An Autobiography of An Accidental Industrial Psychologist
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For many of us who came into psychology in the 1930's, our eventual career objective was medicine, but the economics of the period precluded going to medical school. This was certainly true of my friends and classmates at the City College of New York. I entered CCNY in September 1933, near the bottom of the Depression. It was not only the bottom of a national depression. Our family fortunes coincided closely with the fortunes of the nation at large. Nonetheless, I registered for and embarked upon a premedical undergraduate program. My father was a physician and an oral surgeon, and in my family, particularly on his side, practically all of the professions were represented, including medicine, dentistry, law, accountancy, teaching and engineering. The medical profession was in fact conceived as the destiny of most bright Jewish boys of middle class families, and for that matter, of Jewish boys of lower class families.

The Depression, however, caught up with us in a particularly limiting way sometime in 1934. The fact of the matter was that in the spring of 1934 it was totally out of the question that I register for the second part of the sophomore year. Our economic situation precluded spending money on a college education even at the level of a free institution where fees were nominal and the only things that you had to purchase were books and laboratory supplies. I spent that spring and the summer following working at odd jobs and at a hotel in the Jewish Alps, the Catskills. It was widely regarded that my employment there, compensated by a salary of thirty dollars a month plus room and board, was in fact a stroke of very good fortune. It did permit me to reregister at CCNY in the fall of 1935. By that time, federal programs were in place to provide supplementary student support, and because of my previous academic grade point average I was also awarded an undergraduate scholarship.

It was perfectly obvious, however, that entry into medical school two years hence was out of the question. Since I had had vaguely formed notions of becoming a psychiatrist, psychology seemed an appropriate second choice. Psychology at CCNY, in the middle thirties, was hardly a large enterprise. We were in a department combined with, and subordinate to, philosophy. Chaired by Harry Allen Overstreet, the genial defender of social philosophy, and a favorite of the lecture platform, the Department also included Morris Raphael Cohen, probably one of the most stimulating and challenging professors in my academic history, and Ernest Nagel, who attempted in a modest way to teach us a little bit about logic. In psychology, the department included just four people. Professor Marsh, a patriarch of the old psychological tradition, John Grey Peatman, Henry Garrett's son-in-law and eventually Chairman of the Department of Psychology and probably the most versatile of those who taught psychology, George Milton Smith, and Max Hertzman.

As I recall, I had two courses with John Peatman. One was the single offering in statistics, which was so orphaned that it had the course number Unattached 15.2. Our text was Henry Garrett's early 1930's statistics book, which was innocent of the analysis of variances Students t-test factor analysis, chi square, nonparametric statistics generally, and as a matter of fact, probably seventy percent of what we now include in a basic course. It also included concepts and approaches that have disappeared from current statistical psychological usage, as, for example, the Critical Ratio. The other course I took with Peatman was psychology of business and industry; the author of the textbook used I have completely forgotten. My main mentor, and the person who encouraged me most, was George Milton Smith, a man characterized by dry humor, liberal attitude, and versatility across a number of fields. He taught, for example, abnormal psychology, social psychology, and a number of other titles which today would be divided among credentialed specialists in sub-fields of psychology. While Max Hertzman was on the faculty, I never took a course with him. Milton Blum was at the Business School on 23rd Street. I never took courses with him, but I did serve for about a year as his teaching assistant.

Consistent with my then interest in clinical psychology, I obtained through the Bureau of Social Research, as I think it was called, a nonpaid research assistantship at the Psychiatric Clinic of the Court of General Sessions, popularly known as the Tombs, under Sol Mackover. He and his wife Karen, who was at Bellvue Hospital working with Loretta Bender, were the godfather and godmother of New York clinical psychologists. I spent almost two years at the Psychiatric Clinic. At that time the clinic was involved in assisting David Wechsler in the collection of normative data for the newly-available Wechsler-Bellevue test, and I gave many of them to felony indictees.

