

Autobiography

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In the first marking period in the sixth grade, my grade in arithmetic was NP (not passing). In high school, I gratefully rejected the opportunity to take mathematics beyond the two years required. In college, I survived freshman algebra only because of a saintly professor who gave help sessions, often for me alone, each evening at the end of an ordinary working day. How did it happen, with a background like that, that I now find myself a mathematically-oriented measurement specialist speaking and writing on topics such as latent trait theory?

In fact, how did it happen that I've become identified with the leftof-the-hyphen term in what is regrettably often written as "IndustrialOrganizational psychology"? In graduate school, the topics such as counseling, grievance procedures (the topic of my M.S. thesis), work motivation, and employee attitudes interested me greatly. My dissertation was the first study in what later came to be called "span of control" research. With graduate student interests emphasizing what later became organizational psychology, how did I come to be almost the prototype of the traditional, unimaginative personnel selection type?

Four trends in things that have happened to me since receiving the Ph.D. from Purdue in 1952 (under C. H. Lawshe) may account for the directions I have taken. However, since adult behavior is often influenced by early experiences, some of my early background may be relevant.

Some Personal Background

My father was a salesman. Throughout his adult life, his income depended on his own initiative and skill in meeting people and convincing them of the soundness of his products. My mother was principally a housewife. During the depression years, she found it necessary to take on paid employment, and she saw no reason why employment should be disagreeable just because it was necessary. So she looked for jobs where she would learn to do things she wanted to know how to do anyway. At one time, therefore, she became a candymaker, beginning as a chocolate dipper and eventually taking over management of the kitchen for a chain of eight stores. Another time she took a bakery job where she learned the art of cake decorating; her mentor was another victim of the depression who had majored in fine art in college and used frosting instead of oils on her palette. With the outbreak of World War II, these pursuits seemed frivolous; she then took on a job in a war plant to learn how to operate a wide variety of office duplicating machinery.

As an only child, I had only the experience of these two adults to copy. I started earlier than they would have wished, I suppose; I got my first job paying a commission just before my eighth birthday. In the spring of 1932, a man gave me six copies of a current magazine to sell with the promise that I could keep a penny from each nickel sale. The sudden wealth from those sales led to a route of regular customers which, by the end of the summer, had expanded into selling six magazines, under two publishers and marketing agencies, with a total income of nearly \$1.25 a week. I have not been seriously unemployed since.

In high school, I was active with the school newspaper, band, dramatics, and other extracurricular activities; there was no time to be more than a casual student -- until my senior year. In that year I studied chemistry seriously under an outstanding teacher, one who used the personalized system of instruction before Skinner had outlined its principles. I spent mornings, lunch hours, and large parts of afternoons (after delivering papers) in the chemistry lab doing experiments for the sheer fun of it. I went to college in the fall of 1942 to major in pharmaceutical chemistry, intending to continue for graduate work to prepare for a research career.

Six weeks into the second semester, the call to active duty in the Army came. My military career was a particularly uneventful one. After three basic trainings and college work in two unrelated training programs, I was sent to Italy as a mule skinner, reclassified a clerk-typist, and spent the rest of my post-war military career learning to type discharge papers for soldiers staying in Italy as civilians. At one point I augmented the regular salary by working as an officers' orderly.

One of the college programs was a premedical course at the University of Iowa; it included two terms of introductory psychology. Military supervision was lax, and the course met early in the morning. My attendance record, interest in the course, and examination average all fell somewhere near the bottom of the distributions. That was satisfactory; I needed to do well enough in chemistry to stay in the preferred program, but badly enough in psychology to avoid assignment to a medical school. I tried psychology again in Italy when the course was offered by the Armed Forces Institute, thinking it might be more palatable at a later hour. It wasn't. I dropped out.

My favorite military assignment came at the end of the war in the separations center for the Mediterranean Theater of Operations. It was a pleasant sort of personnel work, and I enjoyed it immensely. Since it was a small operation, there was time to get to know the people we prepared discharge papers for. It was from one of them that I first heard of industrial psychology, his intended career. As he described it, it sounded moderately interesting.

After my own discharge, I took a factory job as a materials expeditor. I also wanted to test myself to see if I could be disciplined enough to go back to school successfully. I could, I reasoned, if I could stick it out through a course in psychology. The available night school course was one in applied rather than general psychology (there was never a mention of axons or dendrites), and it was relevant in a fascinating way to the work I was doing on my job.

I returned to Iowa in the fall of 1946, with two years of college credit, I was beginning to vacillate between my earlier plan for a research career in chemistry and a career change to personnel research. There were practical matters to consider, I already had a couple of years of chemistry, but I would need to start from almost the beginning in psychology. However, when I walked into the chemistry lecture hall and saw all of the things that had been added to the periodic chart while I had been gone, it seemed that I would be starting from scratch in either case. I promptly changed to a major in psychology. So much for careful career planning.

