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SPECIAL SERIES:  
Personality Autobiographies

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## Who Wants to be a Psychologist?

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Every significant piece of public policy, every important generalization in history, economics, political science, and sociology depends on (largely unevaluated) assumptions about human nature. Personality psychology concerns the nature of human nature; it is, therefore, concerned with one of the most powerful and dangerous forces on earth. Developing adequate methods for conceptualizing human nature and forecasting significant components of social behavior—for example, integrity, creativity, leadership—would seem to be a matter of real urgency. Nonetheless, personality psychology has a minor and marginal status in academic psychology. I have spent my career trying to understand the origins of human behavior, trying to develop measurement models for capturing key elements of social performance, and trying to defend the study of personality against the complaints of a seemingly endless supply of academic critics.

### IN THE BEGINNING

My parents were from farm families in the Texas panhandle; they moved to Los Angeles during the great depression of the 1930s with little money or prospects. I was born there in 1937. When I started kindergarten, the little girls could already read, and I realized that they knew something I didn't. They still do.

In 1942, we moved 50 miles east of Los Angeles to the hard scrabble town of Fontana. Henry Kaiser had built a steel mill there and staffed it with ethnic families from the rust belt—tough, no nonsense people with strong religious convictions. Money was always a problem at home, and I was encouraged to start working early—but such is the tradition of farm families who can't survive without a stern work ethic. I found my first paying job when I was 13 and have continued to work since then.

By the second grade, I was a voracious reader. School, however, was boring, and I was an indifferent and disruptive pupil for several years; I became quite familiar with paddles and principal's offices. From early childhood, however, I was fascinated by "animal behavior" and spent a lot of time gathering and watching insects and various critters. In the sixth grade, I started a Biology Club and persuaded other kids to bring in specimens—mostly desert reptiles. In high

school, I discovered girls, alcohol, and geometry and found them all as interesting as animal behavior but for different reasons.

I began reading evolutionary theory (George Gaylord Simpson), which seemed intuitively correct and thrillingly controversial from the perspective of Christian fundamentalism—which I had abandoned when I was 10. During the summer before the 12th grade, I read Freud's (1900) *The Interpretation of Dreams*, and I was hooked. Many of Freud's claims were implausible, but the idea that the mind can operate outside awareness seemed obvious, and the notion that one could trace errors and mistakes that seemed random back to underlying erotic preoccupations was a revelation. I wanted to be a psychologist and decode the secrets of other people—to what purpose, it wasn't clear.

### COLLEGE

Then it was time for college, and I had no idea what to do. The University of California at Riverside was close to Fontana, and I could commute, so I enrolled. During enrollment, I visited the Psychology Department where the faculty studied white rats in laboratory situations. This had nothing to do with evolutionary biology or Freud, so I decided to ma-

major in Physics—I wanted to understand the meaning of the universe as well as the secrets of other people. I reasoned that if physics didn't work out, I could always try psychology.

That 1st year at Riverside was hard. I worked in a supermarket 20 hr a week, I took a 21-unit course load, and I flunked my first three exams—Chemistry, Math, and Physics—even though I actually studied for once. I thought about quitting, but then decided that “I would show them” and began improving my performance. At the end of the year, I asked my lab instructor when we would get to the meaning of the universe. He told me to forget it, that physics was not concerned with meaning. I decided to figure it out for myself. My textbook contained many equations that included mass as a variable. I began trying to solve a bunch of equations simultaneously to determine the nature of mass; after several hours I realized the analysis was circular, that nothing would come out of the equations that wasn't already there in the original definitions. Then, when I learned that electrons can be either particles or waves (which still makes no sense to me), I gave up on physics.

In the spring of 1956, I passed some difficult exams, interviews, and physicals, and received an Naval Reserve Officer's Training Corps (NROTC) scholarship. Because I was paying my own way through college, the Navy scholarship was important. It covered all tuition costs, lab fees, and books and even provided \$50 per month in salary. That \$50, in conjunction with the money I earned working in the evenings, meant I could stay in school. Riverside didn't have an NROTC unit, so I enrolled at the University of California, Los Angeles (UCLA) but as an Engineering student. The curriculum at Riverside was such that UCLA was easy by comparison. After a course in I-beams and boiler rivets, I decided engineering was not for me. That created a crisis: Going to college was about getting a job. Engineering would lead to a job, but the subject matter was dull. Remembering how much I had enjoyed reading Freud, I became a Psychology major. It was a monumental disappointment—boring and uninformative. The first semester of statistics, taught by the estimable Andrew Comrey, culminated in *t* tests, which we had learned in our first Freshman Physics lab at Riverside. Irving Maltzman taught History of Psychology. In his first lecture, he noted that many people thought psychology began with Aristotle. “But,” he said, “Aristotle believed in teleology and was obviously a fool.” I had just written a paper on Aristotle's *De Caelo*, I knew that Aristotle wasn't a fool, and I walked out of the class.

Shortly after entering UCLA, I found a job as a janitor. Cleaning toilets was unpleasant and after a month, I found another job—but before I could quit, my boss asked if I would manage the business for him so that he could pursue his real estate endeavors in the evenings. He owned a “building maintenance” business—a janitorial service with two clients who had buildings on Rodeo Drive in Beverly Hills. The first building was owned by the Wrather Organization, venture capitalists who had underwritten Disney; the second was

owned by Alan Ladd, then the wealthiest man in Hollywood. I occasionally met Rosalind Russell, Tyrone Power, Steve McQueen, Alan Ladd, and William Bendix in the elevators. More important, my boss, George Szervey, was a marvelous man. He had been a champion marathoner as a youth, a professor of art history in Budapest, the manager of the Dutch East India office in Djakarta, and owned the best private collection of Indonesian art in the United States. From him I learned about art, music (his wife Kati had been a world class concert violinist), food, wine, clothes, and table manners, all essential training for life in the middle class.

George was a terrible manager, he had constant turnover, and when someone didn't show up for work, I had to fill in. I inserted myself between George and the staff—all college students—and reduced the turnover to near zero. This stabilized the business and allowed us to provide more consistent service. I worked for George for 3 years, and I learned a lot about how to manage smart working-class talent.