As a research project, I did a study of the reliability of psychiatric diagnoses. Collective interviews of a series of persons examined were conducted by the three psychiatrists who formed the psychiatric clinic staff. From each psychiatrist I obtained both ratings on traditional trait scales such as aggressiveness, hostility, introversion, and so on, and, much more important, from each psychiatrist a diagnostic label using the then available American Psychiatric Association nosological system. (This labeling system operated at three levels. First was the overall classification, such as normal, psychotic, psychoneurotic, and mentally deficient. Under that was specification of particular diagnostic labels, such as, for example, under the general category of psychosis, schizophrenia, paranoia, manic-depression and so on. The third level referred to personality syndromes.) My research was originally conducted within the context of an honors class enrollment at CCNY. This was essentially a program of independent study on the basis of which you submitted either a literature review or a research paper. I wrote out the results of my research. What they showed was a devastating lack of agreement among the three psychiatrists with respect to diagnostic labels. When I submitted my paper to the chief of the clinic (psychiatrist Dr. Walter Bromberg) for clearance and possible publication, his first reaction was to forbid it. However, after several years of negotiation, and after he had left the clinic himself, he agreed that it could be published if the locus and the personnel involved were sufficiently anonymous. My earliest paper therefore dealt with the reliability of psychiatric diagnoses. It became a Psi Chi prize paper, and the award paid my way to one of the first APA conventions I attended.

After graduating from CCNY, officially in February 1938, I obtained through then Commissioner Austin McCormick, (a "liberal" criminologist who introduced behavioral scientists into New York prisons), of the New York City Department of Corrections what was intended to be a clinical internship at the New York City Reformatory for Boys at New Hamptom, New York, sixty miles from the city. I well remember repairing up there late one night, in a black limousine which he drove at an alarming speed, with the Superintendent, a man by the name of Sacher. I spent about six months in a sort of internship status. That means that I obtained room and board and nothing more.

Mine was an essentially unsupervised internship. I discovered when I arrived that I was the psychological clinic. Once every two weeks or so the chief, and only, psychologist in the Department of

Corrections, Bertram Pollens, would visit and review some of my cases. I made every possible mistake that a recent bachelor's psychology graduate could make, and in fact probably invented a variety of mistakes that most candidates never thought of.

Shortly before my term at the New Hampton Reformatory drew to an end, I had an offer from David Shakow for an uncompensated internship which would provide room and board at the Hartford Retreat. Not knowing then what I know now about the splendid training that was available from Dr. Shakow, and reacting to six months of nourishment and shelter but no pocket money whatsoever, I turned down the offer and returned to New York City.

There, living for almost two years in the most penurious of circumstances, I spent a great deal of time looking for employment. The fact of the matter was, that a bachelors' degree from CCNY, even adorned with Phi Beta Kappa and a Cum Laude degree, appeared to have no economic value. I particularly remember visiting the personnel departments of the major corporations in New York City such as AT&T, New York Bell, Metropolitan Life Insurance, and so on. Most of them had some psychologists employed, generally as adjunctive to or actually administering their personnel departments. However, they were all very candid in telling me that industry was not prepared at that point in time to hire Jews in technical and professional jobs.

In the Spring of 1939, having survived by a variety of part-time jobs, I took an examination which was to determine pretty substantially the rest of my career. It was the Junior Professional Assistant Examination, in the option of psychology, for the federal government.

Between the time I returned to New York City in 1938 and the Summer of 1940 when the results of my examination yielded a job in Washington, I continued my education at the one school of which I was aware at which graduate education was substantially free. That was the New School for Social Research. At the New School, students such as myself could work off their obligations for tuition on the basis of working about four hours for every hour of tuition credit. For almost two years I worked in the library of the New School and took courses in the Department of Psychology and from miscellaneous others who were not in the Department. Prerequisites and requirements, if they were stated at all, were very loose. Although the courses that I took at the New School bore little relationship to my interests in clinical psychology on the one hand, and probably less relationship to my eventual occupational attainment, there was no question that this is the most stimulating part of my educational experience.

The Graduate Faculty in Exile had giants in their fields under whom it was a privilege and a challenge to sit. In psychology, of course, the principal figure was Max Wertheimer, who taught a series of courses all of them labeled simply psychology. In addition, the faculty included such people as Karen Horney, Ernst Liss, Bronislaw Halinowski, Horace Kallen, Kurt Rietzler, Hans Speier (who became Director of the Office of war Information), and others perhaps of lesser repute at that time but of great gifts. In addition to the faculty, there were people in a sort of postdoctoral transitional state who, as I recall, didn't teach but sat in the classes of people like Wertheimer. Among them were Solomon Asch and George Katona. When the time came for me to consider a master's thesis I approached Professor Wertheimer, and he assigned me to George Katona. We did in fact begin on outlining a thesis, but that project was dropped when I left New York in August of 1940.