Three things in all of this seem relevant even yet. First is a kind of iconoclastic self reliance. I never had an allowance nor respected those who did; whatever I wanted, I got the money to pay for it by working a little longer at whatever jobs were available to me. As a child, I saw no reason to assume that it should be otherwise, nor do I now. Second, although I am in many respects a philosophical holist, I have been fascinated since high school chemistry in atomistic, analytical processes. Early interest in Mendeleev's periodic chart of the elements led easily to later interest in Guilford's theory of the structure of intellect. Third, I developed the habit a long time ago of either exploiting opportunities that came my way or riding out the unexploitable without much protest. Once in high school, caught in the middle between the competing demands of two teachers, I simply chose my own path and awaited the consequences without a great deal of interest in them. I've never been known for single-minded devotion to a long-range plan.

Employment History

1952, which was when I got my Ph.D., was a recession year. Only three jobs in industrial psychology came to my attention at all. The first two of these were non-academic positions, and I had already decided that I wanted the variety and independence of an academic life. The first position that came was a one-year, soft-money position; with a wife and two children, I wanted something more permanent. The second was in one of two research groups in a major corporation; the environment did not appear to be a particularly peaceful one. Finally, in mid-summer, a young man who had been teaching courses in applied psychology at Bowling Green State University quit his job to open up a used car agency and, coincidentally, to open an academic position for me. His resignation interfered with the summer activities of the department chairman, so a blitzkrieg recruiting campaign was begun. As a part of that campaign, he and the college dean declared their intention to develop at Bowling Green the same level of prominence in industrial psychology at the M.A. level that Purdue enjoyed at the Ph.D. level. This seemed feasible. I came. Perhaps it is an indication that childhood signs of initiative had all petered out that I have stayed now into my 27th year. There were certainly times in the early years when I wanted to leave, and there were other times when opportunities for change were available -- but the two never seemed to occur at the same time.

The teaching load at Bowling Green in 1952 was 15 hours. In the first semester, I was assigned a course in occupational information, one in general applied psychology, and three sections of introductory psychology. In the second semester, I taught a course in personnel testing and four sections of introductory psychology. The course in personnel testing was offered to graduate students in psychology; it was also a required course for undergraduate students in business. A variety of undergraduate students in other majors also enrolled, for reasons never fully clear to me.

The personnel testing course was something of a disaster. I used both the Thorndike (1949) and the Lawshe (1948) textbooks. The Thorndike book was much too complex for most of the undergraduate students, but the Lawshe book lacked some of the advanced material that the graduate students needed. I fell to writing out "translations" of certain of the Thorndike chapters, trying to infuse them with Lawshe's practicality and easy readability. Such writing was time-

consuming, but, since I could use preparations from the first semester for my other four classes, I managed to do a fair amount of it.

There was no time in that first year to think very much about developing a graduate curriculum in industrial psychology. But, in 1953, I collared virtually everyone I could at the APA meetings to glean their ideas about an "ideal" M.A. program. From these conversations, I outlined a one-year program that seemed to integrate the best of what I had heard. Rather proudly, I showed it to the department chairman, who seemed singularly unimpressed. He sent me to the college dean, who seemed embarrassed by it. He in turn passed the buck to the Dean of Faculties, who seemed annoyed by it, especially when I told him that it was my attempt to meet the commitment I had made in coming to Bowling Green. The gist of our further conversation was that neither he nor the President knew of such a commitment, nor would they have approved of it; the agenda for the University was to pare course offerings, not add them! After some discussion, he said there might be some chance to get the new curriculum approved if I could convince others in the psychology department to eliminate more courses than my proposal would add.

This turned into a remarkable opportunity. A committee chaired by Dael Wolfle (1952) had prepared a little book on undergraduate instruction in psychology in which four different specific curricula were described. The seven of us who made up the psychology department immersed ourselves in the book and in curriculum reform. We designed an integrated undergraduate and graduate program and we were enthusiastic about it; it permitted me to offer a new seminar on current research in industrial psychology and courses in job analysis, training and supervisory development, and motivation and morale. The testing course remained as it was.

The new curriculum reform included a series of four undergraduate laboratory courses; I was assigned one of these, a course in "human motivation." The only motivation textbook then available was the one by P. T. Young (1936). It was not new, it had a heavy stress on basic sensory processes and on studies with animals, and it did not fit the curricular objectives.

Therefore, in the summer of 1954, while on a Committee for Economic Development "internship" at Motorola, I put together a mimeographed "textbook" to use for that course in the fall. Fortunately, the book was never published. It was as deficient in scholarship as one would expect of a book thrown together in such short time. My knowledge of motivation theory was limited; so were opportunities for library research during much of the time I was writing. The book borrowed heavily, therefore, from secondary and even anecdotal sources. Despite the book, the course provided impetus for me to think about motivation and to read avidly the books published in the mid 1950's. I continued to teach it, although with different text material, each year until I left on leave of absence in 1963.

The undergraduate motivation course greatly influenced the content of the graduate course in motivation and morale at work, adding theoretical issues that otherwise might have been ignored. Studies of achievement motivation especially interested me; my first serious attempt to do research in this field used TAT-type pictures designed to elicit themes related not only to achievement motivation, but to the various levels in the Maslow hierarchy. A faculty research grant enabled me to have questionnaire material printed beautifully, to hire interviewers and train

them in three different cities, and to collect a substantial amount of data. The results of the study were never published for a good and sufficient reason: we were never able to find a way to score the responses reliably enough to do anything else with them. This early experience was a strong reminder that good research does indeed require competent measurement.

