children are more likely to have developed useful skills and to have learned that the greater satisfactions accrue from making things rather than from breaking them.

Among the many skills and activities that we should and do feel good about are the ones designed to make other people feel good. These include not just helping and nurturing but also just being an enjoyable companion. The male homosexual whom I knew best, a cousin of my wife's, was someone who brought new life and joy to any room he entered. He was funny, he livened up any gathering, he was a delightful and imaginative host, an ideal guest, and a boon companion. His special qualities were wonderfully captured by actor William Hurt in the film, *Kiss of the Spider Woman*. Hurt plays a gay man, in some South American prison on a minor morals charge, who is celled with a political prisoner with whom Hurt's character, predictably, falls at once in love. This very macho cellmate is at first repelled by Hurt's effeminate ways but the Hurt character tells such interesting stories, produces a dinner party out of meager materials, and generally creates a light, fantastical atmosphere that mitigates the boredom and foreboding of imprisonment.

The actor Harvey Fierstein exemplifies the same qualities; only the intractably homophobic would fail to get a lift when he enters the room. What I am suggesting is that many gay men, at least those with more feminine natures, seem to make an art of daily living, they enliven the tedious, decorate the drab, make the mundane more amusing. These are all behaviors designed to keep one's SWB up above one's innate set-point. Perhaps this is a feminine trait---the single item on the Strong Vocational Interest Inventory that best distinguishes women from men is: "Decorating a room with flowers." Our Minnesota female twins in fact described themselves as somewhat happier on average than the males. Perhaps the euphemism "gay" is more apt than I had previously thought.

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” a parent or 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 birth and put up for permanent adoption. I would also empower the family-court judge, if the prospective mother was found to be at risk for substance abuse or venereal infection, to confine her to a nursing home where she would receive adequate nutrition and medical care, 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 or equivalent surgery.
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 above. 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.⁴³ In June of 1999, I summarized the arguments for parental licensure in a talk to some 600 lawyers attending the Minnesota Attorney General’s annual Continuing Legal Education Seminar and I was encouraged (and surprised) when the subsequent questions and comments were both interested and positive. That same year 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

⁴³ Lykken, D.T. (1998). The case for parental licensure. In T. Millon, E. Simonsen, & M. Birket-Smith (Eds.), *Psychopathy: Antisocial, Criminal, and Violent Behaviors*. New York: Guilford Press.

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 recently had cardiac bypass surgery, 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 2001.⁴⁴

The fact is that the American under-class is created and maintained by awful and usually single parents producing doomed children who mostly grow up to be awful parents too. Black Americans, who constitute about one-eighth of the population, commit about one-half of the violent crimes. There is no evidence that Blacks are genetically more criminally inclined than Whites. What we need to explain is why the small fraction of Blacks who are chronic offenders is as large numerically as the even smaller fraction of Whites who are responsible for most White crime. If sociopathy results from the inadequate parenting of biologically susceptible youngsters, and if rearing by unmarried single mothers (who more often provide inadequate parenting) is at least 6 times higher among Blacks than Whites in present-day America, then most of the Black:White difference in the rate of production of sociopaths can be accounted for.

I believe that parental licensure would, in 20 years or so, drastically reduce violent crime rates, both Black and White, and to an equal level. Then the three of my grandsons who are partly African-American will not grow up to frighten people they encounter on the street at night.

Several hundred thousand years of evolution have conditioned human brains to consider procreation to be their *raison d'etre*, and, therefore, parental rights are felt to be the most sacred rights of all. For example, no psychological treatment has been found to work with dangerous sex offenders and imprisonment, while it does prevent recidivism, costs more than \$25,000 per year. But there is a humane and inexpensive treatment that *does* work. European research over the past 50 years has shown that repeat sex offenders who accepted voluntary castration showed subsequent recidivism of less than 5%. In Denmark, of 900 previously dangerous sex criminals who were released after voluntary orchiectomy, only 2.2% reoffended and none in an aggressive manner. A study of 1600 repeat offenders in Germany showed that recidivism dropped from 84% to 3% after voluntary castration. Modern DNA techniques allow the identification of the guilty party in most sex crimes with great certainty. Yet not one of the 50 states requires a repeat sex offender to be genitally disarmed before he is released from prison. In 2003 and again in 2005, in response to concerns about how to deal with local sex offenders, I wrote letters to the editor of the Minneapolis newspaper citing these European findings. Although