The shaping of my interests and career in the field of organizational industrial occupational psychology was largely the result of having passed the Federal Civil Service Examination. The

Occupational Research Program had been organized early in the 1930's to provide the new United States Employment Service with tools and techniques for hopefully placing unemployed workers into jobs. In the years between 1935 and 1939 the service, directed by Carroll Shartle and staffed with many recruited from the Employment Stabilization Institute at the University of Minnesota, had made a number of very significant contributions.* In the first place, the program organized and published the Dictionary of Occupational Titles a monumental undertaking which clarified, classified and described the jobs in American industry. In the second place, the Occupational Research Program had embarked upon extensive test development for vocational counseling and employment selection purposes, and had produced a very large number of aptitude tests of considerable variety. In addition, what was a major innovation, the ORP developed a kind of test called Oral Trade Questions, which were designed to assess the level of achieved knowledge in particular trades and crafts. The program, unfortunately, was wound down through fiscal 1939 with very large staff cutbacks so that by the Summer of 1940 only a skeleton staff existed. In anticipation, however, of the defense and war efforts, the Occupational Research Program was very substantially refunded beginning in August 1940. It eventually worked its way up to a staff of well over three hundred. In August 1940, twenty-five of us successful competitors in the Junior Professional Assistant Examination for Psychology arrived in Washington. We reported to work on a muggy hot day, the fifth of August. A month later, another group numbering forty reported for work, and staff additions continued throughout the war period. Members of the program included many who were to become distinguished leaders in the field: Ernest McCormick, Ernest Primoff, Nathan Jaspen, Beatrice Dvorak, Daniel Miller, and others. Although originally we were located under the Social Security Board, we were soon moved, first to the War Manpower Commission and then later in the war when the War Manpower Commision was being dismantled, into the Department of Labor.

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Our original mission was to provide the Employment Service with the tools and techniques to transfer people from non-defense occupations to war occupations. An elaborate and interlocking program emerged. The psychologists for the most part were involved in three major activities. The first was an extension of the Dictionary of Occupational Titles and the creation of the supplementary techniques in which, for example, jobs would be grouped on the basis of aptitude characteristics rather than on the basis of work processes or industry codes. The second activity involved the continued development of tests, and resulted in the production of the General Aptitude Test Battery. The third major activity involved the creation of what became known as job families for individual jobs and for whole industries. The individual job family, for example, began with an occupation like machinist. By the use of descriptions of the job collected in the field, and by the use of the Workers Characteristic Check List which the Occupational Research Program had inherited from Viteles' Job Psychograph by way of the Employment Stabilization work by Beatrice Dvorak, jobs drawn from other industries would be grouped on the basis of similarity of work processes and aptitude requirements in increasingly remote groups. The basic idea was that the Employment Service could transfer to a critical occupation (such as machinist) people who had been employed in occupations and industries which shared at least some of the aptitudes and skills required for the job of machinist, but were not defense related. From individual job families we went to industry studies, in which we would take as a referent each significant job in the industry, for example, coal mining, and then list in successively remote group's on the basis of this aptitude data, work process, and so forth, jobs from other industries, particularly from non-defense or non-essential industry, that were related to each referent job.

A culmination of this job family work was the development of a series of volumes toward the end of the war which related military occupational specialties to civilian employment. These volumes were designed to facilitate demobilization of the military back into the civilian economy. We published three. One was for civilian occupations related to military occupational specialties for enlisted personnel. Another was for civilian occupations related to military occupational specialties for officer personnel. The third was a volume dealing with naval personnel. We probably were somewhat naive in putting these documents together, because of all of our publications these received the most widespread press coverage and of the most hostile sort. The main thrust of the attack was that we were putting together, for enlisted personnel, transfers back into the civilian economy solely in low-level, unskilled and semi-skilled occupations. (In fact, if you try to identify on the basis of aptitude requirements civilian occupations related to riflemen, there are very few.) For officers, of course, the whole spectrum of managerial, professional and technical jobs in the civilian economy seemed to be related to their military occupational specialties. In any event, the war period saw the publication of the General Aptitude Test Battery, the development of the notion of occupational aptitude patterns, the various supplements to the Dictionary of Occupation Titles, and the area in which I worked for the most part, namely, job families.

In 1947, the Occupational Research Program was substantially dismantled. From a peak employment in about 19441945 of 300, reductions in force left a residual staff of not more than 30. Reductions in force were conducted on a hierarchical basis, so that supervisors would give the bad news to their subordinates and then they in turn would get the same bad news from their own supervisors. Those of us who had been in the government for six or seven years did have transfer rights. In fact, there was an opening in the Personnel Research Service of the Adjutant General's Office, under Edwin R. Henry, to which I was referred. I remember that Ed interviewed me, and expressed the hope that I'd turn the job down because he had a person on his staff whom he would like to promote into the available opening.