Working 20 hours per week, carrying 18 to 21 hours per semester, I was mostly tired in those days. There were, however, some brilliant classes in philosophy and the history of science. I read David Hume and learned the power of skepticism and the folly of settled beliefs. From the magical Hans Meyerhoff (a dashing Viennese and former Office of Strategic Services operative), I learned how existentialism, phenomenology, Nietzsche, and psychoanalysis come together around the problem of authentic self-knowledge and how hard that knowledge is to achieve. With my smart and intellectually engaged roommates, Virgil Jose and Larry Meyer, there were many conversations about Albert Camus and J.-P. Sartre and the evils of logical positivism, analytic philosophy, and American foreign policy.

In 1960, I graduated (Summa Cum Laude and Phi Beta Kappa) and got married, with no realistic career plans. I was commissioned as a regular officer in the Navy and went to sea on a destroyer, thinking I might have a career in the Navy (I had graduated first in my class with a congratulatory telegram from the Secretary of the Navy). The Navy was one of the worst and one of the most important experiences of my life. I was sea-sick for 3 years, but I worked hard, and when I finally had a chance, I turned an incompetent gunnery department into an award winning operation, due largely to my experience managing talented working-class kids. The senior officers took exception to the way I treated my sailors (i.e., with respect) despite the fact that my department worked and the others did not. I concluded that military leadership was an oxymoron and that the Navy was not for me. I enjoyed the technical side of the work, but the right wing political culture was intolerable.

I thought I might pursue a PhD in English. While in the Navy, I read through the preliminary reading lists for several graduate programs and learned German. As I read, I became increasingly uncomfortable with the fact that literary criticism, although elegantly argued, was nonempirical and never came to a conclusion regarding why great literature is great

or why it should be taken seriously as something more than entertainment.

I left the Navy in the summer of 1963 and took a job as a Probation Officer in San Bernardino County, California while my wife attempted to finish college. The probation work was a transforming experience. My associates were (mostly) wonderful people who cared about the welfare of children. My supervisor was very wise man—Clarence Lankford, a University of Chicago PhD who had worked with Carl Rogers and Bruno Bettelheim. Each day, I had to evaluate problem kids with no empirical guidelines to help—my psychology training at UCLA was largely useless. What was indispensable, however, was a paperback book on *Abnormal Psychology*, especially the section on the personality disorders. I concluded that there are few neurotics and even fewer psychotics in the delinquent population, but all of them had personality disorders.

### GRADUATE SCHOOL

Inspired by my probation work and by the Kennedy administration, I vowed to make a contribution to society. Crime was (and is) a serious social problem, and I decided, in my typically unrealistic way, that I would get a PhD in psychology, identify the origins of delinquency, and develop the understanding necessary to eradicate crime. I applied to Harvard, Yale, Stanford, and Berkeley, stating in my applications that I wanted to do something for society. The first three wisely turned me down, but Berkeley offered me an assistantship.

I had been out of college for 5 years, and I looked forward to graduate school—to being back among people who cared about ideas—with keen anticipation. The Berkeley psychology department in 1964 was the best in the country and featured such stars as Frank Beach, William Meredith, Robert Tryon, Richard Lazarus, David Krech, Leo Postman, Richard Crutchfield, Edwin Ghiselli, Ted Sarbin, Jack Block, Donald MacKinnon, and Harrison Gough. For graduate students, however, it was not great. The faculty was locked in a ferocious civil war in which students could be swept up and then punished without warning. New graduate students had little or no contact with the great figures and instead took courses from assistant professors who were like assistant professors everywhere.

Meanwhile, there was the “student movement.” I was the first person I knew who was opposed to the Vietnam War—my Navy squadron created the Gulf of Tomkin incident that Congress used to legitimize invading Vietnam, and I deplored the war. The Berkeley student protesters seemed opportunistic, not idealistic (i.e., draft dodgers like former President Bill Clinton), and the faculty seemed feckless in the face of the students’ misbehavior. I wanted to study the problem of delinquency, but it was hard to concentrate with all the political distractions, and after the first semester, I thought seriously about quitting. Miraculously or adventi-

tiously, I was invited to take part in an assessment center project at the Institute of Personality Assessment and Research (IPAR), and the experience gave me my first real career focus. The IPAR staff was luminous: Frank Barron, Ravenna Helson, Donald MacKinnon, Kenneth Craik, Gerald Mendelsohn, Harrison Gough, and Wallace Hall represented the best of personality psychology. Creative, imaginative, and dedicated to the empirical study of high level effectiveness, among these people I felt I was finally home.

Outside of IPAR, graduate school seemed mostly a waste of time, and I was determined to finish quickly. I passed the required examinations in the spring of my 1st year and spent the summer and fall preparing for my advanced examinations, in early winter, intending to finish my dissertation in the spring of my 2nd year and get a job. My hopes of finishing a PhD in 2 years were dashed by Jack Block, a member of my examining committee, who thought I should spend more time “marinating in the system.” On the one hand, his gratuitous decision gave me an additional 12 months to complete a dissertation; on the other hand, I had a wife and two children to support, and I couldn’t afford the luxury of marinating in a patently silly system to no certain purpose.

Seven events from graduate school influenced the rest of my career. The first was the response set controversy, a convoluted squabble that started in 1958 and went on for 10 more years. The response set proponents claimed that when people answer items on personality inventories, their responses are determined primarily by the social desirability stimulus value of the item and that the inventories yielded no more information than that. This brought substantive research (i.e., on the nature of delinquency) to a halt while researchers dealt with the response set issue. In addition, those psychologists (behaviorists) who preferred to explain behavior in terms of “situational forces” quickly seized on the response set controversy as a way to further denounce personality psychology. Nonetheless, the response set controversy raised the important question of what, exactly, people are doing when they respond to questionnaire items; the response set answer was more interesting (and less literal minded) than the claim that people provide “self-reports.” This issue has never been properly resolved and still vexes industrial organizational (I/O) psychology.

The second big event was an IPAR colloquium by Warren Norman, in 1965, during which he outlined his evidence for the Five-factor model. I thought his argument and supporting data were compelling, but it would be several years before I could begin to assimilate them.