⁴⁴ Lykken, D.T. (2001). Parental licensure. *The American Psychologist*, 56, 883-894.

signed by an emeritus professor of psychology who had been frequently quoted by the paper over the years on other topics, neither of my letters was printed. It has been wisely suggested that "the genes sing a prehistoric song that today should sometimes be resisted but which it would be foolish to ignore".⁴⁵ Perhaps the loudest of those prehistoric songs is the one that has made any interference with procreational rights the ultimate taboo.

Therefore, I think it is helpful to consider the adoption situation first. Good statistics indicate that children raised by single mothers, by persons who have never been economically independent, by criminals, or by addicts, all have a substantial likelihood of ending up abused, in prison, on welfare, or as addicts themselves. I would be in favor of a statute that prevented such people from adopting other people's infants on the grounds that these babies have their whole lives ahead of them; that they have rights too, important rights; and that this society has the responsibility to protect those rights. If one can agree with me on this, then I think one ought to be ready to consider establishing minimal requirements for biological parenthood.

A DFL Representative offered to introduce a licensure bill here in Minnesota but I think a legislature that would let everyone carry a handgun is unlikely to prevent anyone from carrying (and keeping) a baby, even though that baby's rights to life, liberty, and the pursuit of happiness are greatly imperiled. Still and all, I think parental licensure's time will come eventually, perhaps in your lifetime although not, alas, in mine.

The Last Chapter

In 1998, at age 70, I became a professor emeritus, which provided an opportunity to validate the effectance theory of subjective well-being that I had promulgated in my *Happiness* book. Our modern age provides unlimited and easily accessible entertainment: books of all descriptions; the world's great music recorded in fabulous fidelity; my choice of some 50,000 movies, old and new, that I can rent cheaply and watch when I wish. I am told that even sexual entertainments are available on the internet. Yet I dread the time approaching when entertainments may become the sole accessible ingredients of my waking hours. Because of my humanoid evolution, I need to *accomplish something* from time to time if I am to be content. Ideally, because of my peculiar training and history, I enjoy analyzing research data and writing about it: I submitted another paper to a journal just last week. But, now that I have so much free time, I discover to my surprise that I can actually enjoy doing the laundry, cleaning up the kitchen, going to the grocery, preparing a dinner, even cleaning out the cat box!

I do not fear death; in fact I rather look forward to it. But I do fear the kind of nightmare that frequently precedes death in American hospitals and nursing homes. Poor young Ben, my niece, Carol's, husband, whose galloping cancer not only caused searing pain but also obstructed his breathing so that, during his last hours, he repeatedly

⁴⁵ Bouchard et al. (1990). *Science*, 250, 223-228.

experienced the terrifying sensation of being strangled. My aged mother, who lost with her memory her sense of familiarity so that, for two years before the end, she awoke every morning in a place she didn't recognize, among people who all were total strangers to her. It can be ugly, painful, frightening, and humiliating and it is our fault, yours and mine, because we have not had the will or the courage to figure out a way to prevent such things from happening.

Some brave and considerate physicians used to be willing to take the necessary steps when the time came. Sigmund Freud smoked too many cigars for too many years and ultimately developed a disfiguring, painful, and terminal cancer of the jaw. The great man lingered on, still working, for several years more but finally he told his physician, Max Schur, "My dear Schur, you remember our first talk. You promised me then that you would help me when I could no longer carry on. It is only torture now and it has no longer any sense." Dr. Schur thereupon honored his pledge by giving his patient an injection of morphine sufficient to provide him a permanent peace⁴⁶.