Well, by that time I had become tired of the District of Columbia and of the Federal Government as an employer. Also, I felt that if I didn't complete my education soon, I probably never would. I had managed over the seven years in Washington, working around travel and other commitments, to complete a master's degree at the American University. This was a degree primarily in public administration and personnel management, but my thesis was in the industrial area. It was a factor analysis, after the style of the studies that Charles Lawshe had been doing at Purdue, of the Navy's point-rating job evaluation systems. The most notable thing about that study was the number of hours I spent at a desk calculator rotating) using Thurstone's group centroid techniques.

] In any event, in that year of 1947, while attending the American Psychological Association meeting in Detroit, I ran into C. R. Carpenter, who had just recently acquired a very substantial grant from the Special Devices Center of the Office of Naval Research for studies of the use of films and video-tapes in mass training. He was looking for some people whom he could add to his staff as research fellows. Ray hired three of us: Nathan Jaspen, now Chairman of the Department of Educational Statistics at New York University, Sol Roshal, and myself. We were the first three fellows in a program which has extended now over almost three decades. Jaspen and I set the pattern of the research for a very substantial period of time. We designed the research programs. We assisted our graduate students. We did most of the data analysis. We would take their data on punch cards to navy installations where we had access to large IBM facilities. We could run substantial quantities of data and bring back the results. The students didn't really do very much of their own data analysis. For my own personal development, what the Instructional Film Research program contributed was a very extensive exposure to the whole field of industrial training. How do you do it? What methods are most effective? And how do you evaluate results? I would have been happy, I think, to have continued at Penn State almost indefinitely and, as a matter of fact, I had spoken to Bruce Noore, who was then Head of the Department of Psychology, about this possibility. However, my then wife had grown up at Penn State. Her father was a Professor of Agricultural Engineering and she was born and grew up at Penn State. It was then a very small town, and Ruth wanted to leave.

In 1952 when Ruth and I decided to leave State College, even if I did receive an offer from the Department, a number of opportunities appeared, including a consultantship in Cleveland, another in Cincinnati, and one in Chicago with Inland Steel. What made me decide upon going to Inland was my wifes severe objection to my taking on a job where I would have to travel a great deal and leave her alone with our then two small children. At Inland Steel, my first job was research consultant in a small social science research organization at the main steel mill in Indian Harbor, Indiana. It was in fact, a very interesting employment, because we were substantially dissociated from the production organization and could do practically anything we wanted to do.

The Research Department consisted of five people at that time a labor economist, a graduate in general economics, a social psychologist, a psychologist trained at the University of Wisconsin, and myself. We were physically remote from the plant in rented quarters in downtown Indiana Harbor (although toward the end we were moved into a basement in a building, inside the plant grounds.) Relating to plant management was difficult for a number of reasons. In the first place, many of our assignments came from "Downtown" (i.e., company headquarters in Chicago), which was apparently resented because we were a charge on the Harbor Works payroll. In the second place, many of our studies seemed to intrude upon and constitute a threat to departments such as Personnel and Training, on which our studies most strongly impinged. I was informally transferred to the corporate Industrial and Public Relations Department in about 1954, and by 1956 all the others who survived had been transferred out. The Research Department ceased to exist.

Shortly after I was hired, the Inland Steel Company faced a very substantial strike. During the period of that strike I did a study which eventually in 1968 became the substance of my presidential address to the Division of Industrial Psychology. It was a study of grievances. I was able to collect information on almost 1600 grievances: who had them, what their supervisors were like, what the grievance committee men were like, and so on. I'm not going to summarize my results here: they are given in the paper that I eventually presented to the Division of Industrial Psychology and also published in Personnel Psychology. During that strike also I digressed from psychology to develop a computer technique for the analysis of certain stress tests involving steel. There was a complex mathematical formula by which steel stress was analyzed, but the formula and the handwork were so complicated that it took several hours per test. Because of my knowledge of computer technology and of mathematics generally, I was able to devise a statistical technique which aborted all of the handwork., so that during the strike the company was able to make more tests of steel specimens than they had in their previous fifty years of existence.