In 1963-64, I was a visiting associate professor in the psychology department at Berkeley, where, among other assignments, I offered a graduate seminar in work motivation. It was there also that I became interested in the measurement of the meaning of work. By this time, these topics clearly represented my major professional and scientific interests; eventually, I developed a proposal for research on development of work motivation among recent college graduates in relation to the anticipated and experienced meaning their work had for them. The proposal was one of the winners of the James McKeen Cattell Award, given annually by Division 14 of the APA, and the research was later funded by the Department of Labor. Unfortunately, when I became a department chairman in 1966 I had to leave most of the research responsibility in the very capable hands of my graduate assistant, Frank J. Landy. His research activity moved on steadily from that point, but mine atrophied during the five years of administrative work. My research on the development of work motivation has never been picked up again, although I always have the hope that some day I will get back to it. I still think of the problem of work motivation as the most interesting and socially important of the entire field, although I am no longer up-to-date in its study.

Each year until my chairmanship, I continued to teach the course in personnel testing, and through it I became interested in factor analysis. It was either in my second or third year at Bowling Green that I was given a committee responsibility of bringing in a guest lecturer. I invited Raymond B. Cattell to be that lecturer; he came and we developed a lasting friendship. Bowling Green did not have the computational facilities for the factor analysis of large matrices, but Cattell made his resources available to Marshall Brenner, one of my M.A. students. When Guilford (1956) first published his theory of the structure of intellect, it found, in me at least, a very receptive audience. I wrote some material on factor analysis for the personnel testing course.

The middle 1950's were years when publishers were seeking manuscripts. No one was interested in my manuscript for the motivation book, but several seemed interested in the materials I had been writing for the testing course. I offered a few of my "translations" to the McGraw-Hill representative, who sent them to Ed Ghiselli, who, characteristically, wrote very positive and pleasant comments on what I had written. The result was a contract to which I could devote very little time. Although our teaching load eventually dropped from 15 to 12 hours, the responsibility for all industrial courses, for the laboratory course in motivation, and for the development of additional courses in the educational counseling program eventually resulted in my teaching nine quite different courses every calendar year. In the spring and fall I taught a section of the introductory psychology course. I taught three other courses in the fall, three others in the spring, and two more in the summer session.

Keeping preparations current in all of these courses left little time for either research or writing. The only kind of writing I did during that period consisted largely of articles written to get things off of my chest, such as the one on multiple criteria (Guion, 1961). It was not until the

year at Berkeley that I had any real opportunity for the concentrated periods of work necessary to complete the book. Parts of it were ground out periodically, usually during summers, but the time span between the early writing and late writing covered more than six years. At Berkeley, I was able to complete unwritten chapters, revise the others, and bring everything up-to-date; in the spring of 1964, the final manuscript went off to McGraw-Hill (Guion, 1965b).

When I returned to Bowling Green at the end of that year, it was like going to a new job at a different school. There was a new president and a new dean in the College of Liberal Arts. A computer center was established and functioning. The mood of the university was one of growth, and the psychology department was planning its doctoral program to begin the fall of 1965.

The program included a three-term sequence on research methods; I was assigned the term on correlational research methods, with an emphasis on measurement. When one is teaching students from all the various specialty areas of psychology, one cannot restrict the concept of measurement to testing. The course has required me to learn, at least at an elementary level, something about information theory, signal detection theory, latent trait theory, and other measurement approaches not encountered in classical psychometric theory. The course has been one of the major opportunities I have had to develop professionally through teaching.

The Revolution

The changes at Bowling Green during the year of leave were the dramatic culmination of a long series of events. In contrast to the usual stereotype of the 1950's, ours was not a particularly peaceful campus. Two or three years before my arrival, students had rioted in protest against the policies of an earlier president. A new president had come to the university in 1951, and in the spring of 1956, students rioted to protest his policies. It was particularly traumatic for me because I had been supervising a survey of the attitudes and needs of student leaders on campus at the request of the Dean of Students. My student interviewers reported extreme anger and threats of violence by their interviewees; I discounted their reports as due to inexperience. I should have been able to predict the violence that occurred that year, and I was badly shaken. I was not as badly shaken, however, as the administrative officers at whom real acts of violence were directed. State police were called in, martial law was declared, and only the start of the summer vacation eventually quieted the campus.

Four years later another, a milder riot occurred when campus police, probably remembering the earlier one, used what seemed to be unnecessary force in breaking up some rather destructive hijinks on the first warm day of spring. What began as fun was transformed into violence.