The third event was the publication of Walter Mischel’s (1968) full-throated attack on the merits of traditional personality psychology and personality assessment. I thought the book was poorly written and carelessly argued, but despite its scholarly inadequacies, I believed it would be a big problem for the field, and I was right.

The fourth event was an IPAR staff lunch in January 1967 at the height of the Vietnam war protest movement. The In-

stitute was established to study high-level effectiveness. Outside the building where we were eating, there were howling crowds, hovering helicopters, and plumes of tear gas. Inside the building, the IPAR staff deliberated on other, more academic matters. As a former probation officer, I knew that the police, who were being attacked by the Berkeley students, had problems with personnel selection, which led them to hire thugs as well as competent officers. Impatient with the lunch conversation, knowing that IPAR needed money, knowing that the U.S. Justice Department was funding police selection research, I said “How about a study of high level police performance?” Their response was pure annoyance—afterwards, Tom Bouchard warned me not to raise a subject like that again. I thought, “Between Walter Mischel’s criticisms and your disregard for applications, personality psychology is headed for big trouble.”

The fifth event was being exposed to T. R. Sarbin and role theory. Role theory fits seamlessly with the major tenets of existentialism (Nietzsche, Camus, Sartre) to which I had been converted as an undergraduate, and I found it almost commonsensical. In addition, as a working class boy who had made the transition to middle-class status, I understood the importance of playing roles and the consequences of doing it badly. More important, Sarbin simply had more data to support his views than any other person writing at the time.

The sixth event was Harrison Gough’s views on personality measurement. Classical test theory distinguishes between true scores, which are somehow real, and obtained scores, which are not actually real. In classical test theory, the goal of measurement is to determine a person’s true score, and the meaning of obtained scores is given by their relation to the true scores. Classical test theory blends inexorably into trait psychology as espoused by Thurstone, Cattell, Eysenck, Guilford, and Costa and McCrae. For these people, the goal of personality assessment is to measure traits, which are somehow related to true scores—that is, both exist somewhere but are inherently unobservable. I thought this was so much Platonic nonsense, and Gough seemed to agree. Gough argued that the goal of measurement is not to measure traits but to predict significant outcomes and that the meaning of a score is defined by what it predicts. This pragmatic argument is consistent with Wittgenstein’s (anti-Platonic) theory of meaning, and I found it utterly congenial. Measurement has a job to do and that is to predict non-test behavior. The trait theoretical perspective not only misunderstands the goal of measurement, it also creates monumental confusion by confounding prediction with explanation. Trait theorists propose to use trait scores to predict outcomes *and* to explain behavior. I find it inexplicable that so few people recognize this tautology. In any case, Gough’s model avoids it nicely.

The final event was my dissertation, whose argument I still regard as essentially correct. Freud maintained that the development of a moral sense was the most important step in human development—but of course, one that is ultimately problematical. Without a moral sense, children become crim-

inals; but with a moral sense, they become neurotic. Freud was a spoiled brat, he never learned to play nicely with other children, and he never understood that there is more to development than learning to live with one’s father.

George Herbert Mead (and the role theorists) thought that the most important step in development concerned learning to live with the peer group (the step that Freud missed). My dissertation focused on the contrast between Freud’s and Mead’s views on development. Freud argued that the essence of morality was attitudes toward authority, which were determined by the manner in which a child resolved the Oedipus complex. Attitudes toward authority can be assessed with the Socialization (So) scale of Gough’s (1975) California Psychological Inventory (CPI), the best single measure of delinquency ever developed. The problem is that many people, including myself, who have low scores on Gough’s So scale are not actually delinquent. George Herbert Mead argued that role-taking ability was the foundation of morality. I reasoned that role-taking ability might compensate for bad attitudes toward authority. I developed a measure of role-taking ability and provided evidence to support the point (cf. Hogan, 1973).

In the process of working out the complementary links between psychoanalysis and role theory, I concluded that the personality theory of the future would combine the best elements of Freud and Mead. Both were ardent Darwinists, both understood that much social behavior is unconsciously determined, both understood that development is consequential, one focused on relations to authority figures and the other focused on relations with peers, and both had well-articulated views on the nature of delinquency: For Freud, delinquents have failed to internalize a superego; for Mead, delinquents lack role-taking ability; and both were right. Much of my career has involved using Gough’s approach to measurement to work out the links between psychoanalysis and role theory (cf. Hogan & Hogan, 1998).

## JOHNS HOPKINS

In January of 1967, I interviewed for a job at Johns Hopkins University. Donald MacKinnon urged me not to consider the interview. He said that Hopkins was a distinguished but conservative department and hostile to personality. I was used to adversity, so I accepted the interview, was offered the job, and moved to Baltimore in August 1967. The Hopkins department was small but packed with stars: Wendell Garner and Alphonse Chapanis, founders of engineering psychology; James Deese, an important pioneer of psycholinguistics; Warren Torgerson, the flawed but gifted progenitor of multidimensional scaling; Bert Green, the brilliant but mean-spirited editor of *Psychometrika*; Julian Stanley, the guru of research methods; and Mary Ainsworth, the slightly dotty but inspired amanuensis of John Bowlby. A bit later John Holland arrived, and I came to admire him immensely.

Most of these people won American Psychological Association (APA) gold medals. They came to work at 7:00 in the morning, and left after 5:00. On Saturdays, they left after lunch. I respected them—sophisticated people with a dedicated work ethic. But Donald MacKinnon was right—it was a hostile environment—and for a personality psychologist, it was a like being back at UCLA. At the end of the spring semester 1968, I received the teacher of the year award and thought I would be fired for not paying enough attention to my research. James Deese, Alphonse Chapanis, and Mary Ainsworth—all prize winning teachers—rescued me, arguing that it is okay for untenured faculty to prepare for class.

