Interview of Linda S. Gottfredson

by Howard Wainer & Daniel Robinson (Eds.), *Profiles in Research* (in preparation)

Abbreviated version in *Journal of Educational and Behavioral Statistics* (in press)

Professional Bio

Linda S. Gottfredson (nee Howarth) obtained her BA (psychology, Phi Beta Kappa) from UC Berkeley in 1969, served in the Peace Corps in the Malaysian Health Service from 1969-1972, and received her PhD (sociology) from the Johns Hopkins University (JHU) in 1976. She was Research Scientist at JHU's Center for Social Organization of Schools (CSOS) until 1986, after which time she joined the School of Education at the University of Delaware. She is currently professor of Education and Affiliated Faculty in UD's Honors Program. Dr. Gottfredson is a fellow of the American Psychological Association, Association for Psychological Science, and the Society for Industrial and Organization Psychology. She has made seminal contributions to vocational psychology, personnel selection psychology, intelligence research, as well as the study of health inequalities and human evolution. No stranger to controversy, she is perhaps most widely known for her clear, rigorous, and forthright analyses of individual and group differences in intelligence and their social consequences.

Q. In the post-Sputnik era of the 1960s science was a popular career for young men, but much less so for young women. What was there about your background that started you on a scientific pathway?

I always loved math and science, so the question was deciding to have any career at all. I grew up in Davis, California, where my father served on the faculty of UC-Davis's School of Veterinary Medicine, as had his father before him. The eldest of four, I planned to be a stay-at-home mom, like my own mother. That is what responsible young wives did in those days. I do not recall any pressure from my family either to attend college or not, or that I ever sought my parents' advice about it. I was a good student and conscientious kid, and just kept my own counsel. I greatly enjoyed an NSF program in laboratory science the summer before my senior year in high school, so upon graduation I entered UC-Davis as a biology major.

I have always been attracted to scientific inquiry. Math was my favorite school subject, and I regularly entered the state science fairs. I was taken by the natural world. My most enjoyable times as a child were wandering the fields, orchards, and dry riverbeds near our home, looking for insects and wildflowers for my collections. I enjoyed drawing birds, dogs, and farm animals, and thought it a treat to visit my dad's lab and the noisy, smelly livestock barns near its entrance. I also loved reading detective stories and mysteries. My jobs in various campus laboratories during high school and college confirmed for me that I would enjoy being a lab technician, if only I could get it to fit with motherhood. As it turned out, I would have a PhD long before I would be a mother.

Q. What led you from UC-Davis to Berkeley?

My husband (Gary Gottfredson) and I transferred after our sophomore year of college to UC-Berkeley, where we both majored in psychology. I especially liked the two classes I took with Richard Lazarus, who was always so passionate about his research. I also recall being intrigued by the possibility, achieved in some journal articles, that sufficiently clever research could be designed to settle long-standing scientific disputes.

It was the height of the Civil Rights Era and of protests against the Vietnam War. I was concerned about racial prejudice and inequality, so I volunteered to work in one of Oakland's ghetto schools, riding a city bus from Berkeley to assist a first-grade teacher several days a week. I also took a work-study job at Oakland's new Human Relations Commission, one of the first in the country, where I researched media bias and black underrepresentation in journalism. Anti-war demonstrations at UC-Berkeley escalated during my senior year, and Governor Ronald Reagan sent in the National Guard to restore order. Protestors always hoped to see pictures of themselves in magazines covering such events. Ironically, it would be my husband and I, and a friend, who would grace a centerfold of *Life Magazine* (May 30, 1969, pp. 42-43), the shot being taken minutes before the campus was tear-gassed by helicopter. It showed three decidedly conventional students striding business-like before a phalanx of armed National Guardsmen wearing gas masks, who were preventing the sea of protesters at their backs from moving forward across the plaza. The odor of tear gas still lingered in the Psychology library as we studied for final exams.

A different sort of political storm would hit Berkeley later that year, 1969, after one of its professors published an invited article in the prestigious *Harvard Educational Review* on the failures of compensatory education. But by then Gary and I were off working in Penang, Malaysia, as Peace Corps volunteers. I had not heard of the professor, Arthur Jensen, but would come to know a great deal about him by the time my own university became gripped by similar controversy 20 years later.

Q: Why did you go into the Peace Corps, and what influence did it have on you?

Gary would have gone to graduate school right after getting his BA from Berkeley, but he did not apply because we assumed he would be drafted for the Vietnam War. He ended up getting a 4-F exemption owing to a minor physical problem, which excused him from service, so I suggested we go into the Peace Corps. When we were assigned to Malaysia, I had to get out a map to find where it was.

Serving in Malaysia was the most challenging and rewarding experience of my youth. The country was Asian and tropical, not Western and temperate. Its language, food, customs, history, etiquette, housing, weather, diseases, transportation, bargaining, and pace of life were unfamiliar. The wildlife could be frightening (cobras, sea snakes) and the insects and microbes noxious. Malaysia was at once difficult and exotic, a grand adventure. It taught me a lot about who I am by putting me to the test in new ways. To give my daughters a taste of my Peace Corps experience, I took them as teenagers several weeks for three summers to do volunteer work in rural Nicaragua.

What else did I learn in the Peace Corps? We lived in a small rural village for most of our tenure. Our Malay neighbors were models of grace and generosity, despite being extremely poor by our standards. For the first time, I experienced small-town life, where no one is anonymous and nothing remains private. I also learned a bit about being a racial minority or otherwise conspicuously different. I was an American woman, so I was assumed to be "loose"—and sometimes approached as such. I was white and so I must be rich—a target of opportunity in the shops. At five feet eleven, I was over a head taller than most Malaysians, so I frequently evoked comments from bystanders who mistakenly assumed I would not understand them ("tinggi"—the word for tall—will surely be among the last Malay words I forget). Especially for an introvert like me, it was trying to be conspicuously different everywhere I went. I always felt on-stage or under observation, except with other volunteers. When Gary and I landed in England on our way back to the States, I felt gloriously anonymous and invisible. Little did I appreciate how odd we probably looked, with our peculiar clothes, tanned skin, and shabby baggage!

I also came to appreciate how different cultures can be. Not the vapid sort of "cultural diversity" we are urged to celebrate in the USA today, but profound differences in perceptions of good and evil, responsibilities and rights. Not the delightful panoply of cuisines, dress, festivals, and material culture that all can enjoy, but conflicting visions of governing moral, religious, and political principles.

Malaysia also provided a lesson in the harsher realities of multi-ethnic societies, especially when ethnicities differ in aspirations, talents, and entrepreneurial zeal. It was tri-ethnic, with the Malay and Chinese each comprising 40-45% of the population and South Indians most of the remainder. Ethnic Malays dominated politically because, with Malaya's independence in 1957, the new constitution gave them special political rights as *bumi putra* (the inhabitants pre-dating British rule). However, the ethnic Chinese dominated professionally and economically. Tensions had culminated in the May 13 Incident (race riots between the Chinese and Malays the Spring before we arrived), so the government was imposing financial and political restrictions to restrain the Chinese and was instituting quotas of various sorts to advance the Malays. I wrote about these difficulties in one of my first papers in graduate school, and thought of them when writing about the workforce diversity movement that emerged late in the 1980s in the U.S. (1992).

Q. What did you actually do as a Peace Corps volunteer?

When we reported to the chief health officer of the state, he greeted us by saying that he had told the Peace Corps staff he did not want any volunteers. Not a great start. And while the job I was assigned may have sounded good in the abstract, it was unworkable because it was not on any organization chart. We wives were to be liaisons between rural health clinics and the family planning program, an arrangement probably unbeknownst to both operations. The husbands were to assist with public health campaigns, such as installing water-sealed latrines, screening villagers for filariasis (elephantiasis), spraying DDT in homes to control malaria, and the like.

Eventually I created a more viable job for myself: improving the system by which births and deaths were collected and reported to the State of Penang, where we were stationed. It seemed obvious, for instance, that the health office was failing to record all infant deaths because I calculated from its reports that the state's infant death rate was ostensibly lower than Sweden's. I therefore I tracked down where and why many infant deaths were being missed. I also streamlined the laborious monthly reports the nurses and midwives had to submit. It seemed that the old forms had not been eliminated as new ones were introduced—some the relics of colonial rule. I had good support from the new chief health officer, so Gary and I reenlisted for a third year so that I could complete my projects. I loved the detailed investigative work, the hunt for data, and the organizing of them for practical use.

By exercising these interests and skills, I had also discovered them. At that time, however, I was still ignoring my scientific bent because it did not fit with my perceptions of motherhood. Stated in terms of the theory of occupational choice I published less than a decade later, I was failing to reconsider the viable options that during childhood I had rejected as incompatible with the *social* role I sought. It was Gary who suggested that I go to graduate school while he did. He had been accepted into Johns Hopkins' Department of Psychology, so I explored the options at JHU while working for a year at its School of Public Health. Working alongside PhDs that year, doing what they did, expunged my lingering doubts about being fit for such work. Gary preferred that we not be in the same department, so I entered the Sociology program—which opened up different opportunities.

Q. You transferred to Berkeley because Gary transferred, you considered graduate school because Gary suggested it, and you majored in sociology because Gary did not want both of you in psychology. You seem to have acquiesced to Gary on many issues, both big and small. Is this a misconception? Or was he indeed your guide throughout the early parts of your career?

No, that's not how it was. The transfer to Berkeley was a joint decision. As for graduate school, my sense was that Gary did not want us to be competitors in the same graduate program, but I was perfectly happy to train in JHU's sociology department or school of public health, especially given my interest in social inequality and health. I did, however, take the Psychology Department course I wanted in order to learn factor analysis: Bert Green's Multivariate Analysis. In any case, Gary's suggestion that I go to graduate school was academic encouragement, which for women was rare at that time. No one in my life had ever mentioned the notion, pro or con, as if it simply did not exist. My response was not one of acquiescing to Gary, but of finally taking off my blinders and seeing what should have been obvious all along.

Q. What was there about sociology that you found attractive?

Coming of age during the Civil Rights Era, I had long been interested in social inequality, and inequality is perhaps sociology's chief concern. I liked JHU's Sociology program because it imposed so few bureaucratic requirements (no qualifying exams, no final defense) and no close supervision. I never really had an advisor or felt the need for one. Again, I just marched under my own counsel. I will mention one particular experience in graduate school, however, because it illustrates something about me that people tend to love or hate and that periodically punctuates my career. Stated positively, I cannot go along quietly with scientific practices I think unethical or dishonest, especially when perpetrated by influential persons. The instigation was a course project that required us to survey Baltimore residents in their homes on Sunday mornings. The task asked them to sort cards containing several descriptors of fictitious job applicants, which included race. To my mind, it trapped respondents into seeming racially prejudiced whether they were or not, which some of them also perceived. I reported my experience in class and suggested that the survey be terminated. I, for one, did not want to continue participating in it. I announced this to one of the top survey researchers of the era as my classmates sat in stunned silence. The department chair said I could not refuse the assignment without failing the class (a required course), so I completed my interviews. Everyone had been polite and business-like, which may have only reinforced my willingness to raise such objections.

Gary would be graduating in my third year, so I finished my dissertation in three years. In fact, I was applying for jobs before I had even finished my dissertation proposal. I was surprised but greatly relieved that none of the job interviewers asked about my progress. Our best joint option was for Gary to take an offer from the American Psychological Association (APA) to work at its headquarters in Washington, DC, and for me to take a research position at the Center for Social Organization of Schools (CSOS), which was the grant-supported research center where I had done my dissertation research.

Q: Was this where you got to know John Holland?

Yes. During graduate school and for some years afterward, I worked in close conjunction with John. Although he was a vocational psychologist, he had moved to the Sociology Department when JHU eliminated its Education Department. His advice was always to "keep your eye on the main tent," which for him was focusing on the most basic constructs and assessment devices to implement his theory of vocational choice and personality. I seemed to violate his principle right from the start, which left him confused about just what was my focus. I eventually created a flow chart of how all my vocational research was logically connected, but he wasn't persuaded. I don't recall his exact response, but it was no doubt droll. He is a witty, even mischievous man, and a pleasure to work with. He and Gary once published a bibliography under a humorous pseudonym (Adam Lackey), which they loved seeing cited. But more seriously, John exemplifies someone who went against the tide of his profession (some thought his ideas outrageous and even unethical), but whose ideas and assessment tools were so good that the tide turned in his direction. He would *become* the tide. That probably fostered the same nascent proclivities in me. At any rate, I was soon pointing out (e.g., 1978, 1980) what I considered his typology's most serious limitations (e.g., no vertical dimension for distinguishing occupations by status or difficulty level, which he has since added), though with a mind to rectifying them for use in my own work—which was directed to the lacunae in both sociology and vocational psychology.

Q: You seem to have worked in a succession of fields. How did that happen?

You could say that I have sojourned in five fields since getting my PhD in sociology—vocational psychology, personnel selection psychology, intelligence, health disparities, and human evolution. The unifying theme is individual differences: in interests, abilities, health, and socioeconomic outcomes. Sociologists would refer to them as inequalities. My progression into new fields was always prompted by my attempting to answer a key question about individual differences. Rather than switching between fields, I have become intrigued by contiguous ones, but the shifts in emphasis do demarcate different phases in my career. My progression beyond sociology began in graduate school, and the first stop was vocational psychology.