Over the years, faculty meetings had degenerated from academic forums to sessions at which the faculty passively received communications from the university president. Before the close of a regularly scheduled general faculty meeting after the riot, just prior to the spring recess, one faculty member asked for the floor to read to the faculty a document prepared by some of the dissident students. After some confusion, he did so and was widely applauded for his courage; the situation after the vacation, although tense, was eased when a committee appointed by the president established formal procedures for listening to student grievances. However, on the very last day of the academic year, the faculty member who spoke out was fired. A group of us

supported him, and the revolt of the faculty began. I served as a kind of a field-grade officer in that revolution. We explained our position individually to different members of the Board of Trustees. We wrote letters to influential people and to newspapers. Above all, we gave more serious thought to the meaning of higher education than we had ever done before. During that summer, our jobs were on the line; one member of the Board of Trustees said publicly that the university's troubles would be solved by firing about 20 of the dissident faculty members. Since I was one of 23 who had been publicly identified, it seemed likely that I would lose my job, tenure notwithstanding.

No event has had a stronger influence on my life and on my professional career than this academic revolt. During this time I began to articulate, probably more to myself than to others, the importance of every course in the curriculum as a contributing factor in the liberalizing education of every student. I began to wonder what liberalizing effect courses in job analysis, or in industrial training, or in personnel selection could have on individual students. These were tool courses. They constituted job training, not liberal education.

I should, perhaps, have found some place to have pointed out earlier a very great debt to C. H. Lawshe who, while president of the association, named me chairman of the first committee on professional education in Division 14. In setting out the charge to that committee, he made me aware, for the first time, that a distinction can be made between training and education. I confess to having been less than enthralled with that distinction; our document was probably concerned as much with training as with education, but the seed had been planted. By the time of our revolution, I was ready to take that distinction very seriously.

I began to teach introductory courses in industrial psychology, and the more advanced courses as well, as courses dealing with the relationship of man and his work; although the textbooks were traditional, the classroom lectures and discussions emphasized broad issues. For example, in a section on personnel selection, I would raise such questions as whether society benefited more by systematic selection for excellence or by a first-come, first-serve policy leading to a higher rate of employment. Mason Haire posed a real challenge when he wrote, "... it is strange that the liberal tradition of remaking the world to make it fit Man better, in contrast with the relatively passive caste-ridden Darwinian approach of the correlationist, should come from something called Engineering Psychology" (1959, p. 171). I wanted students to challenge the correlationist thinking they often brought with them. Our discussions were never profound, but issues were raised. It didn't bother me that we did not resolve them so long as the students began to think about them.

Most of the students in these classes were not majoring in personnel administration, nor were they planning to become industrial psychologists; most of them were taking the course because it offered three hours of elective credit at a convenient time of the day in a convenient corner of the campus. When I went to Berkeley, I found the students in the personnel psychology class just as diverse as those at Bowling Green. The following summer, 1965, when I taught at the University of New Mexico, I found even greater diversity and wrote a widely unread article about industrial psychology as an academic discipline (Guion, 1966a). It is not a very well written article, which may be why it has been so little read, but it represented for me the culmination of the thinking that began in the academic revolution on my home campus.

The Church and the Danforth Associates

My grandfather, my father-in-law, and one brother-in-law were (in the latter case, is) Methodist ministers; the church was a central part of my early life, and it has been a continuing part of my adult life. Theologically, my early thinking could be charitably described as eclectic; the church was, perhaps, too much a habit to think about it very seriously.

During the 1957-1958 academic year, my wife, Emily, and I were appointed to serve as Danforth Associates on the Bowling Green campus. The Danforth Foundation was founded by William H. Danforth, founder and president of the Ralston-Purina Company, as an educational foundation with, in its early days, strongly religious over-tones. In one of the foundation programs, the Associates program, faculty members and their spouses are jointly charged with providing a personal dimension to the educational experiences of students. The personal dimension can take many forms; it has sometimes been a tea and cookies approach in a faculty member's home. It sometimes involves study groups or weekend retreats. For me, one important form has been taking individual students on field research or consulting trips. Whatever form it takes, the faculty couple is expected to encourage students to work toward the twin goals of academic excellence and the articulation of their values. The foundation no longer has the strong religious overtones of the earlier era, but the emphasis remains on intellectual excellence in the context defining one's values.

When we were appointed, new associates attended a national orientation conference. At the first conference we attended, we experienced profound stimulation from a series of lectures by, and indeed the personality of, G. Davie Napier, then chaplain at Yale University. A basic set of theological convictions began to emerge from the experience. Another important source of stimulation was provided by a sociologist from Southern University, Lionel H. Newsome, now president of Central State University in Ohio, who spoke to us about events that occurred on his campus to the freedom riders, the Civil Rights activists in the south at that time. We found it incomprehensible that a nation, still giving lip service to its religious heritage, could allow treatment of citizens so antithetical to Christian precepts.

This was not new, of course. Questions of racial "tolerance" had long been taught, at least once a year, in the respectable, organized churches like the one in which I grew up. During the war, during ASTP training in Iowa City, I joined a group of students in boycotting the town barber shops and later in establishing a barber shop that would be willing to cut hair for Negro students. The quiet talks with Lionel Newsom, however, provided an experience of a first-hand report from a highly educated and cultured gentleman, and the experience had a considerably unsettling effect.

At a later conference of Danforth Associates, we met Wendell Whalum, professor of music at Morehouse College, who taught us to sing the freedom riders' hymn ("We shall overcome") and Dr. Benjamin Mays, the incredible president of Morehouse College, known, among other things, as mentor to Martin Luther King. Exposure to these men reinforced the conviction that denial of opportunity for employment for which one was qualified was essentially unChristian behavior.