One afternoon in 1957, my father drove the cleaning lady home and then, returning in his absent-minded way, he drove through a stop sign and was hit, broad side, by a fast-moving car. Thrown from his seat (this was before seat belts), Dad struck his head on the curbing and was taken, unconscious, to the hospital. The Chair of Neurosurgery at the University was a friend of mine and kindly agreed to take over the case. Around midnight, I was summoned to the hospital. My friend, gowned and with his gloved hands clasped in front of him, came out of the surgery and told me kindly: "The skull fracture is much worse than I had realized at first, David. We might be able to save him but there will be a lot of brain damage." I recognized this to be a question. My father's life had always been a life of the mind; he could not bear to know that his mind was now half gone and, if he did not know it, then the man who awoke would no longer be my father. His sudden loss would be grievously hard on my mother but to find her strong, kindly husband turned into a helpless and tormented half-man would be harder still. "Let him go then," I told my friend and he nodded and went back into the operating room. Ten minutes later, "Your dad's gone, Dave. I'm sorry. He never felt a thing."

Now, however, it appears that only Dr. Kervorkian has the necessary courage and he is pilloried for having it. I could look forward to the Final Chapter of my life with much greater feeling of security if every hospital had a wing called the Thanatorium with small suites where people who were ready to die could spend their last hours. We have the technology to banish pain and fear by adequate medication. With family members in attendance, the patients, if they were conscious, would be able to say their last good-byes in peace before being kindly put to sleep. But we shall have to learn to think more rationally about these matters if any progress is to be made. One test may turn out to be whether government and the medical profession can ever come to grips with the fact that the narcotic, heroin, is an ideal geriatric analgesic. Heroin soothes pain at least as well as morphine does and produces less gastric upset in old people. Moreover, it makes the patient feel good. But heroin is not listed in the American pharmacopoeia because it is

⁴⁶ The story of Freud's death is told in an article by Alan Stone in *The Harvard Mental Health Letter* for January, 1997. Pp.4,5

illegal and strongly addictive---and we don't want to let people become drug addicts just because they are dying!

As Harriet's capacities deteriorated last year, she became unable to do one after another of the useful activities that had always kept her happily occupied before. She couldn't go shopping, work in the garden, write letters, make things in the kitchen, clean the cat box—all of which explains my sense of painful relief when she suddenly, peacefully died last November (11/9/2005). (The grief, the real sense of loss, came later—like now.) My capacities are deteriorating also, physically and mentally. If I could have lasted as well as brother Georg did, well into his eighties, then I would have looked forward especially to watching my grandchildren maturing into adulthood. As a psychologist, I know that the other residents, of this “senior rental” in which I live, are not really as miserable as one would think, watching them shuffling about with their walkers or their attendants. But my Alzheimer's is getting stronger and I plan to leave before it takes over entirely. My brother, Robert, suffered from Alzheimer's and his personality died some four years before his body did— I have other plans. The idea of making my own “final exit” is not born of despair but, rather, a kind of prideful determination to make my own choices. I have had an extraordinarily fortunate life, as this autobiography attests, and my good luck will extend, I hope, to being able to make this final decision.

Postscript: I stopped believing in any conventional religion when I was 14 years old. I know it is silly, if almost irresistible, to ask, “Who started the Big Bang?” (silly because, if it was Him, who created Him?) But each step science takes forward seems to reveal new mysteries and surprises, such as other universes. On a simpler and more personal level, I have always believed the Harriet—and especially her mother, Gladys—were capable of a genuine telepathy or ESP. Moreover, working with so many twins over the years, I have heard endless, and I think reliable, accounts of telepathic experiences, especially between MZ cotwins. So I have never felt obliged to reject out of hand all somewhat mystical possibilities.

All of which combines with the sheer attractiveness of the conception, to make me wonder, hope, when mine and Harriet's ashes are poured together underneath that cemetery marker, that perhaps I shall also be joining her somehow in another way.