I trained in sociology at a time when path modeling of status attainment processes (who gets ahead) was a cutting-edge methodology and neo-Marxist theories of social inequality were fashionable. I was skeptical of both, which is partly why I became interested in John Holland's work. Beginning with my dissertation, I worked toward integrating the sociological and psychological approaches to career development. My particular aim was to understand better why individuals of different races, sexes, and social classes tend to end up in different kinds of jobs—and hence different places within the social order. The two disciplines spoke different languages, carved up the empirical world quite differently, and disagreed sharply about human variability and its causes. Sociologists classified occupations vertically, by socioeconomic status, while vocational psychologists ordered them horizontally, by field of work or vocational interests. So, where one emphasized invidious distinctions, the other avoided them. Psychologists studied how people choose occupations; sociologists, how society erects barriers to free choice. Psychologists had myriad assessments and inventories for measuring individual differences in career-relevant interests, abilities, aspirations, and values, while sociologists viewed individuals as psychologically identical and made different only by the circumstances forced upon them. The two fields seemed to have nothing in common, but both seemed crucial for answering the general question they shared: Why do different types of people end up in different occupations?

I began by putting both views of occupations on the same map, literally, by exploiting large national databases to determine the distribution of US jobs across my two-dimensional, type-by-level, map of occupations in the US economy. Next, I obtained data from large, nationally-representative, longitudinal studies of young men to track their aspirations and actual employment, from one age to the next, as they moved into and dispersed across this world of work. I have to say that working with those large samples (1000s, 10,000s) was almost magical, because large *Ns* provide beautifully regular, clear patterns of results. One gets spoiled! As a side note, at that time datasets still came on large reels of tape that we would process at night on the "small" room-size computer in the Center's carriage house, and becoming a keypunch operator was still a good job option.

My major contribution to vocational psychology came a bit later, in 1981, and was theoretical: a developmental theory of circumscription and compromise in career aspirations. While analyzing the survey and Census data, I had also been reading all the studies I could find on career choice and development. It was a slog, but quite useful. I developed an intuition for the data which, interestingly, required looking past the authors' own conclusions about their results. Patterns began to emerge, but striking puzzles as well. I would develop hypotheses about them—say, parents' expectations for their children, or the choices people make when forced to compromise—and then scour specialized literatures for published evidence that could "test" my hypothesis. I never set out to create a theory but, as I explained later (1983), a theory emerged from this iterative process of trying to solve one specific puzzle: why do children's own aspirations for themselves recreate the social distinctions of the parent generation, even before children face the realities of the labor market?

It began inauspiciously with a 6-page sketch of an idea, accepted for publication in the *Journal of Counseling Psychology*, that redefined vocational choice as a matter of eliminating *least* desirable options—circumscription—rather than of identifying the most desirable. Six months later (I had asked to revise a paragraph), the sketch had morphed into a long monograph explaining how children gradually, and mostly unwittingly, define their place in the social world, through occupational preferences, from age three years through adolescence. I had spent those six months exploring related literatures, such as sex stereotyping, cognitive development, and self-concept, to answer specific questions that occurred to me as

I wrote. My theory also drew heavily on insights I had gleaned while researching organizational behavior at the School of Public Health during graduate school (1981b). What struck me most in that literature was the notion that people usually settle for the "good enough" (they *satisfice*) because the information they lack for making the best decision is either unavailable, too costly to obtain, or difficult to use. This idea seemed to describe vocational choice perfectly.

The theory quickly became one of the top three in the field (Holland's and Donald Super's being the other two). I have extended the theory to include the genetic bases of individuality and choice (1999, 2002b), as well as developing ways to use the theory in career guidance (2005c), but otherwise have done no further research with it. Other questions seemed more pressing.

My next creative accomplishment, in 1982 (at age 34), was becoming a mother. As a young scholar, you wonder if you will ever amount to anything professionally. As a woman you worry that motherhood will only decrease your chances. My new husband (JHU sociologist Robert Gordon) and I wanted to have children, so it was reassuring to me to have a solid professional success under my belt. "The baby" turned out to be twins, delightfully dimpled identical red-headed girls (just like their father--except for the sex, of course!). I nearly fainted when told at five months into the pregnancy. Bob was thrilled. I absolutely loved being pregnant, even when it became hard to breathe, or climb stairs, or get through doorways without bumping against the door jamb. I was huge; my weight increased by over a third. At work, I had been reading military studies on severe sleep deprivation, and after my daughters were born I got to experience it for several months. I stayed home with them for two months or so, then set up a crib and baby swing in my office. I hired a teenager to walk one baby in a stroller while I watched over the other in my office. This way I could be with them and nurse them during the day for several more months. I wasn't very productive, but it was a nice compromise and the Center director did not object.

By this time I had become interested in the aptitudes that jobs actually require, which would take me into the second phase of my career. My career theory highlighted how young people often stunt their own career aspirations, so I was thinking of ways that counselors could help youngsters reconsider viable options they may have unthinkingly eliminated from further consideration much earlier in childhood. The key, however, was that the options be viable.

Vocational psychology had dropped its traditional concern with aptitudes and was focusing almost exclusively on how to assess vocational interests and values. I think it was a reaction to fears that ability testing might be culturally biased and, in addition, that it could inappropriately pigeon-hole students and depress their career ambitions. But the community college counselors I talked to were concerned that ignoring the abilities of students who sought their guidance might be tantamount to malpractice (my term). They were queasy about letting—indeed, effectively helping—many students commit themselves to sure failure and wasted time. It was simply not true that students "could be anything they wanted" if only they tried. But how could counselors urge students to be more realistic without quashing their opportunities and ambitions?

The first challenge was to figure out which abilities different occupations actually require—not employer-set entry requirements, but functional demands for getting the work done well by objective standards. I scoured the literature for all relevant data. I fully expected to find that different jobs would require distinctly different abilities, and had been astonished to hear the seemingly-implausible claim, at one APA convention, that a single ability (intelligence) might predict performance in all jobs. As illustrated by my dissertation, I was still a multiple-intelligences person, though that term was not yet in use, still awaiting publication of Howard Gardner's influential 1983 book. I did not find good data on aptitude requirements in the vocational literature, and sociologists mostly rejected the notion of ability differences. They tended to argue that ability was either socially constructed or functionally irrelevant, but their evidence was unconvincing.

So I turned to the next obvious source—employee selection research. What a goldmine! There was a long history of federal research in the civilian and military sectors, because it was of great practical importance to select productive workforces. The military had had some disastrous experiences as a result of ignoring competence, not only during World War II but also during McNamara's Project 100,000 experiment (which deliberately lowered enlistment standards) and following the Armed Services Vocational Aptitude Battery (ASVAB) misnorming debacle (which inadvertently lowered selection standards). Frank Schmidt and John (Jack) Hunter had just introduced meta-analysis to the field, and John Campbell and his colleagues were carrying out huge studies to organize conceptually both the predictor and criterion space. Here were hard-headed empiricists with good data. SIOP conventions were full of my kind of people—data-driven and not impressed by small one-off studies (SIOP is the Society of Industrial-Organizational Psychologists, which is Division 14 of the APA). Other people in

SIOP, most notably EJ McCormick, had been dissecting specific jobs to see which tasks they required workers to do, when, how, and with which skills and equipment. Sociologists tended to treat job requirements and rewards as reflecting only the arbitrary tastes of employers because they assumed, wrongly, that virtually anyone could perform virtually any job. Not so job analysts. They dealt in the nitty-gritty realities of work, whereas sociologists were invoking neo-Marxist imaginings about class oppression, which was somehow being systematically enforced in the myriad hiring decisions in different settings and industries by thousands of employers who were competing with one another for competent help. For sociologists, jobs were nothing more than bundles of rewards, though they weren't much more than that for vocational psychologists either.

I was able to triangulate the data from personnel selection and job analysis with the more vocationally-oriented data on occupational requirements to produce a two-dimensional, 13-cluster "occupational aptitude patterns map" for counseling purposes (1986a). It allowed me to propose a solution to the counselors' conundrum of: "How do I use aptitudes data to increase, not decrease, students' opportunities for career development?" The answer, briefly, was this — If counselees' aptitudes seem inadequate for their preferred type of work, then help them find ways to make themselves more competitive for it, but also help them develop a realistic back-up plan in case their preferred choice is unattainable.

Nothing I had done in vocational psychology to this point had been controversial, although I had defended John Holland when he was attacked in the early 1980s for supposed sex bias in his interest inventory (1982). The notion was that interest assessments should report the same distribution of results for men and women. That is, equal percentages of both sexes should be told they had Social interests (teaching, nursing, social work, etc.), Realistic interests (e.g., engineering, skilled trades, etc.), and so on. This was empirical nonsense, which I viewed as misguided social engineering and bound to mislead counselees. John never backed down, though some inventories started reporting sex-normed scores in addition to the usual, non-normed scores. I would re-enter debates over gender fairness decades later, when Harvard President Larry Summers was pilloried for his remarks about sex differences (2005f).

Vocational psychologists were starting to pay attention to groups who might face special difficulties, so I was asked to write about interest assessment for "special groups," in which I included language minorities, physically handicapped individuals, and others in addition to race, class, and gender groups. I was well aware by then of average race and class differences in ability, so I deliberated whether I should include intelligence as one of various risk factors for counselors to consider, including the fact that some ethnic groups were greater at risk for low IQ than others. Should I cross that line? Professional integrity seemed to require it, so I did. The editor's only remark, though telling, was that my chapter showed "attitude"—an otherwise odd description of a chapter stuffed with dry tables and charts on language spoken, functional limitations, unemployment rates, and the like (1986c).

The Director of CSOS did not think much of my vocational psychology research, which was fair enough, but it was my forays into intelligence and race that started to agitate him. Likewise fair, or at least understandable, because the Center's existence depended on pleasing federal granting agencies, and talking about the role of intelligence in schools was not politic.

Q: Was this how you got involved in the study of intelligence?

Yes, it was. You might say that I stumbled upon intelligence when I set out to catalog the variety of abilities required by jobs. I had set out with the mistaken assumption that seemingly different cognitive abilities, such as verbal comprehension and spatial visualization, are largely independent. Wrongly assuming they are independent, I therefore also mistakenly assumed that occupations of very different types (clerical, technical, crafts work, social service, etc.) would necessarily call upon distinctly different mental abilities. I would not have made that mistake had I been familiar with the psychometric literature. As I soon learned, all mental abilities tend to come bundled together. In fact, they all correlate moderately to highly with the same underlying general mental ability factor, *g*. I was simultaneously learning from the personnel psychology literature that cognitive tests predict performance to some extent in all jobs and, moreover, that their *g* component accounted for virtually all their predictive value. Third, I had also become persuaded that test bias cannot explain the large average difference among American blacks and whites on cognitive tests. Schmidt and Hunter's meta-analyses was turning conventional wisdom in industrial-organizational (I/O) psychology upside-down on all counts, as was Jensen's 1980 *Bias in Mental Testing* outside I/O circles, General intelligence stood out as an important phenomenon, indeed.

I had just done a lot of work figuring out the distribution of jobs by ability level in the US economy, so I was curious what the larger implications of the mean black-white IQ difference might be. How much racial inequality in employment would we predict based on this black-white IQ gap, all else being equal, and how would the expected representation vary by job level? Personnel psychologists knew to expect disparate impact with individual tests in individual jobs. But no one had inquired into the magnitude or patterning of disparate impact to expect across all jobs simply because of black-white differences in IQ mean and variance. For example, black representation falls steadily as job level rises, which is often taken as self-evident proof of more racial discrimination in more desirable jobs. Public policy is often based on such claims.

I had already compiled data on the aptitude demands of different occupations and the typical intelligence levels of incumbents, so it was a simple matter to calculate what proportion of each race fell within those recruitment ranges and would, presumably, be cognitively eligible for them. Based on the two IQ distributions, I knew for statistical reasons that the per capita ratio of black to white eligibles would fall as jobs rose in difficulty and status level, but otherwise did not know what to expect. I was shocked at how disproportionate the ratios were, at both the top and bottom of the job scale—and also how closely they conformed to actual employment patterns. I included the analysis in a commentary I was invited in 1985 to write on an article by Art Jensen appearing in *Behavioral and Brain Sciences* (BBS). My commentary was eventually published (1987), but only after protracted correspondence rebutting the reviewers' and editor's objections. Up to that point, my submissions had always sailed through the review process, sometimes with no revisions at all.

That one little analysis, so simple and obvious, had clearly provoked anxieties among journal reviewers, but it would also instigate years of requests for assistance from personnel selection practitioners. The requests began with an invitation to speak at a small practitioner conference, but escalated into appeals to expose dishonest science at the highest levels of the discipline.

Disparate impact (or "adverse impact," both being legal terms for racial differences in pass or hire rates) was a huge headache for personnel psychologists. During the 1980s, many symposia at the annual SIOP conventions were devoted to it. Selection professionals were being pressed hard by employers and government enforcement agencies to come up with valid tests of applicants' job-relevant skills that did not have disparate impact, but nothing they tried seemed to work. All sorts of questionable practices were being promoted by consultants, who could earn big bucks by selling magic potions to legally beleaguered employers and government agencies. To me, it looked like a race to the psychometric bottom—less reliable, less valid selection procedures. The inevitable consequences of such so-called "improvements" include lower workforce productivity and *worse* disparate impact in promotions. Only academics had the freedom to write honestly about racial differences, and only those who had no interest in earning consulting fees were ever likely to consider doing so. Throw out those who remained unconvinced or skittish, and that did not leave many of us. Coming from a sociological perspective, I was most interested in the big picture, including the national politics affecting selection practices.