For a new faculty member interested in personality, life at Hopkins was challenging. The other junior faculty, all experimentalists, sent graduate students to mock me. One comparative psychologist who studied aggression was particularly nasty. In early 1968, in an effort to engage him, I mentioned that I had just read Konrad Lorenz's (1963) book *On Aggression*. Frank Beach had told me he admired Lorenz, so I thought I was on safe ground. However, my aggressive colleague began yelling, at the top of his lungs, "Konrad Lorenz is a goddamned fool, and anyone who thinks his ideas are interesting is a goddamned fool." There were several such occasions around that time. Later in the spring, after a faculty meeting, we went to the faculty club for a drink. A distinguished scholar (whose name I won't mention) told the group, in a worried manner, that his daughter, who was attending a very liberal college in the Midwest, was coming home for spring break with her boyfriend, and she wanted to sleep with him. He asked the group for some advice. My aggressive colleague jumped up, yanked off his belt, and began flogging a leather couch while shouting, "I would beat her, I would beat her." When he finished beating the couch, he began harassing Mary Ainsworth, denouncing her research on attachment as drivel. I told him to quit it, and he began shouting at me (again). I said we needed to take the conversation outside, but he wouldn't go. I hit him a couple of times, then tore his shirt off. Then I was invited to leave the club. After that, the graduate students stopped mocking me, and the verbal abuse ended.

At Hopkins, the financial grind continued—my real income dropped \$150.00 per month over my graduate student pay, and I had to moonlight to pay the bills. From 1967 to 1982, I augmented my income in a variety of ways. In 1971, I was offered jobs at Harvard, Florida State, and University of California, Davis, but I couldn't consider them because the pay was even worse than Hopkins. My divorce in 1973 added to the financial burden.

In 1973, my friend Louis Galambos, editor of Dwight Eisenhower's papers, introduced me to Joyce Cummings. Joyce was finishing a PhD at the University of Maryland while working at Hopkins as the first Director of Women's Athletics. We were married in 1974. She is the best natural researcher I have ever met, and our careers have evolved together since then.

Because I always had to work, I never participated in organized athletics as a kid, and I always wondered if I had any talent. East coast winters are best spent indoors; squash is a game that is played indoors. In 1967, I took it up, became addicted, and played at least once a day until we moved to Tulsa in 1982. My wife Joyce is an excellent athlete; she also took up squash, and for several years, we traveled on weekends to tournaments in Philadelphia, New York, Washington, and New Haven. We both received national rankings, although Joyce was always ranked above me. My experience on the squash tour is one of my fondest memories, and I still miss it.

Another set of great memories concern cooking. I have always been a picky eater. My mother was a good cook and, after I left home, I began learning to cook for myself. In 1968, I met Bill and Catherine Garvey who were the best amateur cooks on the East coast. We spent many pleasant evenings cooking, eating, drinking wine, and talking philosophy and politics. The highlight of this period was the occasion when Jacques Pepin, formerly Charles De Gaulle's chef, came to Baltimore to give cooking lessons as a fund raiser for the Walter's Art Gallery. The Garveys invited him to dinner, promising that we would cook, after his busy day. I was so nervous, I made my entire dish as a trial run the night before so I could do it in the Garvey's kitchen the next night. Pepin was a marvelous guest, the evening was a great success, and it was the highlight of my cooking career.

Baltimore was a dangerous city—with a 50% unemployment rate, someone tried to break into our house about once a week, and my car was frequently vandalized. I bought a big, mean, black Labrador to help out in the evenings, and he solved a lot of problems. At some point, he began to sleep all night, and I bought a Jack Russell Terrier named Gucci to wake him up when trouble came. Later we got Dinger, then Gracie (named after Grace Slick who won my heart when she tried to put LSD in a punch bowl at Julie Nixon's White House reception), and finally Willie (named after Willie Nelson). We essentially organize our lives around these smart, stubborn little dogs.

Meanwhile, the problems I thought Mischel's (1968) book would create came about. The first 13 papers I submitted for publication were rejected outright. The following summarizes my experience at the time—in 1969, the Maryland State Police asked me to study their selection process. Using the CPI, I gathered data from three classes at the police academy; identified criterion data; ran the analyses on almost 300 cases; and developed interpretable, cross-validated regression equations with substantial correlations. My literature review showed that there were no good published data on the predictors of police performance. I wrote up my results and sent the manuscript to the *Journal of Applied Psychology*. The editor, Edwin Fleishman, returned the manuscript with the comment that "everyone knows these tests don't work." That is when I realized that data are irrelevant to academic disputes, that Nils Bohr was right when he

said that you never convince your academic critics; rather, you must wait until they die (cf. Hogan, 1971).

The antipersonality sentiment was only part of the problem. I wanted to study delinquency. The *doyen* of moral development was Lawrence Kohlberg who believed that the study of moral conduct was pointless, that nondelinquent children were potential fascists (because they respect authority), and that the proper focus of research was on moral reasoning. On the one hand, I had interviewed many delinquents who could reason beautifully about moral issues; on the other hand, Kohlberg's assessment process was a psychometric train wreck, as my two students, Bill Kurtines and Esther Grief (1974), demonstrated in *Psychological Bulletin*. However, the Kohlberg movement had caught a political wave—it offered quasi-scientific justification for opposing the dreadful war in Vietnam—and its lack of empirical support didn't matter. As a result, delinquency research ground to a halt.

By 1974 (7 years after my degree), I decided I had made a strategic mistake with my career; like Freud, I thought moral development was the core of personality development and something with which any competent theory of personality must deal. However, the personality psychologists thought I was a developmentalist, the developmental people thought I was a personality psychologist, and I seemed to be making little impact in either area. Tenure at Hopkins came only after promotion to full professor, that promotion was determined almost entirely by external review, and that review depended on visibility. I gave up on delinquency and focused on personality. I published my first textbook on personality theory in 1974; the book was a critical success and a financial failure (Hogan, 1976).