For me, as one under contract to write a book on employment practice, any acquiescence in that denial of opportunity was a form of complicity.

In 1966, I was asked to teach a Sunday School class on theological perspectives. To teach that class, I had to learn a great deal that I had not previously known. I read theological expositions from the Confessions of St. Augustine through the Social Gospel of Walter Rauschenbusch to the more contemporary theologies of Niebuhr and Tillich. By this time the civil rights movement had culminated in the 1964 Civil Rights Act, and the racial unrest had melted into a more general malaise; Martin Luther King had been shot. Although its literature was quaint, the Social Gospel seemed to me to be a very powerful statement of the obligation of those who would call themselves Christian to serve others, particularly those who, by circumstances beyond their own control, had been exploited and shunted aside by society. It became a firm conviction that I should, as a professional industrial psychologist, be concerned with the problems of fairness and equality in employment opportunities.

The Civil Rights Act and Some Coincidences

In the summer of 1954, when my evenings were spent in a YMCA hotel room, writing that unfortunate book on motivation, my days were spent in a fellowship from the Foundation for Economic Education with the Motorola Company in Chicago. The home office, then at the Augusta Boulevard Plant, was located in the midst of a black ghetto area. The corporate Human Relations Manual had, on its front page, one of the most beautiful statements on equality of opportunity I have ever read. The only black faces I saw, however, were among applicants in the employment office. Discussions with some of the middle-management people suggested that Motorola was interested in the employment of blacks only insofar as necessary to avoid open warfare with the Chicago Fair Employment Practices Commission.

My association with Motorola never continued beyond that summer, although work begun that summer was expected to continue. My final report to the company raised the question of inconsistency between actual hiring practices and the human relations policy statement; I have never known whether these comments were seen as evidence of inability to observe accurately or as the ranting of a troublemaker. In any event, I rarely thought of the experience until the famous "Motorola case" came up during the debates of the Civil Rights Bill in Congress in the fall of 1963, and it made those debates seem more personally interesting. During the period of debate, between the two proposed Tower amendments, I was part of a panel that addressed a meeting of an association of personnel managers in the San Francisco Bay area. Casually, simply to illustrate another point, I mentioned that the concept of a moderator variable might do much to solve the general problem of inadvertent unfairness in employment and the specific issue of test fairness in the Motorola case.

The comment attracted some attention, and the program chairman asked me to return for a later program to discuss the topic. I did, some months later, but the mood had changed drastically. In the parlance of the theater, the audience sat on its hands. We learned later that nearly everyone had been instructed to listen attentively but to say nothing. My presentation was a plea for research in the Bay area on what has since been called differential prediction. There was no response. The program chairman, George Strauss, also editor of Industrial Relations,

seemed upset by the lack of response, so he asked me to write the comments for publication in Industrial Relations.

I didn't get around to writing it until I returned home. Dr. Richard Shore, who had taken my place in 1963-64 and stayed another year as an associate, offered critical comments and supportive reinforcement for drafts of the paper. The article appeared early in 1966 (Guion, 1966).

In 1965, Dick Shore left academic work to join the Department of Labor in its Policy and Planning Division. One of his early assignments was to draft an order for the newly-formed Office of Federal Contract Compliance concerning the use of employment tests. He asked me to come to Washington and discuss the order with him. It fit very closely the kinds of things I had written in my book and the Industrial Relations article; I was naturally quite pleased with it.

In meeting with Mr. Edward Sylvester, the first director of the Office of Federal Contract Compliance, I expressed the opinion that, if the order were issued and enforced, it would result in the elimination of unfair, inadvertent racial discrimination in employment within a generation. To Mr. Sylvester, this seemed an admirable goal. The argument was that, as decisions came to be made on the basis of valid predictors, young blacks would find that qualified people could indeed get jobs; they would therefore be more encouraged to seek job qualifications than had black youth of the past. In a generation's time, the memory of discrimination would be wiped out, and blacks, like whites, would develop qualifications according to their abilities and initiative and be rewarded for their efforts by finding employment consistent with those qualifications.

It seems rather naive, now.

To recapitulate, my book was published in the spring of 1965, the Industrial Relations article was published in the spring of 1966, and through the coincidence of an association with Dick Shore, the thrust of that article had been incorporated into a draft of the Testing Order.

Procedures had to be followed, however, before the issuance of a Federal order. I assisted the staff at OFCC in assembling the names of people whom I thought would be sympathetic with the objectives of the order, and a meeting was held in Washington. It was one of the more astonishing meetings I've ever attended. The document that I had believed to be so right was bitterly denounced by colleagues whom I respected. One psychologist, who had already published substantially on the need for equality of opportunity in employment, and who had written widely about the necessity for careful test validation as a basis for achieving that equality, said flatly throughout the morning session that it is not necessary to validate tests; psychologists, he claimed, develop through their experience a sense that enabled them, to identify good tests without the time and expense of validation efforts. After the shock wore off, and I was able to engage the man in a private discussion, I found that he made these comments for the benefit of his employing organization -- he would have found it embarrassing to go back home and say that the validation studies he should have been doing over the past several years, and didn't, were now going to be required by federal directive.