At that time, personnel selection psychologists knew a lot about tests and job analysis, but not much about the organization of human traits, including cognitive abilities. In 1985 the Personnel Testing Council (PTC) in Southern California held a conference on the "*g* factor" to help its selection specialists in industry, government, and the military better understand why they were having such trouble ridding tests of disparate impact. The group invited Arthur Jensen, Jack Hunter, Robert Thorndike, myself, and one other sociologist to discuss the general intelligence factor, *g*, and its relation to job performance.

The received wisdom in personnel psychology still held that intelligence could not dependably predict job performance because different jobs require different, independent abilities. These particular speakers were invited because they had been proving that wisdom mistaken. Jensen had recently reintroduced Spearman's *g*—the "*g* factor"—and shown it to be the psychometrically unitary, common core of all cognitive abilities; different mental abilities are not, in fact, independent. Hunter and Schmidt had just introduced meta-analysis to the field and, by using it, had demonstrated "validity generalization" for all cognitive tests in all jobs. That is, cognitive tests predict performance differences to some extent within all jobs, and cognitive ability is the best single predictor of job performance, especially when performance is objectively measured. They, Jensen, and Thorndike had all shown that *g* alone predicts performance almost as well as does a whole battery of cognitive tests. All of us were finding that *g* predicts performance better in higher-level, more complex work. Jensen had provided evidence that the extant black-white gap in average IQ reflects a difference in *g*, and I had estimated the pattern of disparate impact one would predict at different job levels owing to this average disparity in measured intelligence. Collectively, the speakers dispelled any mystery about why personnel specialists were failing in their assigned task of expunging disparate impact from their tests, especially their most predictive ones.

PTC members thought it a helpful conference, so I suggested publishing it. I had been on the editorial board of the *Journal of Vocational Behavior*, and was able to persuade its editor to bring out a special issue based on the conference. I was still working at Johns Hopkins, but would not be much longer. The director had given me notice earlier that year that the 1985-1986 academic year would be my last at CSOS.

Q: Why did he terminate your employment? How did that happen?

There was great camaraderie among the researchers at CSOS (Center for Social Organization of Schools) when I joined, but it depended on like-mindedness of a certain sort. For example, it was OK to be Marxist but not to favor any idea that could be construed as politically right-of-center. The more I dealt with intelligence or fairness in testing, the less comfortably I fit. Every week or so one of us would give a lunchtime seminar. One of mine was on the adult occupations of men with dyslexia, because I had been collaborating with some researchers at the Johns Hopkins Hospital (Gottfredson, Finucci, & Childs, 1984: Finucci, Gottfredson, & Childs, 1985). When I reported that high-IQ dyslexic men got high-level jobs, though ones not requiring much reading or writing (e.g., they became executives rather than doctors or lawyers), a black colleague accused me of insinuating that blacks don't get ahead because they "are stupid." The dyslexia study had nothing whatsoever to do with race, but her complaint vividly demonstrated that intelligence itself was off-limits. One need not link it explicitly with race, because an association would automatically be imputed. In another of my lunchtime seminars, on forms of test bias, one of the minority post-docs complained that I was racist when I classified race-based scoring of cognitive tests (race-norming) as, technically, a form of test bias. Another time, a white colleague ridiculed my interest in intelligence in a note taped in the foyer of our little building, a converted residence. It was all petty stuff, but it illustrates the manner in which groups set boundaries and enforce taboos by threatening disapproval and expulsion from the group. It is a powerful tool, not just because humans are quintessentially social animals, but because reputation is all in academe and politics.

It was around the time I was working on my BBS commentary when the CSOS director announced to our large team that my services, and only mine, were irrelevant to our next multi-year institutional application for federal funding. Because our salaries were paid entirely from such funds, this meant I was being fired. To his credit, Karl Alexander (a Sociology professor who participated in CSOS) voiced the otherwise unspoken: I was being ejected on specious grounds.

That same year, Bob Gordon and I had put together a symposium for the 1986 APA convention on the relation of IQ to racial differences in employment (my specialty) and crime (Bob's). We enlisted Richard Herrnstein, Charles Murray, Raymond Cattell, and Edmond Gordon for commentary (Gordon agreed but cancelled at the last minute). It was the first activity in our joint Project for the Study of Intelligence and Society, which has been aimed at establishing a "sociology of intelligence." It entails looking at how human variation in intelligence affects human life at all levels, from the individual, to the group, to the way societies structure themselves (see Gordon, 1997 for examples). CSOS researchers were routinely encouraged to issue press releases about convention presentations, but the JHU press office refused to release ours, at the director's request.

As a sidebar, Bob and I had to convince a skeptical Charles Murray over dinner after the symposium that there were, in fact, good data indicating that IQ tests are reliable, valid, and not culturally biased against blacks, just as Julian Stanley had had to convince Bob himself years back. Murray and Herrnstein would later join forces to write *The Bell Curve*, a magnum opus on the importance of cognitive ability in determining the class structure of modern societies.

Q: What led you to Delaware?

I was sorry to leave Johns Hopkins. The people there were sharp. Some, like John Holland and Julian Stanley, had become personal friends. I had known Julian from when Gary Gottfredson was a student in his department, and he and Bob were good friends. Julian was always energetic, always enthusiastic about his work, and could bend your ear about it for some time. He was also a kind and generous man, ever the Southern gentleman. He routinely sent interesting articles to colleagues, commented quickly on any manuscript I sent him, was invariably encouraging, and even gave our daughters savings bonds. If anyone doubted his avuncular concern for the students in his talent search programs, they need only have noted many of their names, or their parents' names, in the guest book at his 2002 wedding. Julian was twice a widower and now marrying a very young-looking and attractive fellow octogenarian. Working at Hopkins had been idyllic in ways. As a research scientist, I got to spend 100% of my time working with data. Actually that's not quite right. I had to spend months every year writing grant proposals, and you had to tailor the proposals to your sponsors' guidelines and interests. So you either had to piggyback your interests onto theirs or else pursue them on your own time.

I was within weeks of unemployment in 1986 when I was invited to interview for a one-year visiting professorship in the Department of Educational Studies at the

University of Delaware, where I would teach the sociology of education. The invitation came from the sociologist I had enlisted to speak at the 1985 PTC conference. I had (and still have) published virtually nothing in sociology proper, but it was largely for my sociological work that I was invited to apply. I had quickly stopped submitting manuscripts to sociology journals because I would get responses like "I just don't believe the world works that way." But I did have a chapter on the origins of the occupational status hierarchy in the 1985 annual edition of *Research in Sociology of Education and Stratification*, edited by Alan Kerckhoff. It would have been an important contribution to sociology had the field not ignored it so thoroughly, but my colleague appreciated its power to explain a crucial but neglected puzzle: what is the basis of the occupational status hierarchy and why is it virtually identical in very different societies?

The first question was usually waved off with some vague reference to "power," but much theorizing in the field rested on the answer. As I noted earlier, sociologists seemed to be assuming that occupations differed only in the socioeconomic rewards they bestowed on incumbents or the power they allowed them to exercise. Differences in skill requirements, if they existed, were irrelevant. Some argued that, with sufficient training on the job, virtually anyone could perform virtually any job if only there were not arbitrary social barriers blocking some people's entrance. One major theorist viewed educational credentials as one such barrier, arguing that physicians, for instance, should work their way up from being hospital orderlies. My chapter triangulated evidence from job analyses and personnel testing studies to show that there really is a functional basis to the occupational hierarchy and that higher-level jobs really do require higher levels of intelligence for good performance. Occupations are most distinguished by their cognitive complexity, which means the occupational hierarchy reflects a *g* factor among jobs' demands.

My chapter also drew on signaling theory in economics to explain how employers use educational credentials as a valid though fallible signal of worker capability (general intelligence) when they have little time or information by which to assess job applicants. The signaling function could explain a puzzle: years of education predicts who *enters* the most prestigious jobs better than does intelligence, but intelligence predicts who actually *performs* them well whereas education does not. Moreover, by conceptualizing jobs as flexible constellations of tasks, as job analysts do, I could also explain how the occupational hierarchy could expand or contract, and thus evolve, depending on how reliably workers were sorted by intelligence to differentially difficult jobs: more reliable sorting would allow and induce more differentiation by intelligence demands. It was a novel and powerful idea, my host explained to the assembled faculty.

The job was over an hour away and I had to leave before my children woke up to get there for my morning classes (I would eventually stay over one night a week), but it was a paying job. And it might lead to a tenure-track position, which it did the following year. Moreover, I could carry out whatever research I wanted without having to worry about getting grants to pay my salary, in contrast to what was the case at Johns Hopkins. Such luxury! The department was interdisciplinary, which I liked, because I have never fit neatly into any particular discipline (a problem when I had been looking for a job). It meant that Bob had to take over more childcare, but he has always been very supportive of my career, even after we separated. I would rush back by 6:00 pm, however, to pick up our daughters at day care. I was a highly valued member of the department for three years, and the dean asked me if I would consider chairing the department.

During those first three years at Delaware I produced two special issues of the *Journal of Vocational Behavior (JVB)*, both based on Personnel Testing Council (PTC) conferences of the same names: "The *g* Factor in Employment" (1986d) and "Fairness in Employment Testing" (Gottfredson & Sharf, 1988). I have already mentioned the first. I enlisted Lloyd Humphreys, Leona Tyler, Richard Arvey, and Robert Linn to comment on the papers in the first conference. I had no clue what they would say, but was amused when Lloyd wrote about my paper (1986b) that "If anything she has been too restrained [in her] evaluation of the black-white difference on the *g* factor." Had I known Lloyd better then, I would not have been surprised. He was as honest and earnest as they come when it came to exploring the social challenges of racial differences in abilities. He is also an example of a hard-core empiricist; he could change his mind in the face of data, which was evident in his commentary.

The JVB was a small-circulation journal directed to vocational psychologists, however, so I purchased and mailed 6,000 copies of the special issue to individuals on various academic and professional mailing lists, but mostly in I/O psychology. It was part of Bob's and my Intelligence Project to circumvent the usual disciplinary barriers to disseminating "controversial" scientific articles. And it worked. I gather it created quite a buzz in I/O circles, with recipients asking each other "how many did *you* get?" (The mailing lists overlapped.) Virtually overnight, the *g* factor became a staple concept in I/O circles. I would use the strategy again in two years with the second special issue of the JVB, which I co-edited with personnel psychologist and expert legal witness James Sharf. That volume focused on the fairness and legalities of using *g*-loaded tests in hiring, as well as the federal government's use of race-norming in reporting scores on its employment test (the General Aptitude Test Battery, or GATB) to participating employers. The contributors were again major protagonists in the debates over test fairness, including Frank Schmidt, Jim Sharf, lawyers Clint Bolick and Richard Seymour, along with others. The JVB editor had been criticized for publishing the 1986 volume and therefore balked at publishing a second, but she eventually agreed nonetheless.

Q: You were going great guns at Delaware. You must've found the atmosphere to your liking. Is this when you decided to make it your permanent home?

Yes, I liked the University. Most of all, I could finally devote my time to topics that interested me, especially intelligence and the social dilemmas in employment testing. I did not take the supportive environment for granted, however. It was reassuring that my dean had rebuffed complaints from the affirmative action officer in 1987 that he was hiring an "academic racist," but I applied for tenure and promotion to full professor as soon as allowed, in 1988. It was fortunate that I did so, because I would never have gotten tenure at UD after that time. Despite a very strong recommendation from the department, I almost did not get it that year. The colleague who had invited me to apply for the job at UD was chairing a higher-level promotion and tenure committee, and he apparently persuaded it to recommend against tenure and promotion. Upon appeal, the university granted me tenure but not promotion, though encouraged me to come up for promotion again the following year.

In the meantime I organized the department's yearly speaker series. I recruited John B. Carroll, Hans Eysenck, Robert Gordon, Lloyd Humphreys, Arthur Jensen, Richard Lynn, and Robert Plomin to speak on the educational implications of intelligence differences. The series was exceptionally well attended, but it apparently provoked till-then quiescent opposition to my presence on campus. A storm was gathering.

As I mentioned, my department was interdisciplinary, and the colleague in the next office, Jan Blits, turned out to be a political scientist and former civil rights worker in the days of segregation in the South. He wasn't afraid to disagree with the crowd when he thought it mistaken—a rare find. He still stuck by the principles for which he had fought in the civil rights movement and that had inspired me. It happened that the National Research Council (NRC) came out with its judgment on race-norming in 1989 (*Fairness in Employment Testing, Hartigan & Wigdor, Eds.)*, before I applied again for promotion. The NRC report not only recommended race-norming, but also claimed it was scientifically justified, which was not true. Many I/O psychologists were outraged. Lloyd Humphreys wrote in *Science* that it was statistical legerdemain.

The committee was giving the green light for racial quotas in the name of science. Anyone is free to advocate quotas, but not to camouflage their politics as science. Jan Blits and I published two articles analyzing the report (Blits & Gottfredson, 1990a, b), in which we demonstrated how its science was politicized and, in one, why we believed that racial quotas would render blacks permanently unequal. The committee had played up the social benefits of hiring blacks under lower standards than whites while glossing over the costs. One of the most insidious costs would be to greatly increase the ratio of blacks to whites among new hires who would later perform unsatisfactorily on the job. As the committee's own analyses indicated, most of the unsatisfactory performers under race-norming would be black: the practice would color-code failure. I also wrote an essay for the *Wall Street Journal* ("When job-testing 'fairness' is nothing but a quota," 12/6/90, p. A18) in which I exposed language in the draft civil rights bill that would effectively mandate race-norming, as well as the fact that the U.S. Equal Employment Opportunity Commission (EEOC) was already threatening to sue companies if they did not race-norm to eliminate disparate impact in hiring. My second claim was based on over-the-transom EEOC memos. This was one of the cases where practitioners had approached me to help blow the whistle. The essay caused a furor and Congress banned race-norming in employment when it took up the bill again in 1991.