In 1974, with help from Julian Stanley, I received a large 4-year grant from the Spencer Foundation to study “verbal giftedness.” Julian was studying mathematical precocity. I argued that we needed people who could think clearly about complex moral and political issues to balance the efforts of gifted rocket engineers. My good friend Catherine Garvey—a psycholinguist and developmental psychologist—and I ran this complex research project. It involved recruiting very bright 13-year-olds (average IQ = 165) from all over the Baltimore/Washington area, assessing their reasoning ability, putting them through an intense enrichment experience during the summer, and then measuring changes in their reasoning ability. I can summarize our conclusions in terms of four points. First, the study of intelligence is one of the most anti-intellectual areas of all psychology—there are no theories of intelligence, there is just a measurement model. Second, on average, reasoning ability didn't change after our enrichment experience, but scores on standard measures of verbal intelligence went up very significantly. Third, we could predict the reasoning ability of the 13-year-olds very nicely using the CPI. And finally, gifted programs are primarily welfare for the overadvantaged—we were unable to recruit any working-class kids into the program (cf. Hogan, 1980).

In 1976, a smart young British psychologist, Nicholas Emler, came to the United States seeking a reconciliation with his estranged wife and a meeting with Lawrence Kohlberg. He came to Baltimore to talk with the notorious Kohlberg critic (me), we got along famously, and he moved into our big old house along with three cats and two dogs. Nick is well-educated, steeped in European social psychology (he was a colleague of Serge Moscovici), and later assumed Michael Argyle's chair in social psychology at Oxford. We had a marvelous time exploring the ideological foundations of American social psychology (Hogan & Emler, 1978). Our 1981 paper (Hogan & Emler, 1981) on retribution is the best thing I ever published.

All moralities use a notion of some larger, final good to justify their content—the do's and don'ts of everyday life. Conventional wisdom also maintains that the concept of distributive justice is the highest version of justice, and retribution is a primitive impulse. We (Hogan & Emler, 1978, 1981) noted that traditional moral theorists (e.g., Kant and Mill) are silent on the question of what to do about people who refuse to play by the rules. This is a serious omission because in the context of everyday life, few of us are the beneficiaries of others' spontaneous generosity; mostly we are exposed to others' efforts to manipulate, exploit, or cheat us. Moreover, social systems won't work in the absence of mechanisms to enforce the rules (e.g., the police). Although traditional moral systems stigmatize retribution, we concluded that revenge is what most people actually mean by justice. We outlined an alternative view of morality based on the value and necessity of retribution for maintaining the social order but where the process of retribution becomes progressively bureaucratized until it fails altogether, with ensuing vigilantism and lawlessness driven by a sense of morality (Hogan & Emler, 1981).

#### *JOURNAL OF PERSONALITY AND SOCIAL PSYCHOLOGY*

Personality psychology was under attack on all fronts: the sociologists, the I/O psychologists, the response set people, Mischel's groupies, the fundamental attribution error, and experimental social psychology in general—personality psychologists, John Darley announced, “are individual differences freaks.” I thought that if the discipline was to survive, it must secure access to a publication outlet. My wife was working for a consulting firm in Washington and knew Earl Alluisi, who was then Chair of the APA's Publications and Communications (P & C) Board. In the fall of 1977, we met Earl for dinner at his favorite Greek diner in Washington, DC. I explained to him that it was virtually impossible to publish measurement based personality research in the APA journals because the social psychologists controlled the relevant outlets. Earl, thinking about his legacy as the P & C Board chair, decided to split the *Journal of Personality and*

*Social Psychology (JPSP)* into three independent sections with separate editors: one section of which would be dedicated to personality, and I would be the editor. I have always been somewhat ashamed of how I became editor—that is, through sheer politics—but I didn't know what else to do.

The development of a section of *JPSP* devoted to personality was a turning point for personality psychology. It provided a badly needed publication outlet; it demonstrated that the discipline was intellectually viable; and it allowed young academics, those without tenure, to have careers—because they could publish in refereed journals. As editor, I would occasionally override the reviewers and publish papers that I thought were creative but out of the mainstream and, therefore, upsetting to conventional reviewers. There were many such cases, but two that I remember well were papers by Dan McAdams and Dean Keith Simonton. The reviewers didn't like them, I loved them, I exercised my secret prerogative, and these two subsequently had brilliant careers.

Prominent social psychologists were furious, claiming that I had perverted “their” journal (*JPSP*)—this even though, under their leadership, *JPSP* had become a financial disaster, the least cost-effective journal at APA. The social psychologists also argued that a personality section was pointless because there were no personality submissions to the poverty-stricken *JPSP*. With an outlet for personality manuscripts, however, submissions shot up. By the end of the first year (1978), the personality section of *JPSP* was the third largest journal in APA behind *American Psychologist* and *Journal of Applied Psychology*. We had also exhausted our page allotment, and I requested additional pages. By the end of the second year, the Pub Board asked me if I would like to edit a stand alone journal devoted to personality. I thought others would regard such a move as self-aggrandizing on my part—it wasn't about me, it was about keeping personality psychology alive—and I declined. Nonetheless, each year we exceeded our page allotment, and I duly put in a request for additional pages while maintaining the same 90% rejection rate as the two social psychology sections of *JPSP*. In addition, the new personality section immediately turned *JPSP* into a cash cow for APA.

In the Fall of 1977, Brian Little secured a grant from the Canadian Arts and Humanities council and invited some people to go to Canada to debate the issues vexing personality psychology. The location for the debate was Stanley House, the summer retreat of Sir Frederick Arthur Stanley, Governor General of Canada from 1888 to 1893 and the donor of the Stanley Cup, the oldest prize in professional athletics. Stanley House was an old wooden structure that resembled the set for the Canadian Chain Saw Massacre. It was deep in the woods, on Chaleur Bay, in a very remote location. In addition to Brian Little and me, the attendees included Ken Craik, Jerry Wiggins, and Walter Mischel. At breakfast on the 2nd day, I noticed that the cooks were upset. I asked Blanche, the drama queen hostess assigned to us by the Ministry of Culture, what was wrong. She reported that

the cooks, on their way to work that morning, saw Crazy Jacques in his yard. “Why was that unusual?” I asked. “Because Crazy Jacques had been institutionalized for several years.” “And why was he institutionalized?” I asked. “Because he tried to burn down Stanley House.” “And how do you know that he tried to burn down Stanley House?” I asked. “Because he burned down all the outbuildings.” At that point, Walter Mischel, who was sitting across the table from me, blurted out, “What was his diagnosis when he was institutionalized?” I concluded that his antipersonality stance was a pose designed to draw attention to himself and advance his career, and then he found himself trapped in a public posture. Although he had made his academic reputation arguing that there are no stable characteristics guiding peoples' behavior, when his own safety was on the line, he suddenly believed in personality.