The meeting eventually became more positive in tone; constructive suggestions were offered. The OFCC staff committed itself to preparing a more detailed document, taking into account some of the more severe criticisms. A subsequent meeting was held with some of the same people and some new ones. Again, it was a half-day of negativism followed by tentatively offered constructive suggestions. Eventually, a continuing Advisory Committee was officially formed under the co-chairmanship of Howard Lockwood and Raymond Katzell. I had served previously as a consultant to OFCC; we had come to a disagreement and parted company after the departure of Mr. Sylvester, so I was not initially a member of that committee. However, I later replaced Marvin Dunnette when he broke his leg in a skiing accident.

It seemed an interminable amount of time, but the Testing Order was finally published (Office of Federal Contract Compliance, 1968). It was a far cry from the two or three-page document Dick Shore had originally drawn up, but the essential principles of that document were still intact.

It should be noted, perhaps, that in 1965, conventional wisdom considered the Equal Employment Opportunity Commission a paper tiger. The prevailing opinion seemed to be that the use of the courts would mean such long delays in achieving equality that none would in fact be achieved. The Office of Federal Contract Compliance, however, had the authority to lift a federal contract almost immediately if an employer were found in noncompliance with the equal opportunity provisions of that contract. This, it was thought, meant real teeth.

By the 1968 presidential election, however, the EEOC was making itself felt, and candidate Nixon promised that there would be but one Federal voice to "harass" employers on matters of equal opportunity. Fulfillment of that campaign pledge would and did require an act of Congress, but, early in the Nixon administration, pressures developed for a rapprochement between OFCC and EEOC. The Advisory Committee was convened, with EEOC representation from Dr. William Enneis and attorney Philip Sklover. The intent of these meetings was to produce a revision of the Testing Order that could be issued jointly by EEOC and OFCC.

At one point in these discussions, I represented the Advisory Committee in a subcommittee with a Labor Department attorney, Enneis, and Sklover to draft the actual wording of what might be issued as a joint order. The most important aspect of that mini-conference was the concern of the EEOC over a practice, according to their allegations, of deliberately choosing a test that satisfied the validity requirement but with the maximum adverse impact. That is, there was fear in EEOC that the regulation might play into the hands of prejudiced employers who would use valid predictors as deliberate instruments of discrimination against minorities. Written into that draft, therefore, was a provision that, if different predictors were found to be similar in validity, the employer should choose the one with lesser adverse impact; if adverse impact existed with a valid predictor, the employer should seek another predictor that would have less adverse impact. When we met a week later with the full Advisory Committee, the wording of this provision, and of others, evoked concern in the committee. It was to be rewritten.

The Advisory Committee to OFCC had a hurry-up-and-wait history. When a crisis was impending, OFCC officials would call a hasty meeting of the committee. When there was no crisis involving the Testing Order, the committee and its work would be forgotten. So it was with

this revision. Nothing more was heard from the agency we were to advise until after the EEOC issued its Guidelines in 1970 (Equal Employment Opportunity Commission, 1970). The draft the four of us had presented had been substantially changed during the hiatus of nearly a year. Provisions about the Advisory Committee had already expressed misgivings had turned into statements that were technically obscure or ambiguous, interpretable as requirements virtually impossible to meet. Moreover, by that time, data were beginning to accumulate suggesting that the differential prediction hypothesis was really not a very good one, after all. The language of the Guidelines said, in effect, "You have to look for differential validity and, if you find it, you have to use it." Since it was becoming apparent that regression equations for blacks and whites were very likely to be parallel if different at all, and to have lower rather than higher intercepts for blacks, a literal adherence to that rule would defeat the purposes of both EEOC and OFCC. The problem was seen as serious enough to the new management of OFCC that the Advisory Committee kept working diligently until it was able to publish a revised OFCC Testing Order (office of Federal Contract Compliance, 1971). A footnote to that Testing Order, which always seemed to me to be extremely important, acknowledged that any differences in the wording of the OFCC Order and the EEOC Guidelines either reflected differences in legal authorities or clarifications of the interpretation of the EEOC version; it also acknowledged that the Equal Employment Opportunity Commission had accepted these clarifications.

During much of this time, I had been listed as a member of the APA's Committee on Psychological Tests. The term of office was three years; the first two years passed without a meeting of the committee. In the third year, in April of 1971, the committee met to consider possible revision of the *Standards* (APA, AERA, and NCME, 1966). Because of the OFCC work, I believed the *Standards* needed supplementing more than revision; it seemed to me that there should be a document prepared for the guidance of test users to parallel the 1966 *Standards* which, as I perceived them, were directed more to test publishers. E. Belvin Williams, another member of the OFCC Advisory Committee, was liaison to the Committee on Psychological Tests from the Board of Scientific Affairs. He shared the view that the additional material was needed and seemed inclined to believe that the *Standards* themselves were due for revision. The committee decided to try to write a single document including both any revisions necessary in the *Standards* -- and it was originally believed that these would be few -- and new material for test users.