Q: You said you planned to come up for promotion again. You got caught up in a big controversy at your university at about that time, didn't you?

Yes, there was a multi-pronged effort to destroy my reputation and cripple my research. Shortly after the 1989 fall semester began, a faculty-staff group led by a Linguistics professor pressed the university administration to block all funding from the Pioneer Fund. The call ostensibly had nothing to do with me, despite my being its only grantee at UD. Their claim was that the Fund was racist, fascist, anti-semitic, and contrary to UD's commitment to diversity. The university had been dealing poorly with minority discontent on campus, so I became a convenient scapegoat. The state and campus newspapers depicted me as racist and a Nazi, and the UD African American Coalition denounced my work as dangerous to both

African Americans and the university. Concurrent with this, my department denied my second bid for promotion. Its ostensible reason was that my two, new publications on race-norming revealed a "tendency to misrepresent." The faculty vote had flipped from almost unanimously favorable the year before to almost unanimously negative this time. My chair, also flipping to recommend against promotion, said I had set civil rights back 20 years. The funding and promotion cases reinforced each other: I was tarred as engaging in dishonest, evil pseudoscience.

It was a really bad time. Bob and I were separating and I would be moving shortly to Newark (Delaware) with our daughters. He would remain an involved father and good colleague, but it was very difficult, especially for the children. Department colleagues shunned Jan and me, and even friends among them believed the charges. Faculty would cross the street to avoid us and avert their eyes when passing in the hallway. News coverage was ugly. The Sociology Department stopped giving their majors sociology credit for taking my sociology of education course. My formerly supportive dean started searching for mistakes for which he could punish me and, eventually, for pretexts to break my tenure. My chair reclassified our race-norming articles as non-research during our annual evaluations. The university asked UD's Faculty Senate Research Committee to investigate the Pioneer Fund. It held hearings and in the spring of 1990 recommended blocking all funding from Pioneer, which the outgoing president did. What were the grounds? That the Pioneer Fund supported the sort of research that Bob and I did (a fatal rationale for UD in national arbitration, especially since the committee had inadvertently quoted directly from it). The Board of Trustees backed up the decision, responding to the president of the Pioneer Fund that even if the charges against the Fund were false, mere perceptions were enough.

Press coverage became nastier as it became national. The worst was when several journalists who pretended to be writing stories on academic freedom (even seeming to befriend my children) published ugly hit-pieces such as "Professors of Hate." One magazine doctored its photos to make me look like a witch. The Black Student Union (BSU) disrupted one of my classes, photographers in tow. When the BSU threatened a boycott against the College of Education over my teaching, the dean asked to inspect my students' papers to see if I was teaching racist content (I refused, which was yet another mark against me). This sort of thing went on for three years, day in and day out. Jan was also threatened in various ways, partly because he had been first author of the two publications on race-norming, but mostly because he helped me fight back. He had planned to come up for promotion, but his prospects now seemed dim.

Jan had gotten Bob and me organized to fight back from the first day. We investigated all the charges against the Pioneer Fund, in detail, for months. None of them held up. It was all a pastiche of innuendo and falsehoods, much of it quite vile. In the process, we got to know the Fund's president, Harry Weyher, very well. He was an honorable man, who as a young lawyer had worked for John Marshall Harlan, later a U.S. Supreme Court Justice. Harry was committed to funding top scholars who could not get funding elsewhere for research on human differences. For example, he funded Thomas Bouchard's study of separated identical twins before others would. He helped support Art Jensen's work, and Lloyd Humphreys'. Bob and I first went to the Fund in 1986 when we needed support to bring the participants to our 1986 APA symposium. Because I had been unable to get funding elsewhere, the Fund had become a lifeline for me by 1989. The funding controversy at UD was seeded by materials from an academic at Ferris State University who seemed intent on crippling the Fund by picking off its major recipients, one by one. Other institutions never obliged him, but UD did.

I appealed the department's promotion decision and pressed to get my appeal heard by the Faculty Senate's Welfare and Privileges Committee, which was a slow and uncertain process. The department flip-flop had been engineered by the man responsible for my not getting promotion the year before. It was in retaliation for my rebuffing his sexual advances (I wasn't the first), but my complaint went nowhere. The vice president who decided sexual harassment complaints was the very same administrator who was in charge of defending UD against my funding and promotion appeals. As a member of the department, Jan had seen the outside letters in my promotion application, so he knew that the promotion and tenure committee had misused them. Their letter recommending against promotion made the only negative review out of nine seem like four, and it included none of the highly positive comments from the other eight. The dean tried unsuccessfully to keep those overwhelmingly positive peer reviews from the university appeals committee, so its members were outraged when they eventually got to read them just days before the hearing. They were further outraged when my chair and members of the promotion committee refused to attend the hearing, as required.

We pressed for national arbitration of the funding decision, which the faculty union could grant. Thankfully, UD's American Association of University Professors (AAUP) Grievance Officer, George Cicala, was an unwavering ally. (The AAUP president had been on the committee urging that Pioneer monies be banned.) Our young pro bono lawyer, Steve Jenkins, risked his career and prospects for making partner by taking on my case. Jan and I spoke with him nearly every day for months on end, often at length. He is a real hero, and I still do not know how to repay him for his years of commitment. Colleagues outside the university such as Edwin Locke rallied support by putting out a call for colleagues to write letters to the UD administration. Many in the I/O community did, regardless of which side of our debates they had been on. Though not in my field, ex-APA president Robert Perloff made a point of being publicly supportive at official APA functions.

Unlike Art Jensen's troubles 20 years earlier, mine had come from inside my university. Jensen's university had come to his aid, partly by providing him a bodyguard and screening his mail for bombs. They protected his ability to teach and do research. My troubles were hardly as severe. There were never any physical threats to me, nor certainly any of the prolonged national frenzy. But my case illustrated a new trend in the suppression of unpopular research: it was up close and personal. I was prepared for trouble from outsiders, but had been demonized and hobbled by those closest to me.

I doubt that my UD colleagues or administrators expected me to fight back. I am a soft-spoken woman, so they may have assumed that I am weak. I had also been slammed hard simultaneously from different directions: funding threatened, promotion denied, character smeared in the press. I got the first whiff of sexism in my career when the chair of the promotion committee came to me after the department meeting and condescendingly said he knew I must feel hurt and disappointed. I told him I was angry. Jan and I went on the offensive. We gathered evidence, some of the most valuable in memo wars with administrators—which we developed as an art. We did our homework, whereas our tormenters were sloppy in their confident right-thinking. The only way to win at our university was to show that its rules and regulations were not being enforced. We therefore learned them inside and out in order to use them to our advantage. Most broadly, we were engaged in a set of concurrent, interdependent chess games with various administrators and agents: you had to think many moves ahead, anticipate each opponent's moves, entice lower-level administrators into bad moves that would force the hands of higher-level ones. We became more media savvy, which helped turn press coverage in our favor. We also had the advantage of being in the right.

Every independent panel eventually ruled in our favor. On May 31, 1991, the Faculty Senate committee hearing our cases concluded that our department chair's and promotion committee's evaluations of our joint work had been unfair, specifically, that they relied on a single referee of self-admitted political bias, suppressed directly contrary evidence in all the other reviews, violated my academic freedom in so doing, and presaged similar unfairness and bias toward Blits when he came up for promotion. In short, the committee found our evaluators guilty of committing the scholarly crime of which they had falsely accused us: "misrepresenting the views of others." The hearing panel also concluded on July 21, 1991 that the Sociology Department had violated my academic freedom by voting to discontinue cross-listing my course on ideological grounds. The national arbitrator ruled on August 9, 1991 that the University had violated my academic freedom by "doing precisely what it said it would not, and should not do—… delving into the substantive nature of grievants' work."

Some department members started to fear losing their homes in potential lawsuits. The chief miscreant agreed to a phased retirement that prohibited contact with students or participation in personnel decisions, and he approached us out of the blue with a settlement offer. The university reached an out-of-court settlement with us on April 29, 1992, which included a year's paid leave of absence—but only after the dean had put the university in further legal jeopardy by escalating his campaign against us after we won the funding arbitration. It also specified that Jan's bid for promotion to full professor would be monitored by an observer and bypass the department altogether, which infuriated our colleagues. Responding to our case, though tardily, the national AAUP issued a statement that denying funding on ideological grounds violated academic freedom (*Academe*, 1992, September-October, p. 49). The term political correctness had not yet been coined when our controversy began, but it was later used to describe it. Historian Alan Kors, who had been personally supportive, wrote in his 1998 book with Harvey Silverglate (*The Shadow University*) a scathing account of the whole episode, naming names. It is also described in Morton Hunt's 1999 *The New Know-Nothings: The Political Foes of the Scientific Study of Human Nature*, as well as various media accounts. The Linguistics professor who started it all eventually self-destructed, his unstable behavior getting him "fired for cause" in April 2007 from a college presidency.

Art Jensen's wife, Barbara, had warned me at the 1985 PTC meeting that the path I was about to tread could be very costly. She was right. (Sadly, she passed away on June 10 this year [2007].) Most of all I regret the difficulties it caused my children. I even worried for their safety. I could not protest that my children were being hurt, however, because then I would have been called a bad mother too.

I could not complain that the controversy was interfering with my work, because I would have been accused of not doing my job. I became emotionally and physically exhausted but could not appear vulnerable. I had to hold my head high, seemingly unbowed. Still today, fifteen years after the uproar ended, faculty from other departments will, after working with me on some committee, confide that I am actually nothing like what they had imagined.

What did I learn from the experience? Most of all that unusual situations test people's characters. Old friends may betray you, but total strangers come to your aid. It is hard to forecast who will play the goat and who the good Samaritan. The single most touching moment was one that initially alarmed our secretary. A man entered our office suite one day who was obviously neither student nor faculty. He was roughly dressed and carrying something close to his body. He asked for my office. When I looked up, he thrust a red rose at me and said thank you, then walked away. It turned out he was a grounds worker at the university who wanted to show his appreciation for my steadfastness.

Q: You have obviously had to work with people who tried to force you out of UD. Have you reconciled your grievances with them. What kind of relations do you have with them now? How did this evolve?

A. Jan and I had brought formal grievances under the terms of our union contract. Both my department and the University had been judged to have acted improperly. We had won a war that people thought us crazy even to fight. I just wanted to get back to work, unmolested. It felt good to be publicly vindicated, though I doubt it changed any minds in the department. Many were angry. They felt the University had betrayed them in settling with us and appointing a monitor for Jan's promotion. They weren't in the mood to reconcile. The UD administration refused to do anything to normalize us, and may have given us a year's leave of absence partly to ease tensions by separating us from departmental affairs. Antipathy to working with us may also be why, for years, successive chairs put us on committees only outside the department. The dean also threatened retaliation. We were delighted when the University moved all our teaching and most of our service outside our college, to the Honors Program. So we really were not working much with departmental colleagues for a long time. Overt hostility gradually faded with time, but I never assume that it cannot arise again in an instant. We have proper working relations with departmental colleagues today, even those who were judged to have acted improperly toward us, but I have no illusions that they would ever stand up for us.

On the other hand, it is gratifying that the UD Faculty Senate has kept electing both Jan and me to chair some of its major committees. For instance, Jan has run the Welfare and Privileges Committee, which, among its other highly sensitive duties, hears all cases in which the university seeks to terminate a tenured faculty member's employment. (It was the Senate committee that judged three of our cases.) For many years I chaired another committee requiring a reputation for absolute fairness, because it awards the university's highest honors for faculty excellence, which carry cash prizes up to $10,000.

Q: Where did your research program stand at the end of this three-year controversy?

A: I had not been able to do much during those three years. I had been invited to do some pieces on workforce diversity, which had just come into vogue (1992, 1994b, 1997c). Affirmative action hiring was coming under increasing fire, and diversity provided a new rationale for racial preferences. Diversity hiring was not meant to redress injustices or advantage minorities, it was said, but to improve the company's bottom line. From what I could discern, it was old wine in a new bottle.

It was hard to get back up to speed, to take up where I had left off. I literally felt sick trying to read articles about intelligence, as if I had been negatively conditioned. But there was no ignoring the race-norming issue. Despite the practice now being illegal, the NRC panel was still defending its recommendation against my criticisms, so I would be invited to respond (e.g., 1994d). And as far as some personnel psychologists were concerned, the ban only made their job harder. By artificially equalizing the scores of blacks and whites, race-norming had made it possible to avoid disparate impact while still using valid selection devices. Predictably, the psychometric slide to the bottom accelerated, prodded by the Employment Discrimination section of the US Department of Justice (DOJ). Now unable to demand that tests be rescored to erase the appearance of race differences in relevant skills and knowledge, DOJ would begin pushing the profession to corrupt the tests themselves.