In 1978, I was promoted to professor and granted tenure. I began to think about what I might do next for a career challenge. I considered trying to make some money, but (wrongly) dismissed it as being too easy. Meanwhile, in the fall of that year, I taught a graduate seminar in personality assessment that proved quite eventful. An obscure young personality psychologist named Paul Costa (an Eysenckian by predilection) had just moved to Baltimore to take a job with the National Institutes of Aging located in the Greek section of the city, and I invited him to participate in the seminar. Stimulated by some comments on the Five-factor model by Jerry Wiggins, I suggested to the class that if Warren Norman were right, then every existing personality inventory should be measuring the same five dimensions but with varying degrees of efficiency. I suggested we use the CPI as a test case.

We defined the five dimensions, then everyone sorted the CPI item pool into those dimensions based on content. It was an easy task, completed with considerable reliability. The only problem was that the group found it needed six categories. Using the item sortings, I created six content-based scoring keys for the CPI. I then rescored all of my archival CPI data using those keys and reran the analyses. In every case, the content keys yielded validity coefficients significantly higher than those obtained with the standard scales. I never wrote up these data out of respect for Harrison Gough. However, I concluded that the inventory of normal personality of the future would (a) maintain the measurement goals of the CPI but (b) use the base structure of the Five-factor model. We spent the remainder of the year writing and testing items for what we called the Hopkins Personality Inventory (HPI), and Paul Costa returned to South Baltimore to develop two more scales for his Eysenckian-inspired, three factor NEO.

#### TULSA AND INDUSTRIAL/ORGANIZATIONAL PSYCHOLOGY

In January 1980, my wife joined Johns Hopkins as a Research Professor, supporting herself on research grants.

Meanwhile, the Psychology Department, now controlled by a small group of neoconservative experimental psychologists, began its final death spiral. In October 1981, I visited the University of Tulsa, at the time the 10th wealthiest (per capita) university in the country. The President and the Dean of Arts & Sciences asked me what could be done to enhance the Psychology Department and in particular, how to structure a new PhD program. I suggested starting a graduate program in I/O psychology because there were none west of Tulsa, and based on surveys conducted by Division 14 (Society of Industrial and Organizational Psychology [SIOP]), there was significant demand for such training. In addition, I suggested the program be organized around personality to give it a unique focus. They responded by offering jobs to both Hogans, giving me an endowed chair and the chairmanship. We moved to Tulsa 9 months later.

Several interesting developments followed from our move to Tulsa. First, our salaries were very significantly increased, and for the first time in our lives, we didn't have to worry about money. Second, because of our new status in the University, we began socializing with the senior administrators and the Board of Trustees, who were the wealthiest people in a very wealthy town. From those experiences, I learned a great deal about how wealthy people behave and how universities really work (badly in both cases). Third, with a few personnel changes, the psychology program flourished. In a short period of time, it was significantly more productive—in scholarly terms—than comparable sized programs at Tulane, Southern Methodist University, Notre Dame, Emory, and Brandeis. In addition, our majors, enrollment numbers, and teacher ratings soared, and we attracted top-flight graduate students from all over the Midwest. Finally, none of this seemed to matter. On one hand, the Psychology faculty resented the changes; on the other hand, senior administrators, who had their own agendas, resented our success. Before long, I was defending rather than enhancing programs, and in time, that became tedious.

In 1982, I was invited to Lincoln, Nebraska to participate in their annual symposium series on motivation. My copresenters were Larry Pervin and Seymour Epstein. We shared an appreciation for the valid insights of psychoanalysis—Pervin is a practicing psychoanalyst. We also shared the view that Walter Mischel was wrong headed in his claims—Epstein subsequently published a series of papers showing that an elementary application of the Spearman–Brown formula refuted Mischel's argument about the variability of behavior across situations—and a disaster for the discipline. My talk was the first presentation of a model of personality that I call "Socioanalytic Theory"—this title signals the necessary links between Freud and Mead (Hogan, 1983).

In the spring of 1983, William Bennett, the Secretary of Education in the Reagan Administration who knew about my research on moral development and on gifted education, invited me to Washington to be an Undersecretary of Education. It was flattering, but the politics weren't congruent—I

am an old-fashioned liberal—and despite my new salary, I couldn't afford to take the job.

Our understanding with the University of Tulsa was that we could do consulting on the side. We developed a reasonably successful small business in which the HPI was the key. In 1982, we changed the name of the HPI to the Hogan Personality Inventory and sold the publishing rights to National Computer Systems (NCS), where the marketing people suggested it would nestle between the Minnesota Multiphasic Personality Inventory (Hathaway & McKinley, 1943) and the Strong–Campbell Interest Inventory (Strong, 1938). The problem, I discovered, was that NCS was a catalogue business and couldn't provide clients with research or consulting support. I brought several business opportunities to NCS, they didn't respond, and I lost the opportunities, but more important, NCS would only allow me to use the HPI as a customer, paying for the use of my own test. I felt as though a child had died.

In 1982, I began teaching a graduate course in Introduction to I/O Psychology. I discovered that the field was little more than applied experimental social psychology. Moreover, although virtually every significant topic (e.g., motivation) was a problem in personality psychology, the field was quite hostile to personality (and parts of it still are). My wife and I decided that it was impossible to sell personality to senior social or I/O psychologists, so we searched for promising younger people who might listen. Along the way, we met many such folks including Douglas Kenrick, David Funder, Roy Baumeister, Deniz Ones, Murray Barrick, Mick Mount, Gordon Curphy, and Adrian Furnham. By 1990, personality research had taken off inside I/O Psychology, and subsequent annual meetings of SIOP featured more personality papers than the annual APA meetings.