Previous sets of test standards were written by committees of leading scientists, but the social activism of the period seemed to require a far more democratic process. It was decided that one member would collect opinions from a wide variety of test users and incorporate their opinions in a draft revision. The other members of the committee would then challenge the draft and recommend changes leading to a document to be submitted for approval to the profession at large. Since I was scheduled for sabbatical leave in 1971-72, I was assigned to serve as the committee's scribe. Naively, we thought the job could be done in a year.

It proved to be very nearly a full-time job. Fortunately, the Educational Testing Service, where I spent the sabbatical, gave me a great deal of time to work on the project and committed its resources to the typing and distribution of various drafts. I traveled to Washington, Boston, and New York to meet with groups of people to discuss their criticism of the existing *Standards* and their suggestions for the new section for test users. A first draft of a

revision was prepared incorporating virtually all suggestions, including those that were contradictory. It reproduced verbatim the portions of the 1966 *Standards* for which there had been no challenge so that changes could be read in context rather than simply as amendments. A pencil line was drawn in the margin to identify material taken directly from the existing document.

Peculiar things happen; machines do not always work as planned. In this case, the duplicating equipment rarely picked up the pencil lines. As a result, people read the old material, not for context, but to criticize it. When we circulated the draft to the committee and to various eminent psychologists, including many who felt the 1966 *Standards* needed no revision, we received many criticism of old as well as of new material. Clearly, in the tenor of the times, with the particular emphasis on the civil rights implications of psychological testing, more revision was required than we had anticipated.

A major theme of the deliberations of the committee and of its various hearings concerned the civil rights of people being tested. The research literature on the questions of racial bias were sparse. Working definitions of bias or unfair discrimination had been written in my *Industrial Relations* article and by Anne Cleary in the *Journal of Educational Measurement* (Cleary, 1968); although her definition was couched in correlational term and mine was couched in the language of expectancy charts, the two definitions seemed similar to me. It was not until spring of 1971, when the Thorndike (1971) and Darlington (1971) articles appeared in the *Journal of Educational Measurement*, that it became obvious that there were a great many more problem in defining psychometric bias than we had originally thought. The research that these articles ultimately generated had not yet been done, yet we wrote principles to govern the development, evaluation, and use of psychological and educational tests as if we knew the answers to the research well in advance.

A disclaimer about the revised *Standards* (APA, AERA, and NCME, 1974) is in order. The chairman of the committee, Dr. Frederick P. Davis, was not satisfied with the various drafts for the 1974 revision, although his criticisms often were too general to result in specific changes. As we came closer to the end of our work, he became increasingly agitated by what he perceived to be its technical inadequacies. I have often felt that it was his personal disclaimer when, in the preface to the *Standards*, he called me its principal author. The statement is, in a sense, correct; I did most of the writing. Through the first three drafts, however, I wrote the things that were suggested to me by the various critics whose opinions were to be incorporated. By the fourth draft, I had also become disenchanted with the process and began to avoid including things with which I would disagree strongly. Nevertheless, the authorship of the *Standards* must be recognized as being far more diffused than the work of any one person. In the first place, much of the 1966 *Standards* did survive and was retained unchanged in the 1974 edition. In addition, specific standards and accompanying comments were often literally dictated by other members of the committee during its meetings. It would be very hard for me to point to specific provisions of the *Standards*, as I could point to specific provisions of the 1968 Testing order, and say that I could take either the credit or the blame for them.

The revised *Standards* is designed for a broad audience. As with earlier versions, it includes standards directed to educational testing and to clinical testing. Many people in industrial

psychology felt that it was not explicitly enough related to employment practice. Therefore, the Division 14 Executive Committee decided to write its own statement of principles and assigned Mary Tenopyr and me to the task. In keeping with the democratic principles that had been followed in writing the *Standards*, a committee of 50 people was appointed to review our work and to contribute to revisions of the material we drafted. Our purpose was to write a document that would be consistent with the *Standards* but would focus unambiguously on personnel testing. Nevertheless, Dr. Tenopyr and I wrote according to our own convictions and submitted the result to the larger committee for review. Some of the changes we sought in language or principle were not accepted. For example, we preferred to restrict the use of the term "validity" to either criterion-related validity or construct validity; we felt that, as a term, content validity represented both poor semantics and poor understanding of psychometric theory. Nevertheless, by majority rule, content validity remains in the Division 14 *Principles* (1975).

With this document, too, I do not want to be perceived as fully responsible for the content. I'm not renouncing either document; in fact, I think they both are about as good as democratic approaches to technical matters will allow.

The Present Trends

My present work and beliefs seem consistent with my early experience and attitudes. I am highly concerned with individuality. The primary unit of investigation and of measurement in psychology is, for me, the individual--not groups of individuals or organizations. I believe I would hold to this view even without the peculiar chain of events that led me away from the study of work in motivation, span of control, and other organizational issues to an emphasis on personnel testing.