In 1996, the president of a private test development company sent me a very long technical report for a new police selection test, administered in 1994 to 30,000 applicants in Nassau County, Long Island (NY). He sent it to two other members of the Society for Industrial-Organizational Psychology (Division 14 of the APA, hereafter SIOP) as well, including Frank Schmidt. He blacked out the names of the developers and simply asked us for our evaluation. I read it and was appalled. The new test battery succeeded in virtually eradicating disparate impact by eliminating all cognitive demands except reading above the first percentile of police incumbents. Of the many tests administered to those 30,000 applicants in 1994, only eight personality scales and reading at rock-bottom level were actually counted toward their scores. The report claimed, improbably, that the new battery predicted job performance *better* than previous ones, but the claim rested on a series of mistaken and questionable statistical procedures. DOJ was already pressuring police departments around the country to adopt the new test battery, lauding it as "state-of-the-art." When I finally learned who had headed up the project, that was the biggest shock of all. They were among the top members of the profession, some of them past presidents of SIOP.

Investigating further, I heard disturbing stories about the uncommonly high proportion of passing candidates who had criminal backgrounds or suspicious gaps in their personal histories, while lawyers and other highly educated individuals (drawn by the very high salaries) had failed it. Much later I would hear about the extreme difficulty the police academy had in training the new hires, as Frank Schmidt and I had predicted. I spent months studying the report. Technically, it was a complicated, multi-stage project, and I had to figure out how they had seemed to accomplish the impossible. I also interviewed test takers, met with police union officials, got technical reports for the county's prior much-litigated tests, collected relevant court records, and so on.

It was a tricky situation, however. Not only had the new test been developed by leading lights in my adopted field, but DOJ had been a full partner in its development by contributing half the consultants. If I attempted to make my case through the usual publication channels, I risked being crushed before the field took my analyses seriously. I therefore decided to publish my conclusions first in the *Wall Street Journal* (1996c). Blits helped me come up with a term that would encapsulate for a general audience what they had done. He imported one from political science: they'd *gerrymandered* the test's content, just like political parties gerrymander voting districts to sway results in their favor. I later wrote lengthy analyses for a police magazine and *Psychology, Public Policy, and Law* (1996d).

The test developers involved were livid, and accused me of unprofessional behavior by not going to them first. They circulated rebuttals, each of which I answered. The next SIOP convention featured a debate, which arrayed five of the test developers or their supporters against me. The organizer had refused to add Frank Schmidt to the panel, and then reneged on his promise to call on Frank first from the audience. Looking across the ballroom, it seemed that the whole convention had come to watch the conflagration. I gather that one member of the test development team was so frightened of me that he did not show up to the session at which he was to receive an award. But I had been afraid for a while too, because I had infuriated some very powerful people, including the head of the employment discrimination section of DOJ. Behind the scenes, he was trying to discredit my analysis by smearing me as a pseudoscientific racist. When I testified before a congressional committee looking into the Nassau County, NY police test, Reps. John Conyers and Maxine Waters came well armed with his slander. But like our tormentors at UD, they were sloppy with their facts and were reduced to embarrassed silence.

The Nassau County test became an example at SIOP of what *not* to do, but other test developers were already lining up to satisfy DOJ in some other way. And Nassau County was stuck with a poor pool of police recruits, whom the union would now have to stand behind.

Q: You began to write even more on intelligence research.

A: All the questions I had been pursuing yielded answers that led me in that direction, right from the beginning. The four developmental stages in my career aspirations theory were driven by children's gradual cognitive growth, specifically in the ability to perceive different dimensions of people and occupations. I have already traced the other questions for you, from understanding the ability requirements of jobs for counseling purposes to understanding patterns of inequality in employment, especially by race. But two other forces were also taking me in that direction. One was the media backlash against *The Bell Curve* (Herrnstein & Murray, 1994), published in 1994. The other was curiosity about the phenomenon itself. What is intelligence, really? What is it good for, and why, exactly? It is not enough to show skeptics correlations between IQ and some valued outcome, even hundreds of them. You need to explain why those correlations exist, to open the black box of what intelligence is and does, and how. In my mind, only by making a case for the common-sense, transparent plausibility of the role of intelligence in everyday life could I effectively explain how individual differences might cause social inequality, not merely reflect it.

Q: How were you affected by The Bell Curve controversy?

A: *The Bell Curve* had pushed intelligence onto the front pages. Some journalists were seeking balance in their coverage of the book. They must have been referred to me as a willing expert for the defense, as it were, especially on race. It was odd to suddenly be interviewed as a respected authority on IQ rather than the wicked scientist—and for holding exactly the same views. Only a small slice of the book actually dealt with race, but that is what the controversy swirled around.

Now, it is not as if journalists had never interviewed me about race and IQ. Few people realize that it is risky for journalists themselves to give credibility to IQ, especially the sort of research Bob Gordon and I were doing. They are subjected to an editorial review process just as we academics are. No matter how high up they were in the news organization, the journalists who interviewed us tended to get flak from above if they took us seriously. The tenor of the piece might be changed, or the headline be made to say the opposite of the text. The piece might be spiked altogether. For example, *Forbes* senior editor Peter Brimelow wrote a feature article about my work when the 1988 JVB special issue appeared in print, but his article was killed at the last minute. The *Village Voice* reported that it caused such an uproar at the magazine—a "copy desk revolt"—that Steve Forbes himself had to step in. Dan Seligman periodically wrote about IQ matters in his column at *Fortune*, and he also wore out his welcome at his magazine. I know science writers whose editors forbid them to write about the topic, unless critically.

A bit of the *Bell Curve* coverage was excellent, such as the first review in the New York Times Book Review, a feature in Newsweek, and two half-hour TV segments on Ben Wattenberg's *Think Tank* in which he had Doug Besherov, Glenn Loury, Christopher Winship, Roger Wilkins, and myself probe the issues. But most coverage was rubbish. Snyderman and Rothman's (1987, 1988) survey of journalists and IQ experts had shown the two groups tend to hold opposite views of the facts on intelligence. This latest media frenzy reinforced my sense that as the science had become more conclusive, the attempted refutations were becoming shriller. Much was ad hominem. Herrnstein and Murray had cited articles by various Pioneer Fund grantees, such as Bouchard, Jensen, and Richard Lynn, as would be expected of any scientifically credible treatment of the topic, but that allowed critics to drag out the lurid charges against the Pioneer Fund. The most condensed piece of vitriol was a really despicable segment by ABC news anchor Peter Jennings on the evening news. It highlighted the smears about the Pioneer Fund and even ran footage of what appeared to be Nazi death camp doctors. It was sickening. I cannot tell you how dishonest his team had been. Bob Gordon would later write a detailed analysis dissecting the perfidy in those eight minutes (1997b).

ABC News had interviewed us both at length. I had traveled to New York City, where Jennings' team interviewed me on camera for hours. They were clearly surprised and frustrated by my answers, which I often turned into mini-tutorials. They used none of it for the broadcast. I suspect they had wanted me just for a mug shot. My interviewers had clearly expected me to look like the witch in the doctored magazine photo. They did not recognize me when I stepped off the elevator and were visibly startled when I introduced myself.

Like other intelligence researchers, I was disturbed by the bulk of the media's grossly distorted coverage of intelligence research. Our past experience was that letters to the editor defending unpopular research or researchers rarely got published. I therefore proposed an opinion essay to the *Wall Street Journal*. The editorial features editor at the time, David Brooks, suggested an alternative: a short statement by 10-15 experts describing the knowledge they considered scientifically mainstream. What I sent him, "Mainstream Science on Intelligence," had 52 signatories and itemized 25 ABCs of scientific knowledge about intelligence. It was all very basic stuff to us, though it clearly surprised Brooks because he commented something to the effect that "it sure wasn't wimpy." I submitted the manuscript with the understanding that the *Journal* could not edit even a word of it and that it would appear later as an editorial in the journal *Intelligence* (Gottfredson, 1997b). Although its publication was received with deafening public silence, it was widely disseminated. Murray was not the only one thrilled by its publication. Academics and others could now point to a short, simple, authoritative statement that backed them up scientifically for holding supposedly "fringe" views about intelligence. Like the two JVB special issues I had put together before, the statement gained extra influence by joining the voices of diverse, respected scholars.

As a result of writing the Mainstream statement, the editor of *Intelligence*, Doug Detterman, asked me to put together a special issue of the journal to address the controversy in some more extended way. The result was "Intelligence and Social Policy" (1997a). Once again I invited top scholars, including John B. Carroll, Robert Plomin, Lloyd Humphreys, David Lubinski, David Rowe, and Bob Gordon, and then distributed several thousand free copies. I had searched in vain for someone who could write about the value of intelligence in everyday life, so ended up researching the issue myself (1997d). I set out to explain why *g* matters in daily affairs, not just in school and jobs, and it is my second most-cited article. Three of the articles, Bob's, mine, and David and Lloyd's later won Mensa awards for excellence in research.

Q: Some people see you as the public spokesman for g. How did that evolve, and did it involve Art Jensen in some way?

A: I had discovered early on that if you write and speak about intelligence differences, people are apt to challenge you on all aspects of the topic—from psychometrics to genetics. So I had been trying to educate myself more broadly. Relevant or not to your work, you have to be able to field questions about anything the critics might raise. You also have to understand their claims and evidence better than they do. Bob Gordon had been important in that self-educative process, because he is an expert on test bias as well as the relation between IQ and crime. So had Art Jensen. He generously commented on all the manuscripts I sent him, as did Julian Stanley, Tom Bouchard, Frank Schmidt, and others. Only after such people checked them out did I feel confident I had not made some embarrassing error.

Speaking of opportunities to be embarrassed! Art stayed with Bob and me when he came to speak in my 1988-89 lecture series at UD. I had only briefly met him before. Bob knew him, so I asked what he thought Art might like for dinner. He said Indian food. I had eaten a lot of it in Malaysia when I was in the Peace Corps, and after returning to the States I learned to cook it by finding cookbooks that reproduced it properly. It involved grinding lots of exotic spices, and the like. Only after Art arrived did I learn that he was a master of Indian cooking. I heard that he later joked that it was my self-taught Indian cooking that convinced him I had high *g.* I enjoyed the irony of being intellectually regarded for an activity that feminists often wrongly dismiss as demeaning. I also got some recipes and pointers from Art when I visited him and Barbara this spring.

But back to your question. I had done a lot of reading about the psychometrics and genetics of intelligence, but did not know much about the field as such. Julian Stanley had referred the *American Scholar* to me when it wanted an article on "what do we know about intelligence?" I got all back issues of *Intelligence* to see how research and ideas had been evolving. By the way, the *American Scholar's* editor, Joseph Epstein, got some flak for publishing the piece he had invited (1996e) and a contrary piece by Robert Sternberg soon appeared. *Scientific American* also asked for an article on the *g* factor for its Winter 1998 issue devoted to intelligence, presumably as balance for articles by Sternberg and Gardner on their conceptions of multiple intelligences. The *Wilson Quarterly* asked me to address the educational relevance of single vs. multiple intelligences views of intelligence (2004c). That piece, "The *g* Factor and Schooling," won a Mensa press award. I also did entries on intelligence and practical intelligence for various encyclopedias. These pieces ranged from straight coverage of facts about intelligence to ones including observations about the "democratic dilemma" that intelligence differences create (especially, that equal treatment does not produce equal results).

Other editors requested pieces on the IQ controversy itself, which led to "Egalitarian Fiction and Collective Fraud" (1994a) and "Equal Potential: A Collective Fraud" (2000a) in *Society*, "Skills Gaps, Not Tests, Make Racial Proportionality Impossible" (2000c) in *Psychology, Public Policy, and Law,* "Pretending that Intelligence Doesn't Matter" in *Cerebrum* (2000b), and "Suppressing Intelligence Research: Hurting Those We Intend to Help" in the book *Destructive Trends in Mental Health: The Well-Intentioned Path to Harm* (2005b). The latter piece dealt with the furor over Arthur Jensen's work, and I have also contributed to volumes honoring him (1998a, 2003b).

During the 1990s I started getting invitations from fellow academics to write about the implications of intelligence differences for schools, several for gifted education in particular (2001). The latter might sound like taking coals to Newcastle, but intelligence is a touchy subject in gifted education too. There has been a move in recent decades to "democratize" gifted programs, and the methods and consequences of doing so are much the same as those seen when employers reduce cognitive demands in order to hire a more diverse workforce. I wrote a chapter on those trends ("Realities in Desegregating Gifted Education," 2004b) for Diane Booth and Julian Stanley's 2004 book on multicultural challenges in gifted education, *In the Eye of the Beholder*. After receiving my invited contribution to a different book on gifted education, one co-editor had to convince his shocked collaborator that I really knew what I was talking about. Other invitations asked that I speak directly to the implications of racial-ethnic differences in IQ for schooling in general, and they varied from the highly empirical and technical for graduate-level instruction in school psychology (2005a) to more discursive overviews for undergraduates (2006b). So, I have served somewhat as a resource for scholars seeking straightforward analyses of these contentious subjects.

As sociologists, Bob Gordon and I have always been interested in how societies react to and structure themselves around their members' differences in intelligence (e.g., see Gordon, 1980, 1988, 1997a). The persisting controversies over intelligence are part of this general phenomenon, what we dubbed the sociology of intelligence. As a long-time participant-observer of them, I have drawn on that experience in articles on how political and social pressures affect the field and public perceptions of it. For example, scholars are rarely censored outright, so how do politically incorrect views and results actually get burdened and suppressed? How is the taboo against looking into the genetics of racial differences in intelligence enforced? How are falsehoods about intelligence made to seem true, and the truth made to seem false? My most recent book chapter ("Logical Fallacies Used to 'Discredit' Intelligence Testing," in press) deals specifically with that issue, while some of the articles I have already mentioned, plus others, describe the social mechanisms by which politically incorrect research is suppressed (e.g., 1994a, 1996a, e, 2007a).