My experience in the Navy had sensitized me to the problem of leadership, which ranks alongside crime as one of the



FIGURE 1 At a reception for Timothy Leary hosted by Steve Strack at the American Psychological Association Convention, Los Angeles, 1994. From left, Robert Hogan (author), Patricia Hogan (sister), Timothy Leary (kindred spirit), and Joyce Hogan (wife of author).

enduring problems of human social organization. I taught Social Psychology during the 1970s. The textbooks all maintained that there is no such thing as personality; in addition, there is no such thing as leadership—leadership is a function of “the situation,” and in the right situation, anyone can be a leader. This is, of course, the sort of academic twaddle that gives psychology a bad name. In 1985, I began studying the leadership literature in detail. It soon became apparent that there was no convergence among researchers regarding the characteristics of effective leaders. Thinking about my experiences with authority figures in my life, I wondered if one could approach leadership through a study of failure (Hogan, Raskin, & Fazzini, 1990).

Herzberg’s (1966) classic study of motivation indicated that the most disagreeable fact of organizational life was one’s immediate boss—which was consistent with my experience. An unpublished paper by Jon Bentz and a monograph by McCall and Lombardo at the Center for Creative Leadership both indicated that at least half of any population of managers—chosen on the basis of intelligence and social skill—would fail. They also suggested that the causes of failure were pretty consistent—there were a small number of irritating interpersonal behaviors that eroded a manager’s ability to build a team. There seemed to be about 11 of these character flaws, and they matched the standard taxonomy of personality disorders rather well. We spent 5 years developing the Hogan Development Survey (HDS), which we published in 1997 (Hogan & Hogan, 1997). It is designed to assess these themes, and the resulting inventory has proved to be a robust predictor of occupational performance.

The academic study of leadership is ad hoc and lacking in a larger theoretical context. My view of leadership begins with Darwin. People evolved as group living animals. The groups were in competition—competition between humans has been a (if not the) major force driving human evolution. If one group prevails over another, the losers often disappear from the gene pool—according to legend, when Genghis Kahn invaded Persia, he killed every living person. Leadership is the solution to the problem of coordinating group effort. In hunter-gatherer societies, the lead man is distinguished by his skill at hunting or provisioning, his good decision making and wise counsel, and his ability to resolve disputes. I think people are prewired to seek these qualities in leaders. However, over the past 13,000 years, since the end of the last ice age and the beginnings of agriculture, settled human communities—as contrasted with hunter-gatherer groups—have been “led” by war lords. Government by warlord is the default position in human societies. In every hunter-gatherer group, from time to time, a bully (a proto-war lord) will emerge and try to dominate the other members of the group. The others, resenting the prospects of domination, will sanction the upstart. The intensity of these sanctions will continue to the point of murder. The citizens of contemporary societies dominated by war lords rarely have that option (cf. Hogan & Kaiser, in press).

The characteristics assessed by the HDS are qualities that define bullies. These characteristics are often masked by good social skills. Because managers are hired or promoted based on interviews, many people with no talent for building a team achieve leadership positions. The HDS is effective at identifying these people in advance. It is also interesting to note how the study of morality and personality come together in the study of leadership. Good leaders have integrity (the first stage of moral development), which can be measured. Good leaders have good social skills (the second stage of moral development), which can be measured. And good leaders have a broad and attractive vision of the future (the third stage of moral development), which also can be measured. Bad leaders lack one or all of these characteristics. Moreover, who we are determines how we lead—personality is directly linked to leadership style (Hogan, Curphy, & Hogan, 1994).

### HOGAN ASSESSMENT SYSTEMS

In 1982, we sold the rights of the HPI to NCS, and the HPI disappeared down a corporate rat hole. In 1992, the new head of assessment products at NCS called me and said that in his view, NCS had mishandled the HPI. He proposed returning the rights to the HPI plus the current book of business if we would promise not to sue them. We agreed, we assumed control of the HPI (Hogan & Hogan, 1995), and I had a new challenge. I built a gunnery department in the Navy, I built a journal, and I built a psychology department, but could we now build a business? The challenge was interesting; although Joyce and I knew nothing about business, several prominent personality psychologists had made a lot of money by turning their research into marketable products—James McKeen Cattell, J. P. Guilford, Raymond Cattell, David McClelland, John Holland, Harrison Gough, Douglas Jackson, and David Campbell.

We had validity data for every job in a very large trucking company, so the motor freight industry seemed a logical target for sales. I looked up every trucking company within one Southwest Airlines flight from Tulsa that had grossed \$10,000,000 in 1991, contacted them, and explained the benefits of rational and valid personnel selection. It was awful. After 2 months of this, it occurred to me that it is as unpleasant to be rebuffed by a 1 billion dollar company as it is to be rebuffed by a 10 million dollar company. So I contacted the 10 highest revenue producing trucking companies in the country, and three of them became clients. We grossed \$100,000 that year, and our business has grown about 40% per year since then.

Developing a business is not for the faint hearted. There are many aggravations, not the least of which is the process of being “vendorized” (my term). Human Resource (HR) managers in big companies regard sales people as vendors. They have vendor pens where vendors are required to wait

until the HR manager can find time to see them. It is a galling experience to be vendorized by a former secretary who is primarily preoccupied with her bowling game that evening. A second major aggravation concerns the fact that although poor managers cause employees to behave badly, there are employees who regardless of how well they are treated are perpetually unhappy and looking for revenge.

For me, the challenge was to gain a conceptual understanding of how to run a business. I am finally there, and I can summarize my understanding in terms of three points. First, it is much harder to run a small business than a big business—companies like General Electric have enormous inertia—and most businesses are poorly run, which accounts for the 90% failure rate among small businesses. Second, the reason so many businesses are badly run concerns their management. I never cease to be amazed at how crazy and inept chief executive officers (CEOs) can be; they become CEOs on the basis of politics, not demonstrated leadership. And third, in developing our business, I now understand the meaning of intelligence. To manage a business, one must solve problems all day long. One rarely has all the information one needs, the decisions are consequential, and the fate of the business is the sum of the decisions one makes. Intelligence is the ability to do this—to solve a wide range of consequential problems correctly with inadequate data. Every person I know who has built a successful business is smart; but I know a lot of people in business with high test scores who are not very smart—the finance group at Enron, for example.