Unless fortuitous events have, however, been undoubtedly responsible for the very great emphasis in my professional work on measurement and on the necessity for competent measurement. I did not deliberately set out to become a measurement expert, and I am certainly not much of one. There are far too many issues in measurement theory on which I must consult other experts in order to proceed. I have become known as a measurement expert not because of early background but, rather, because I needed a textbook for a course, because I wrote clarifying text material for that course at a time when textbook publishers were seeking manuscripts, and because the textbook was published in the same year that the Equal Employment Opportunity Commission and the Office of Federal Contract Compliance were established. The success of that book is surely due largely to its timing. Its timing and the coincidence of a brief period of working with Dick Shore led to my involvement in writing the OFCC Order, the EEOC Guidelines, the APA *Standards*, and the Division 14 *Principles*.

In spite of all of that involvement, however, some early background still shines through, and I have been perceived by some civil rights advocates as a turncoat who has abandoned respectable views. I have opposed, beginning with the AT&T hearings before the Federal Trade Commission, what I perceived to be a distortion of the intended purpose of the Federal documents. I have, on a number of occasions, attempted to testify in various District Courts on the difference between the meaning we intended and the meaning attributed to those documents by plaintiffs, attorneys and witnesses who were not a party to the writing. The distortion is, of

course, understandable in view of the fact that the EEOC has in fact been the dominant organization; it was not a paper tiger, and its responsibility was an immediate one, not one that could extend over a generation. The result has been a document to be used as a club, treating the bigoted and the unaware alike. It has been a club for getting jobs for people, irrespective of their qualifications, on what has subsequently become called a group parity basis.

From a theological perspective, it is not enough simply to say that minorities must not be discriminated against. If one is not to speak hypocritically of the brotherhood of man under the fatherhood of God, one must view unfair discrimination as wrong, regardless of the group identification of the victim. If one adheres to the notion, exemplified in the parable of the talents, that rewards should go to those who have earned them, then it is reasonable to assume that jobs should go to those who have developed the qualifications for them. The task, it seems to me, is for employment psychologists to find improved ways of meeting out inadvertent discrimination and improved ways of assessing the qualifications of people to do jobs. There are, of course, other tasks for psychology if discrimination is to be ended. There are social, developmental, and educational issues of opportunity to develop qualifications; psychology, if it is to promote human welfare or brotherhood, must deal effectively with these problems. But problems of broadening the vocational perspectives, increasing the motivation to learn, or decreasing the economic and psychological barriers to learning intrinsic to a prejudiced society -- these problems will not be solved by ignoring their effects in employment offices.

A final work concerns the narrowing of the scope of my work over the last 15 years. My teaching no longer runs the gamut of Industrial and Organizational psychology; my only graduate teaching is an introductory seminar for industrial students on research and the general course on measurement and correlational methods of research. Although personnel selection has been my area of specialization, it is not a consuming interest. I really do not like to be identified only as a testing specialist or a selection expert. I believe that personnel selection is important and that personnel testing is a useful tool in that selection. Selection is chronologically the first place for an existing organization to apply psychological principles in the development of its work force. That fact, however, does not make it the most important. Discovering and articulating the general principles governing the intentions of people to perform well or not, the principles of providing an environment that will promote growth, and the principles that will promote creativity and proficiency in one's work and make it a maximally satisfying experience -- such scientific work will prove far more important, in the long run, than showing organizations how to select employees. However, I believe that the principal emphasis of my work in selection should be generalized to a similar emphasis in all of these other areas: an emphasis on greater sophistication and competence in measurement. I remain strongly convinced that progress in these areas cannot go far unless those who specialize in them take heed of some of the lessons that are being learned in research on personnel selection.

The first of these lessons is straightforward: minority groups, including women, should not be discriminated against after they are hired just as they should not be discriminated against in hiring. Environments promoting growth, opportunities for satisfying experiences, and opportunities to form intentions to do well should be as available to people belonging to one group as they are to those in another. Research in these areas should address, not ignore, the

possibility of inadvertent bias. Discrimination against certain groups, except discrimination against the incompetent, is a poor way to maximize criteria.

Second, research techniques need to be better. Validation designs that were first enunciated in the 1920's, and were still enunciated in my textbook in 1965, are often inadequate. Problems have included throwing out good hypotheses about predictors because of inadequate sample sizes; validities have been claimed or sought against criteria that at best are suspect, more likely useless, and at worst actually opposed to the goals of the organization. I'm referring here to the ubiquitous use of ratings as measures of performance. Small samples and poor criteria are as common in other research areas as in selection.

Third, conventional measurement theory may be inadequate; it relies too heavily on the peculiar characteristics of the sample used and may therefore lead to very limited applicability of findings. Maybe, as Schmidt and Hunter (1977) have suggested, the old cry for situational validation was totally unnecessary. I am not convinced. Validity in an organization where people are very well trained, for example, is not likely to be the same as validity in an organization where training is haphazard. In short, it seems to me that personnel testers must move, and I am making such a move, toward the study of sample-free measurement, and the study of the limits to the generalizability of the results of research. The problem is not unique to personnel selection. Those who would study organizational environments, those who would study leadership, those who would study the aspects of work motivation -- all must pay greater heed to competence in measurement if they are to work toward the development of a fundamental, generalizable science that will promote human welfare at work.

I hope that, in any future version of an autobiography, I will be able to report that my work has moved forward to apply these lessons broadly.

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