Then there is the question about whether such taboos are good or bad. Maybe it is in a society's best interests not to know or talk about certain things, or to keep certain knowledge within a select "priesthood." Perhaps it is more ethical to speak benevolent lies than "dangerous" truths. I hear that a lot, though it is rarely stated so baldly. Some truths are unpleasant and discouraging, to be sure, but why, exactly, are they too dangerous for others to know? Might not ignorance be more destructive? I have addressed the dangerous knowledge presumption most recently in a commentary in *Perspectives on Psychological Science* (2007a): "Applying Double Standards to 'Divisive' Ideas." It focuses a spotlight on how ideological pressure is exerted in the name of scientific caution, and it pushes critics to back up their assertions.

It is unhealthy for both a science and its host society to be so at odds. That's the reconciliation I have concerned myself with. Right now, if you venture outside the field of intelligence, it is like stepping into Alice's Wonderland. Everything is topsy-turvy. True is false, and off with your head if you say otherwise. Simply reciting the evidence is not enough when popular wisdom is diametrically opposed, especially when so many people are so emotionally invested in it. I therefore pay a lot of attention to the emotions and common misconceptions to which critics appeal. Both have to be appreciated when trying to educate people about the evidence and what it means and doesn't. When I write, I am always thinking about how people will receive it. Not to soft-pedal anything, but to prevent predictable misreadings.

For example, if you state that people's IQ scores are *stable* over time or highly *genetic* (both true), many people will hear you claiming that intelligence level is *fixed* in stone from birth (false)—unless you anticipate and correct that common misunderstanding. Or, they may assume that *g* is just a narrow academic ability, which critics have encouraged them to believe, unless you explicitly explain otherwise. I therefore treat everything I write as a pedagogical opportunity, for example, by providing implicit definitions in the way I phrase things or by explicitly stating what I am *not* saying when emotions are likely to impede understanding. Or, conversely, as in my in-press chapter on anti-testing fallacies, I try to teach a general audience the basic facts about *g* and how tests are constructed and validated in the natural course of explaining how the fallacies work. It was a complicated exercise to do all that at once without seeming pedantic. I also spent many weeks distilling the welter of fallacies into a dozen fundamental categories and developing labels that capture their essence. Otherwise, my analysis could have come across as just an eye-glazing list of nit-picks. Twenty years of teaching undergraduates about intelligence has been a great laboratory. So have many hours in conversation with journalists. Both have helped me understand the variety of misconceptions and emotional stumbling blocks people have, and also given me much opportunity to test ways to minimize them.

That chapter illustrates another sort of pedagogy as well: a tutorial for intelligence researchers on how to spot and rebut fallacies that distort public perceptions and to which they themselves sometimes fall prey. Although intelligence experts sometimes rail against those public misperceptions and try to set the record straight, we have mostly retreated into our labs and professional societies and journals, as if from a crazy alternate universe that only distracts us from real science. Although understandable, that retreat constitutes collective capitulation. But what is the alternative for a band of dedicated empiricists who have no time or taste for scrapping with non-scientific critics? Do what they do best—analyze.

The misperceptions about intelligence are sustained, often actively so, with empirical falsehoods and logical fallacies. The former can be rebutted by facts, but fallacies cannot. The latter persuade by making the true seem false and the false seem true, which protects fallacious conclusions from empirical refutation. So, at the same time that fallacies pump confusion and hostility into the public sphere, they destroy the scientist’s chief means of self-defense. They do extraordinary mischief. Both their strength and their weakness are the same, however: they operate by stealth. They can be refuted by exposing their illogic, but only by doing so. My aim in identifying the major anti-intelligence testing fallacies and detailing how they work was to help other scholars reduce their pernicious influence.

Q: You said that you started writing about health and the evolution of intelligence in the last few years. How did that come about?

A: I certainly never expected to be working on either issue. Evolution seemed distant from my concerns, and I had never considered the possibility that intelligence might be relevant to physical illness. Illness is a biological problem that your doctor takes care of, right? How wrong I was. And talk about not seeing what is right before your eyes! That changed instantly when I saw a news article about health literacy in large clinic populations and how low literacy contributes to poor adherence to treatment, which is an enormous problem in medicine. It converged with key insights I had gleaned while editing the special issue of *Intelligence.* In researching my own piece (1997d), I had discovered that adult functional literacy tests are probably mostly tests of *g* (for native speakers). Such tests simulate tasks in everyday life such as reading maps and menus, filling out job applications, and calculating change, and all branches of literacy research have separately concluded that it is a general learning and reasoning ability. Bob Gordon's article (1997a) had argued that everyday life can be viewed as a test and it jolted me into thinking more about the psychometric properties of daily tasks. David Lubinski and Lloyd Humphreys' article (1997)also brought home Bob's point when they noted that a set of garden variety tests of everyday knowledge (sports, history, politics, health, etc.), when long and varied enough, also yield a *g* factor. So could the flow of daily life, which was Bob's point. Might the same be true of health self-care, in particular?

Intelligence researchers think a lot about what intelligence is and what it predicts. Psychometricians think a lot about how to measure it. But few people other than Jensen had written much (that I had seen) about the nature of the *tasks* that call it forth. That is, what are the properties that make some items and tests better measures of *g*, technically, more "*g* loaded." What is it out there in the world that makes what's in the head matter? My "why *g* matters" piece (1997d) was built around his answer, task complexity, as had some of my earlier work on *g* and employment. Jensen's conceptualizing *tasks* as having different degrees of *g* loadedness, which parallels the notion that *people* have different levels of *g*, has had a profound influence on my thinking (1998a). Job analysis provided me a way of thinking about the variety and structure of tasks in daily activity, be it on the job or off. My paper joined the two to explain *why* intelligence matters in jobs and daily self-maintenance, as gauged by functional literacy. Task complexity might thus provide a tool for ferreting out where else in daily life higher levels of *g* might, or might not, provide individuals with an advantage. Different spheres of life could be conceptualized as subtests, some with more *g* loaded demands than others (2003b). The human ecology could be visualized as a topography of *g* loadings, with steep gradients in some areas and flat ones in others.

Chronic illness and accidental injury seemed promising areas to explore. There is a huge accident literature, going back many decades. It had dismissed the notion of "accident proneness" because it had not found any personal traits, including intelligence, that predicted accidents in industrial settings, transportation, or elsewhere. However, it made very clear that accident prevention is a quintessentially cognitive process. Why? Because it requires detecting hazards in order to keep events and systems from veering out of control (driving a car, running machines, etc.). Hazards are ubiquitous. The theoretical problem was therefore not to explain why accidents happen, but how they are prevented. What struck me most was, first, life requires negotiating myriad hazards, and second, most are so small individually that we are easily seduced into ignoring them (such as not always wearing safety goggles or seatbelts). We are then lulled into complacency when repeated lapses do not harm us.

But here's the important psychometric point. Each hazard we encounter (ice on the road, etc.) is like an item on an IQ test, only much less *g* loaded. Each requires cognitive processing: Do we recognize the situation as potentially dangerous? If so, do we accurately assess its seriousness? If so, do we take appropriate action to avoid harm? If harm is already being sustained, do we act to limit the extent of damage, and so on? Higher *g* would likely give people an advantage in all these respects, even if only tiny in any one instance. But as psychometricians know from the Spearman-Brown Prophecy Formula for test reliability, that edge need only be non-zero and consistent. With enough items, their common variance (the same edge in all) will add up, and the large chance factors affecting success on each one will cancel each other out. The result is a strong test of *g*, if *g* is the most consistent influence. So what does this have to do with accidents? It means that, if *g* matters, you should see its influence more strongly when you aggregate more and more "items": more exposures per person (cumulative measures comparable to the GPA or overall job performance) or more people per hazard (rates of accidental death in different populations). I did the latter in my article (2004 a) for David Lubinski's special section in the *Journal of Personality and Social Psychology* on the 100^{th} anniversary of Spearman's seminal article on *g*.

Preventing and managing chronic diseases can be viewed in the same way. I had already begun to look at them as life-long jobs or careers and to analyze them as such using the tools and insights of job analysis. Such analyses make it clear that managing a chronic illness like diabetes is a much more complex job for patients than health care providers realize. Worse yet, it is a job they do not expect, do not want, and are not prepared for. They do not get much instruction or any days off. I have been pursuing the applied value of reconceptualizing chronic illnesses in this way, for example, with diabetes educators and policy makers (2005d, 2006a, b, c, d, 2007b), but I am also interested in its implications for understanding health inequalities. Health scientists have long rejected intelligence as relevant to health, though until recently there was not much direct evidence one way or the other. That has changed, largely because of Ian Deary (e.g., Deary et al., 2004), whose research team has shown that higher childhood IQ lowers the relative risk in adulthood of various unhealthy behaviors, chronic illnesses, and premature death.

Health scientists have long drawn on sociology's standard explanation for all inequality: social class differences in health stem from external circumstances and not from anything about the individuals themselves. They therefore tend to explain away the correlations between IQ and health by treating IQ as mostly a stand-in for education or other indicators of social class. At the same time, however, health scientists have been mightily puzzled why the SES-health gradient is so "remarkably" general and linear, regardless of time, place, type of health system, and virtually all diseases, leading them to speculate about a yet-unidentified "fundamental cause." Moreover, the gradient only steepens when health care is free to all. My focus has therefore been, as before, on the mechanisms by which higher *g* might promote better health performance: in this case, more health knowledge, healthier behavior, and more effective prevention and self-management of chronic illness. A lifetime of acts in health self-care (or lack of it) might cumulate into a moderately *g*-loaded test, hence providing a causal explanation for Ian's correlations between IQ and health. If true, rates for specific types of morbidity and mortality should differ according to a group's average IQ. When IQ scores are not available, rates should differ most when using the best surrogates for *g*, in the order literacy, education, job status, then income. This is the pattern I found in the literature for health and health behavior.

Ian Deary and I wrote an article for *Current Directions in Psychological Science* (Gottfredson & Deary, 2004) outlining the early findings relating IQ to mortality (2004) and both of us contributed to David Lubinski's special section on Spearman's *g*. For my special-section article, I used the *g*-predicted patterns for accidental death and other health outcomes to support my hypothesis that average social class differences in *g* might be the mysterious "fundamental cause" of social class differences in health (2004a). This work led to invitations, one by a black-led community group, to discuss the implications of black-white IQ differences in efforts to reduce racial differences in morbidity and mortality.

Q: Were you working on the evolution of intelligence during this time? Was it related to the health research in some way?

Yes, I was, and one emerged from the other. While working on my paper for David's special section, I had been invited to contribute a chapter (2007c) to a book that challenged the dominant view of intelligence in evolutionary psychology, which holds that there is no such thing as a domain-general intelligence. Rather, the consensus in that field claims that the human mind is like a Swiss Army knife: it has myriad heuristics evolved to solve very specific evolutionary problems, such as cheater detection. I suspect that evolutionary psychology was hostile to notions of a general intelligence partly because it might have seemed the kiss of death for an already socially touchy enterprise—looking into the evolved origins of human behavior. But dissatisfaction with this "massively modular view" has been growing. It clearly ignores the evidence for *g,* so I agreed to write a piece about the generality of *g*. Whatever caused intelligence to evolve, it clearly is general now.

I became uneasy, however, because the paper I planned to write would not really confront the modular explanation for how cognitive abilities had evolved. In fact, how *could* a highly general ability have possibly evolved? Ian Deary's studies showing a link between childhood IQ and adult illness and death could not provide an answer, because the chronic diseases that kill most of us today tend to kill us in our post-reproductive years. Many prior hypotheses seemed unpromising because they pointed to selection pressures that are not unique to humans, such as group hunting and tool use. The cold-climate hypothesis could not explain the remarkable increase in human brain size (and presumably intelligence) before humans dispersed out of Africa. And the emerging "social intelligence" explanations did not comport with evidence about the phenomenon supposedly being explained, because *g* predicts performance best in purely instrumental tasks, not socioemotional ones. So, what in the human ecology could have advantaged brighter individuals such that they left behind more genetic descendants than their less bright peers? I did not rule out sexual selection (mating success), but focused on natural selection (survival until reproduction).

I spent a year or two reading and thinking about human evolution, including studies of living hunter-gatherer societies, to which Rosalind Arden, who is a graduate student of Robert Plomin, had introduced me. I already knew that accidents are the major cause of death in the USA during the reproductive years, but now discovered the same is true around the world, including in surviving hunter-gatherer groups. The basic causes are the same everywhere: fires, falls, drowning, injuries from animal attacks (e.g., dog bites, goring by cattle), poisoning, work-related injuries, and such. Virtually all are technology-related in the sense that the hazards to which people succumb are by-products of human innovation, whether it be tools or domesticated animals. Such hazards are thus evolutionarily novel. Human innovation has proceeded to the point where virtually our entire physical ecology is evolutionarily novel. Now, there is obviously a large chance element in individual accidents, but all evolution needed was that brighter members of a group have a non-zero survival advantage over less bright members in order for the Spearman-Brown Prophecy Formula to work its magic over thousands of generations.