From the beginning, we emphasized assessment quality—we never sell tests without providing clients with data regarding their validity for the job in question. There are 2,500 test publishers in the United States, and most of them ignore validity. By 1996, we had enough business that we could afford to hire another smart PhD to help with business development (Jared Lock), and since then, I have steadily reduced

the time I spend in sales efforts. In 2000, Rodney Warrenfeltz, an applied psychologist with extensive experience in marketing assessment processes, joined and gave us some badly needed business savvy. In 2003, we hired two smart businessmen to manage the day-to-day aspects of running Hogan Assessment Systems. Since then, Joyce and I have returned to full time research. We employ the largest group of researchers of any commercial test publisher, and we do more personality research than most psychology departments. We are surrounded by some of the brightest young people with whom we have ever worked, and we are doing pioneering research on transportability and synthetic/job component validity and on the use of item response theory and computer adaptive testing to develop multiple parallel forms of our personality measures. These are indeed the good old days.

## FUTURE DIRECTIONS

The primary focus of my career has been to promote personality psychology as an indispensable source of information about human nature—the most dangerous animate force in the world. That there should be a question about this is almost beyond my comprehension, but that is another question. In the late 1980s, I thought it might be useful to edit a handbook of personality psychology, something that hadn't been done in over 20 years. John Johnson and I recruited an unusually talented group of contributors, and after one false start, the handbook appeared in 1997 (Hogan, Johnson, & Briggs, 1997). Laurence Pervin had the same idea, and he published a handbook at about the same time. The fact that two handbooks could be published simultaneously, with very little overlap in the contributors testifies to the ongoing vigor of the discipline. Moreover, our handbook sold well, and the publisher has requested a second edition.

Marvin Dunnette (Dunnette & Hough, 1991) decided to revise his classic *Handbook of Industrial Organizational Psychology* and invited me to write the chapter on personality and personality assessment (Hogan, 1991), and I was grateful for the opportunity. Although the chapter was exhausting to write, it served to build an even more substantial bridge between applied and academic personality psychology. In 1999, Brent Roberts and I asked the American Psychological Association to support a conference in which we would bring together the best young personality researchers in Division 8 (Personality and Social Psychology) and Division 14 (I/O Psychology). Our idea was that the pure academics could learn about applied personality research, and the applied researchers could learn about the cutting edge of academic research. The resulting conference was a big success—Marvin Dunnette pronounced it the best conference he had ever attended—and the subsequent book, published in 2001, is widely cited (Roberts & Hogan, 2001).



FIGURE 2 Robert Hogan, circa 2005.

At Johns Hopkins, I had a joint appointment in Social Relations (Sociology), I taught the graduate course in social theory, and I am familiar with classical structural sociology. In Tulsa, in 1982, I was suddenly plunged into the world of I/O Psychology. Industrial psychology is mostly about selection, which I learned at IPAR in Berkeley. But what is organizational psychology? I thought it might be related to organizational theory, so in 1982, I began reading the classics in that field. Mostly, I didn't find the literature to be much advanced over Max Weber, Emile Durkheim, and George Herbert Mead. For the past several years, I have been working on an outline of organizational theory that is frankly reductionistic; it argues that all significant organizational phenomena emerge from the interaction of individual personalities. I have just finished writing a book on this topic, which can be summarized as follows. The fundamental dynamic of organizational life is the individual search for power and how that plays out over time. The rest of the argument draws on Freud's (1921) social theory (*Group Psychology and the Analysis of the Ego*) and Max Weber's ideas on bureaucracy and leadership. Universal themes in human social interaction (coalition formation, seduction, and betrayal) create universal but emergent themes at the organizational level (fiefdoms, rivalries, feuds, and civil wars). Volcanoes erupt, meteors strike, earthquakes create giant tidal waves, plagues, droughts, and huge changes in the weather are world shaking events, but at the quotidian level, human nature is the major force with which we have to contend. And personality psychology is about the nature of human nature.

Although there is considerable interest in personality psychology today within the I/O community, it is still an acquired taste in academic psychology, and the general public confuses it with clinical psychology, recovered memory, and the treatment of sexual dysfunction. For personality psychology to have a bright future, I think three things have to happen. The first is to overcome the legacy of Gordon Allport. Allport defined traits as enduring neuropsychic entities; he had a realist view—traits literally exist somewhere, inside people. His view has focused the research agenda in unfortunate ways. The search for neuropsychic entities turns personality psychology into an adjunct of neuropsychology. Moreover, after 70 years of research, no Allportian traits have yet been discovered, raising the possibility that they don't exist. Allport's views turn personality assessment into efforts to measure traits—as opposed to predicting consequential outcomes—and this basic science agenda will lead nowhere. In addition, Allport stigmatized reputation, saying it was not a fit subject for study, which is simply absurd. And then there is the confusion caused by using the same concepts to predict and explain behavior, which thoughtful observers correctly understand to be a tautology.

The second thing that must happen is for the discipline to foster a genuine climate of ideas. Personality psychology has lost touch with its intellectual roots. Although Freud, Jung,

Adler, Rank, Horney, Erikson, et al. have propounded a good bit of nonsense, they have also pointed to crucial and enduring themes in human life. Modern textbooks concern research topics and ignore the ideas that give these topics their relevance and justification. The solution is not to ignore these early ideas but to refine, extend, challenge, and perhaps even supplant them. We seem to have experienced a massive collapse of conceptual nerve.

The third thing that needs to happen is for personality psychologists to begin studying, once again, significant problems with implications for public policy. My idea of great personality research is the Authoritarian Personality studies conducted at Berkeley in the 1950s. The research concerned a timeless problem—why some people willingly comply with the arbitrary dictates of murderous tyrants. The research was informed by an interesting (if flawed) theoretical perspective—psychoanalysis. The data were based on assessment methodology. And the conclusions have passed the test of time—people who willingly bow down to arbitrary authority also endorse conservative economic and political views and are racist, sexist, and homophobic. Other, less politically volatile examples include the definitive research on creativity conducted at IPAR in the 1960s and McClelland's studies of power and achievement motivation.

It is said that people who criticize the present in terms of the lost achievements of the past are conservatives. It is also said that people who criticize the present in terms of a potentially brighter future are liberals. I firmly endorse all of the above.

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