The obvious problem with my explanation, however, was that there was no sophisticated technology when humans evolved their big brains. Man had hardly any tools at all in the Pleistocene, except for fire and stones reshaped for scraping, cutting, and the like. I was close to abandoning my innovation-driven hazards hypothesis when I read the 1983 *Promethean Fire: Reflections on the Origins of the Mind* by C.J. Lumsden and E.O. Wilson. It suggested that I had not been thinking elementally enough. Innovation and its hazards do not begin with technology, but with the "mind's eye." This refers to man's ability to lift his eyes beyond his immediate, concrete reality to imagine the possible and unseen. To *imagine*, which is the essence of both foresight and innovation. Innovation need only divert attention from the concrete here-and-now, which is where accidents are waiting to happen, to drive selection. As humankind pumped yet more novel hazards into its environment, it could have sped the evolution of its own intelligence. Prevention requires prediction, which in turn requires understanding cause-and-effect relations and probabilistic thinking. We do this all the time when driving. Should I stop talking on my cell phone while driving, or slow down in this rain, or keep a watch on the erratic driver in the next lane? Analogs among the pre-settlement Ache included stepping on snakes while hunting monkeys in the treetops, getting lost overnight in the forest without a fire-brand, and being hit by a tree that someone else was cutting down.

Rosalind Arden, evolutionary psychologist Geoffrey Miller, and I will soon be trying to test my accident and innovation explanation in conjunction with Geoffrey's sexual selection hypothesis, which holds that intelligence serves as an observable signal to potential mates of overall genetic fitness (which is similar to Deary's hypothesis that *g* reflects "system integrity").

Q: You said that math was your favorite subject in school. Your research hasn't been mathematical as such, but has it been shaped by your interest in math?

A: Yes, it has. Mathematics and statistics are valuable research tools, so this interest has allowed me to learn the necessary statistical methods as needs arise and better appreciate what they can and cannot do for us. The three most pernicious words in non-experimental research are "statistically controlled for," as in "we controlled for social class background in order to assess the impact of IQ on educational attainment." Statistical inference and modeling are tricky business because they rest on the correctness of our initial causal assumptions, which are often unspoken and mistaken. One reason I like exploring different disciplines is that you see how different their tacit assumptions are, which thereby exposes some of your own as well. That is why I like to find big anomalies in the literature. They are clues; they are signals that we are probably making a bad assumption somewhere about something very basic. The hardest thing to see is what you take for granted.

I rely on statistics primarily as a tool for describing and organizing evidence, partly because I am a naturalist and not an experimentalist. Memorable intellectual epiphanies in college included learning as a freshman that ecological processes show mathematical regularities and thus can be modeled, and then learning in graduate school (from Bob Gordon) that many individual differences are normally distributed. I like to collect and rearrange means, standard deviations, correlation matrices, rates and ratios, and such to find patterns. Thinking in terms of components of variance is also extremely helpful, because it helps you sort out which aspects of a question people are really working on. For example, I was surprised to learn that most evolutionary psychologists focus on species means rather than within-species variation despite variation being the essential grist for selection. To take another sort of example, some of the anti-testing fallacies rest on confusing within- and between-individual variation. Mathematical thinking is also useful in a deeper, more general sense because it encourages probabilitistic thinking. Bert Green distinguished between algebraic and geometric styles of thinking among the mathematically-minded. If I understand the distinction correctly, I am definitely geometric. I think in terms of spaces and dimensions and like to present ideas graphically. Factor analysis, canonical correlation, and even matrix algebra seem spatial to me. In fact, I would describe myself as more "spatial" than "mathematical."

A: I can see the spatial bent in your earlier references to "gradients" and "topologies" of effects. But did any particular insight—some substantive insight—rest on mathematical-mindedness?

A: Perhaps so, now that you mention it. Some work rested on rethinking effect sizes and learning to "think small." Social scientists generally hope to find big "effect sizes" because their aims are generally to find stronger levers for social change or explain more of the variance in outcomes of interest. But bigger-is-better and small-is-unimportant are bad rules of thumb in non-experimental research because they are often wrongly generalized across different levels of analysis or causal processes. Here is an example from my work on the evolution of intelligence that illustrates how bigger-is-better can throw researchers off-track, even in a field where the phenomenon of interest—human evolution—requires that within-generation effect sizes be minuscule.

The substantive insight, which might seem paradoxical, is that low-probability threats to personal well-being can more profoundly affect survival than high-probability threats. Researchers and policy makers tend to be most interested in socioeconomic measures of personal well-being, such as educational, occupational, and income level, performance in school and on the job, law-abidingness, health, and longevity. These outcomes tend to co-occur. IQ predicts them all to some degree. It predicts some well and some poorly, but usually better than any other single predictor. In everyday life higher levels of *g* thus operate like a life-long tail wind for higher-*g* individuals. Greater proficiency at learning and reasoning makes everything relatively easier because virtually everything in life requires at least a bit of learning and reasoning. From an epidemiological perspective, *g* appears to sit at the core of individual differences in personal comfort, social status, and physical health, so the challenge for policy makers who seek to change those differences is to identify the biggest levers for increasing, decreasing, or otherwise changing *g*'s effects within a population. That is the whole point of my own work on chronic diseases as complex jobs.

Evolution, however, cares nothing about your personal well-being, prestige, or any other functional advantage that higher *g* might afford you. Evolution counts only the number of surviving children, grandchildren, and other genetic descendents you contribute to future generations. Your personal comfort, prestige, and well-being will influence evolution, but only to the extent that they enhance your relative reproductive success, or "fitness." Here comes the mistaken generalization: Because *g* correlates most strongly with these aspects of well-being, many of us had assumed that they or their Pleistocene analogs are the most plausible mechanisms by which *g* influenced reproductive fitness.

Reading the detailed portraits of the Ache hunter-gatherers in Paraguay forced me to consider the opposite possibility. Namely, activities that obviously enhance survival (e.g., food acquisition) may actually be among the *least* likely to have selected for higher intelligence during evolution. Why? Precisely because they are obvious to the individuals at risk. Humans lived in small bands who survived only by protecting the unit from obvious threats to its existence: predation, starvation, the elements, and raids by other bands.

Bands survived by pooling resources, such as the big-packet proceeds that adult men could occasionally obtain. Among the Ache, for instance, good hunters got no more meat than any other member of the band. All Ache adults got equal shares, which they disbursed to their children. Individual talent was turned toward helping the band fight off the Four Horsemen of the Apocalypse. So, as a rule, individual members did not starve; whole groups did. Groups concentrated on the most obvious threats to collective survival, so it may have been the least conspicuous threats to life and limb that disproportionately killed lower-*g* individuals, specifically, the myriad hazards of daily life that still cause so much "accidental" death during the teen and middle years.

The conceptual mistake in this case was to ignore how small groups of mutually-dependent individuals mobilize talent and buffer the less competent, much as families still do today. I described a similar conceptual mistake earlier relating to my work on the emergence of the occupational status hierarchy: by failing to consider employer behavior, sociologists had wrongly concluded that educational level necessarily predicts how well one *performs* a job better than does IQ because it predicts the status of the job one *enters* better than does IQ

The small-is-inconsequential rule of thumb is most apt to mislead when small effects have an opportunity to cumulate. I have already described how seemingly minuscule advantages of higher IQ in daily life could have ratcheted up human intelligence over many thousands of years and also how, in today's world, many small acts of slightly better health self-care can cumulate over a lifetime to explain why brighter individuals tend to be healthier and live longer. I suspect there may be other big questions in the social sciences that also require understanding really small effects: the Spearman-Brown Prophecy Formula writ large across human life.

Q: I was surprised to learn that you don't have any graduate students.

I have never been part of the graduate faculty at UD, and my teaching has always been in the undergraduate program. I would enjoy working with graduate students, but I am happy teaching undergraduates in the University Honors Program. The Honors Program draws excellent students from all majors, and I can teach anything I like to small classes, which usually includes "Intelligence in Everyday Life." That doesn't make it easy, but it makes it fun. I am very lucky.

Q: Do you get hate mail?

No, quite the opposite. I get appreciative emails out of the blue from all sorts of people. I got one just last month from a professor emeritus asking if he could help with my work in some way. The stream of notes and emails is small but regular. It is from such communications that I learned long ago that there is a vast body of suppressed opinion among thoughtful citizens. One reason that I wrote so much about the sociopolitical dilemmas in employment testing, especially those involving race and *g*, was that personnel selection practitioners in non-academic settings cannot speak openly about them. They are being asked to square the circle, just as teachers are. I was elected fellow of SIOP in 1994 for that work, ironically the same body of scholarship that was Exhibit A against me at UD. Practitioners' needs and gratitude have always reinforced my resolve to stand up against intimidation.

My dean once suggested that I liked all the controversy, implying that I had invited it upon myself. Actually, dealing with controversy is a huge waste of time and energy, a tax for doing unpopular work. The dean was implicitly absolving himself of responsibility for harming me, but I think he reflected a common assumption that individuals who put up with controversy must enjoy it (otherwise they would flee from it). There are such people, but this introvert is not one of them! Nor is Art Jensen, Robert Gordon, Frank Schmidt, or any of the other sometimes "controversial" scholars I have mentioned. Some observers say they are brave. I would not dispute that, but that misses their essence, as I see it. Controversy is simply not part of their calculations in doing science. They neither seek it nor avoid it. They march to their own drummers, and that drummer is empirical truth, wherever it leads. They are detectives on cases, not social workers, or politicians, or academic ambulance chasers. They are interesting, independent and resolute, and they are models of scientific integrity. I have always strived to be like them. I never had much homework in high school, so I would come home and watch old movies on TV. Many were about heroism during WWII, which was still rather recent history. I always asked myself whether I would have done what they did. I hoped I could. It has been like that, watching these scholars over the years, and I hope I have inspired others like they inspired me.

Q: How did you come to write your critique of Robert Sternberg's practical intelligence research? That's a tough article.

A confluence of three things: He provoked me, I needed to stop ignoring his claims, and Nathan Brody had just presented a paper disputing some of Sternberg's other, related claims. Sternberg had asked me to write a chapter on *g* for the book he and Elena Grigorenko were putting together, *The General Factor of Intelligence.* I appreciated the opportunity, and I wrote about the practical, everyday value of *g*. On one page of the manuscript I discussed his notion of practical intelligence, but he objected that my portrayal and criticism of it were mistaken and said to change it. He referred me to his just published book, *Practical Intelligence in Everyday Life,* as a corrective.

I had once tried to read his earlier books on triarchic theory and practical intelligence, but could never get far because they did not seem logical. They made my head swim. Yet, his triarchic theory stood as a challenge to *g* theory, and he is an influential figure. And amazingly energetic in promoting his views. I also objected to the manner in which his articles ridiculed and insulted those with whom he disagreed, rather than really answering them. So I bit the bullet and spent several months reading everything he had written on practical intelligence and trying to sort out his findings and claims. I suggested to Nathan Brody that we propose a special issue of *Intelligence* devoted to Sternberg's triarchic intelligence research. He and I would contribute articles, Sternberg would reply, and we would respond to him. The editor accepted our proposal. Sternberg declined to answer my emails for some months after I sent him my manuscript. I had satisfied myself, however, that his claims lacked credible empirical support and that I had figured out how his arguments nonetheless persuade people otherwise. I finished the critique before my chapter for Sternberg's book went to press, so I added a citation to it there (2003a).

Q. To what do you owe your success? I mean you are a great writer. You also have a knack for seeing in your research things that others do not.

Success for me would be to make important contributions to the substance and practice of science. To the extent I have been successful by this criterion, it has been for much the same reasons that anyone is successful. As I tell my students, higher *g* is a useful tool, but it does nothing by itself. You have to use it. That means going beyond the obvious, not settling for the quick and easy insight, which makes it hard work. In any profession it also means putting in long hours. For mothers especially, it means making difficult, guilt-inducing compromises in how you spend your time. Your interests, curiosity, or ambitions have to be intense enough to override the high costs and only intermittent rewards.

You also mentioned my writing. Art Jensen has asked me a similar question, "How did you become such a good science writer?" He says he wishes he were. He is a model of analytical and expositional clarity, so he clearly meant something else. Mulling over his question later, I think he may have been getting at two attributes that others have said distinguishes my best writing from most other academic writing in science: it is spirited, and it is intellectually engaging for both specialist and non-specialist audiences.

The spiritedness springs from a passion for doing, defending, and promulgating good science. It is an energy that can be disciplined and focused, and which can enthuse others as well. I have always preferred to lead than to follow.

Jan Blits is a terrific writer and I learned a lot from him. The pithy conceptualization and turn of phrase are part of it. I work to distill complex ideas to their essence, and it is very rewarding when I succeed. I also listen to the words, phrasing, and cadence as I write. Like Jan, I want to write well so have worked hard at it. Tight logic and absolute accuracy are my first priorities, of course, with word-crafting always secondary, but all are important aesthetics for me.

In writing about my particular topics, it has been important to ground abstract ideas in concrete realities that readers can see and touch for themselves. I think that is what made my circumscription and compromise theory so compelling to many people, as well as why other pieces persuade readers that intelligence really does matter in everyday life, especially health. They start pouring out examples in return. I try to describe *g* operating in the smallest details of daily life that readers see for themselves, but then show how these simple, observable phenomena connect to truly big questions in social life. The approach is novel for many specialists yet accessible to non-specialists.

In any case, I think my better-than-average scientific writing is one reason that I have been invited to write pieces for general audiences. That, and writing about "sensitive" issues in a rigorous and matter-of-fact way. I can tell you, though, that it is humbling to work with professional writers, such as the editors at the *Wall Street Journal, Scientific American,* and the *Wilson Quarterly.*

Finally, you referred to a knack for seeing things that others do not. What you may be referring to is my discerning larger patterns from existing data, which seem obvious once pointed out. It is terrifically hard work. I try to do what I admonish my students to do: look beyond what you see at first glance.

I have done typical sorts of research, such as conducting and analyzing data from surveys, critiquing past research, and analyzing public policy. But I do not count them as my most creative and satisfying contributions, the ones where I see things that others do not, as you put it. These would include my theory of career aspirations (1981, 1996b, 2002b), the origins of the occupational status hierarchy (1985), the aptitude-demands structure of the division of labor (1986a), the importance of intelligence in everyday life (1997d) and health (2004a), and the evolution of human intelligence (2007c). These works all involved pattern finding, triangulating results, or compiling nomological networks from extant bodies of evidence. You could call the approach interdisciplinary data scavenging to solve a puzzle or generating empirically-grounded theories. It often takes the form of analyzing diverse forms of evidence from different disciplines to create an extended logical argument, or “explanation” of some particular phenomenon: Why do young people’s career aspirations recreate the social inequalities of their elders? Is there a functional basis to the occupational hierarchy? What ecological pressures could have led humans to evolve such high intelligence? And so on.

Each took detective work: finding clues, investigating leads, piecing together evidence, resolving seeming anomalies, checking out hypotheses, evaluating alternative explanations, and, eventually, making a case for the most plausible explanation. That is why I end up writing such long, involved articles, which can drive editors crazy. The process also takes time, from months to years, because I often end up looking into literatures I have never explored before. I begin by pursuing hunches, including where to find additional information, but otherwise the process is quite open-ended. I read and read and read, both to familiarize myself with a wide range of relevant data and hypotheses, but also to troll for anomalies, misconceptions, and tacit assumptions in those bodies of work. It is important to cogitate, to mull over what I am learning relative to my prior expectations and knowledge, to let my mind roam through the information. The process becomes exciting when I start seeing consistencies, making unexpected connections, and viewing old things in new ways, at which point the pursuit becomes more focused, even seeming to take me over. I start writing once I am satisfied I have found the heart of an answer. I use that early writing as an aid and goad to clarifying, amplifying, improving, and distilling my proposed explanation to its essentials. Writing imposes intellectual discipline. As I tell the students in my writing-intensive courses, writing is thinking and thinking is hard work. If your writing isn't clear, your thinking probably wasn't either. I therefore try to scour my manuscripts for any lapse in logic or clarity, because it invariably signals a failure to think something through.

This is the sort of work I find most rewarding, though I am often miserable when in the throes of it. I never set out to do it, but seemingly straightforward writing projects occasionally morph into these extended explanations, or theories, when I think I have missed something crucial or something does not sit properly. You have asked me what makes me who I am as a person and scholar. I see this work as reflecting something about my inner nature, which would have come out in other forms had I not become a scholar: I take pleasure in organizing, tend toward perfectionism, and enjoy the challenge of unraveling knotty and confused problems.

This is me, not how I was socialized or trained. My drive to collect and organize information is surely a genetically-conditioned proclivity. Not only have behavior geneticists yet to document any psychological trait that is *not* substantially heritable, but I recently realized that my brothers and father share the same proclivity despite our not having lived together for much of my brothers' childhoods and despite our father being estranged from us for many years. It was only when we gathered during our father's last days that it hit me: we are all "organizers." Each of us has created the same extended phenotype, in this regard.

My father had cabinets, shelves, and sheds for organizing his tools, books, and mementos from his research endeavors world-wide. Looking around my home, I notice lots of storage containers, shelves, decorative boxes and baskets, spice cabinets, and the like. Same trend in my very crammed office, with filing structures to the ceiling all around. Going through my father's workrooms after his death last fall, it struck me that he had a little kitchen of sorts in each—paper plates, snacks, cocoa, etc—just as I have in my office, so he would not have to interrupt his work. My youngest brother recently created a new job for himself in the R&D drug company where he works: organizing the flow and sharing of information within his division (details of production processes, quality control, etc.). Echoes of my birth and death surveillance system in Malaysia? Our mentally handicapped brother is much the same—likes to collect and organize: books, tapes, DVDs, family photos, tools, and such. We all like precision.

My service activities at UD fit the same pattern. When our newly-consolidated college voted to establish a faculty governance council in 1997, I led the effort to develop its objectives, structure, and working procedures. I did the same thing when asked to chair a newly-created committee in our department. Earlier, during the funding controversy, I was asked to take charge of the Faculty Senate's Student and Faculty Honors Committee, the vagueness of whose procedures for awarding its big prizes had made it vulnerable to complaints of unfairness. We researched appropriate criteria, tightened procedures, and put everything in writing. When I was brought back by continuing concerns several years later, I led the committee into the digital age, the first Senate committee to take that step. Over a period of years, we started taking authenticable nominations online, set up a database for reviewing them, created a public website to assure public transparency, and compiled an electronic registry of committee calendars, policies, and procedures to ensure continuity and consistency from one year to the next despite ever-changing membership. The Faculty Senate thanked me for this unusual organizational effort in 2005 with a commendation for "extraordinary leadership and service."

Q: In view of the very real and significant pressure you felt, and the serious consequences you suffered, why did you persevere on this topic? Surely there must've been other topics of interest to you without the inevitable and predictable stürm und drang associated with this one.

When Isaac Newton wrote about religious matters he usually wrote in English, but he switched to Latin on some topics (like the peculiar sexual practices among the Babylonians). Perhaps we would be wise to follow his example and use Latin on those topics and results for which the lay public would likely misunderstand or misinterpret.

A: Yes, I frequently hear these two questions: Why do I keep writing about race and intelligence despite knowing the trouble it will cause me? And, if one persists with such topics, might not it be wiser to write about them in code? I will take them in turn.

I am interested in important questions and this is one within my ken. I have already described how I began my career with no particular interest in cognitive abilities.

However, all the big questions I have pursued turn out to implicate intelligence differences at their core: social inequality, fairness in hiring and college admissions, accident prevention, and so on. The wide dispersion in general intelligence within all human populations is a terrifically important biological and sociological fact. The more the evidence drew me to this conclusion, the more intriguing the topic became.

The question, then, is why I pursue my interests despite sometimes strong social censure. People differ greatly in their willingness to risk community disapproval, and I suspect that humans evolved an aversion to it when they lived in small bands struggling to survive. Scholars are supposed to set aside public opinion to pursue the truth, yet we advance chiefly by the good opinions of others in our profession: journal reviewers, editors, awards committees, the administrators and committees that hire and promote us, and such. Especially when junior, most of us sniff for the faintest scents of approval and disapproval, charting our intellectual paths accordingly. Such extreme caution not only neuters the intellect, but is utterly unnecessary, except perhaps if one seeks high income, high office, or popular acclaim.

No one would place me at the cautious end of the continuum, but having traversed the range, I can report that the practical consequences of matter-of-fact, reasoned, non-defensive candor are minor until one broaches the genetics of racial differences in IQ. The storms may be dramatic but are rare. And even there, the risks are far less than often supposed, and even at their worst can be weathered. If you build a compelling case, you are more likely to be ignored than attacked by critics. Such was the response when Rushton and Jensen published their argument, and I my supporting commentary, in *Psychology, Public Policy, and Law* (2005e) that the American black-white IQ gap is more likely 50-80% genetic than 0% genetic. There will be other tempests, I am sure, but more facts can be spoken openly and safely today than before publication of *The Bell Curve.* So it was after the storm over Jensen's 1969 famous *Harvard Educational Review* article, and so it will be in the future. The ideas for which I was attacked at UD in 1989 are often taken as commonplace knowledge today.

I have pondered, however, what sorts of personalities will risk community scorn rather than change course. Maybe that is what you are asking me. Among those I know personally, one commonality is that they pay little heed to evaluations, positive or negative, by persons whose scientific judgment they do not trust.

Maybe that is what is meant when people say they have "thick skins." I myself do not trust the opinions of individuals who agree with me too readily, and I warn my students that doing so will only hurt their grades. I am not speaking here of mere obliviousness to public opinion, but a strong commitment to some higher principle that outweighs any scorn or inconvenience its pursuit may entail. Tenured academics should be willing to risk something in return for society giving them the remarkable opportunity to investigate questions, theoretical or practical, of their own choosing.

The second part of your question concerned the wisdom of straight talk in all matters scientific. It is a valid question. Some social scientists have written that certain scientific conclusions about intelligence are too "dangerous" to report freely or that society is better served by well-meaning lies. But are they correct? None has provided any reasoned argument or evidence, just allusions to evil and catastrophe, to justify this stance. Might they have it backwards? To my eye, efforts to hide or shade the truth about human variation are doing grievous harm to the body politic (e.g., 2005b, 2007a). Keep in mind that false belief in infinite human malleability led to some of the worst horrors of the Twentieth Century.

I also think it is patronizing and usually self-serving when elites contend that the American public cannot be trusted with certain facts. The onus, in my view, is on those who would withhold information that is so relevant to public life. We should not be surprised that the public often misunderstands the results on intelligence, because it has been systematically miseducated by media accounts, many college textbooks, and other ostensible sources of enlightenment. We should worry about the destruction that disinformation wreaks, as when critics suggest that giving credence to black-white IQ differences would mean believing that "blacks are inferior." My experience is that people get more serious-minded about IQ differences when they start to appreciate the greater practical difficulties and health risks faced by individuals of below-average intelligence, regardless of race.

But for the sake of argument, let's say that I agree not to broach sensitive topics, such as the racial gap in average measured intelligence. What would that actually entail? I could refuse to analyze, write about, or speak about it. But if I then limit myself to the genetics or practical value of higher intelligence among whites, I risk being seen as insinuating racial differences. How do I rebuff that insinuation? My only convincing rebuttal would be to explicitly assert that racial differences do *not* exist, do *not* really matter, or do *not* resist easy manipulation, which, if I know the literature, is tantamount to lying. I could waffle or mislead, but that seems mendacious as well.

Suppose that I teach a sociology of education class, which I did for many years, or educational psychology. Inequality is a core topic in both: Why do some children do better in school than others? And, why are there systematic race and class differences in academic performance and years of schooling? IQ is hands-down the best single predictor of individual differences in both, though hardly the whole explanation. If I mention this fact, my students will ask whether IQ tests are culturally biased and can really measure intelligence. How do I respond? If I review the evidence that IQ tests are highly reliable, validly capture real differences in proficiency at learning and reasoning, and are not culturally biased against American blacks or other native speakers, they will then ask me whether this means that there are race and class differences in intelligence. What do I say now? Do I say "no," refuse to answer, change the subject? OK, imagine I avoided getting into this situation by never having brought up intelligence in the first place. My students will still want to know what creates educational inequality. I can offer up the plethora of more politically correct hypotheses (test bias, teacher prejudice, poverty, fear of acting white, etc.), but this will surely mislead them because many are implausible, some already disproved, and others of minor consequence relative to the impact of intelligence differences.

As you can see, it is not easy as a practical matter to determine at what point in the chain of evidence to start withholding established facts and more plausible hypotheses. Note also that such self-suppression pushes its practitioners inexorably toward arguing against the importance of individual differences themselves, and also acquiescing to the notion that we are all just hapless, passive products of circumstance. My whole career might be summed up as opposing that false and paralyzing belief. No single thing we do may make much difference, but it all adds up, especially when everybody contributes something.

Q: Do your daughters want to be scientists and professors too?

Heavens no. Nina and Lisa are arts and humanities types, talented writers and good at languages. They are magical dealing with children. That's where their careers lie—in teaching children. They also decided early on that they did not want the kind of job I have. I tried to interest them in all the things that interested me as a child, but they would have no truck with bugs and such. Flowers yes, and we love to garden together. They have quite an aesthetic touch. They adored having cats, but as surrogate babies.

They were always disappointed, however, that they were never in one of Tom Bouchard's twin studies. I explained that MZTs (monozygotic twins reared together) are plentiful and he wanted those rare identicals who had been reared apart. People often remark on the coincidence that Bob and I study twins and also have them. Bob would sometimes joke in response that we actually had triplets, but gave one away to study environmental effects. I did that once, but the fellow took me seriously and looked horrified.

Q: Do you have any advice for young women going into science?

A: Yes, and it dawned on me only recently. I see women getting more caught up in committee work and other service activities than do men. The women also tend to be more conscientious about it. In my setting, I observe some men but no women refusing to carry out the assignments they have accepted, and I see relatively more women among the stellar performers. Nonperformance seems to go unpunished, but conscientious performance draws yet more requests to serve. I therefore suspect that women tend to accumulate more service time, much of it untallied. I know that they often have a harder time saying no to requests or to doing just the minimum. My close male colleagues simply cannot fathom such gratuitous helping behavior and think it foolish; close female colleagues cannot understand the males' dismissive stance toward community obligations.

I am referring here to a fairly subtle but powerful sex difference that seems rooted in known differences in temperament, interests, and priorities. Evolutionarily, women have been the glue of social groups. They tend to be more concerned with ministering to families and communities, take more pleasure from such activity, are more moved by the gratitude it generates, and suffer more anxiety and guilt when they skimp on it. I have long recognized these feelings in myself and know they generalize to work settings, but I only recently realized that I experience them as physiological reinforcement. No wonder men tend to behave differently when faced with the same choices. So my advice to women is, "Restrain your natural impulse to indiscriminately serve and help. Choose wisely, because time is your most precious resource."

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