One of the areas in which we were very interested was the area of employee attitudes. At that time, in the middle 1950s, there were available a number of employee attitudes survey techniques, such as, for example, the Brayfield-Rothe Scale, the SRA Employee Aptitude Scale, and a couple of others. My idea was that we would do a study of the relative validity of such questionnaires against assessment of employee job satisfaction by supervisors and union personnel. To do this study we selected the Newark, New Jersey, plant of Inland Steel Container. We knew a great deal about that plant because it had been the subject of a book, Patterns of Industrial Peace . I administered the SRA Inventory, and a number of other instruments including both cognitive and personality measures to about 300 employees. My study and a similar one by Melanie Baehr at the Industrial Relations Center of the University of Chicago were reviewed by Bob Wherry, for Personnel Psychology. Wherry disagreed with Melanie and me about our use of Thurstone's oblique solution of our correlation matrices, and proposed collapsing both studies into one, leading to a general morale factor in place of our three or four factor solutions. Melanie, who had been a student of and collaborator with Leon Thurstone, objected vigorously, and Personnel Psychology published the three articles together Melanies, mine, and Wherry's. A lively debate between Melanie Baehr and Bob Wherry followed in succeeding issues of Personnel Psychology, and one school, Purdue University I believe, used the set of papers as text for a course on applied factor analysis.

In the steel industry, I worked on a variety of topics, most of them very interesting, including one that was a study of the effects of technological change: what effects it had on employees. That was done by a joint industry-wide committee, with representatives from U.S. Steel, Bethelehem, and Inland. Another study surveyed training in the industry. This was a joint industry-union committee; I was chairman of the industry subcommittee. The union chairman was a fellow by the name of Joe Goin. I will never forget him. Goin accused me, although I wasn't even around at the time, of being involved in the 1937 "Republic Massacre. This experience revealed how deeply the history of the unions has been engraved in the psyche of union personnel. In any event, Joe and I eventually agreed that I was not at the Republic Massacre, and I didn't shoot anybody, which of course I didn't. We also came up with a common agreement on the question of training programs for the steel industry. A third industry-wide program I was involved with was as chairman of a committee that looked at testing. I left the industry in 1968 and I don't know where that study came out.

I learned a great deal from the steel industry. I got to know people like John L. Lewis, Dave McDonald, and others who were very important to the industry. I was exposed to the reality of union-management relations, as opposed to the text-book versions. I remember vividly, for example, an illegal work-stoppage and walkout. The official record indicates that the shop steward was objecting to a new industrial engineering job standard, and many hours were spent negotiating the issue. The real reason for the walkout was that the steward wanted to get even with a general foreman who was "messing around" with the shop steward's girlfriend.

At one plant, union-management conflict was resolved off the record when the local union president and the industrial relations manager, who had been in chronic conflict, beat each other up in a no-holds-barred fight in an alley behind the plant, and became firm friends thereafter. I believe that one of the reasons for the failure of psychologists to have much of an impact on industrial relations is that they read a manifest record which has little relevance to the underlying reality. Also, union leadership has been much more stable and longer enduring than management, and the conflicts through which the union leaders came are part of their living reality. These conflicts are not even included in the histories of the current breed of industrial relations managers, who are products of law school and business administration programs that did not even exist in the "times of the troubles."

But in the late 60s the "intellectuals" started to leave Inland Steel. William Caples, the Inland vice-president of industrial relations, to whom I reported, left to be president of Keynon College. Frank Cassell, who was another of my associates in industrial relations, took leave to become director of the United States Employment Service, and after a short return, left to go into private practice and to become professor of industrial relations at Northwestern. Richard Nelson, who was third among the "intellectuals," left to become president of Northern Illinois University.

I. E. Farber, who had been a Spence student at Iowa came to head the Department of Psychology at the new University of Illinois at Chicago Circle. To develop an industrial psychology program, he sought to recruit a leading industrial psychologist. The first fellow he attempted to recruit, and in my opinion probably the best, was Marvin Dunnette. Dunnette wasnt interested, so Farber looked around and heard about me. At that time, I was past-president of State of Illinois Psychological Association, chairman of the State of Illinois Psychologist Examining Committee, and currently president of Division 14. And of course I had published about eighty or so papers, monographs, etc. Farber asked me whether I would come into the Department. That was about 1966. To tell the truth, I was interested but I had cold feet, so I turned him down. But with everybody leaving Inland and my own interests not being realized there, when he tried me again in late 1967, even with all my misgivings because I had been out of academia for so long I said yes, and I joined the faculty at the University of Illinois in January 1963. What Farber wanted me to do, and what in fact I did, was construct a program for graduate industrial-organizational psychology. For that program I wrote course descriptions, outlines, and what not for a series of undergraduate and graduate courses beginning, "Psychology in Business and Industry". In all, the package includes nine courses, which still constitute the program. We put these courses in, and I negotiated with the Management Department. I said, we have all these courses and you people are training individuals to be useable in the industrial context. Why don't we cross-list some of them? We did. Today, in the graduate level courses and the advanced undergraduate courses, about one out of three students is a Management major.

When I joined the University in January 1968, of course I didn't have any grant money, but by the end of 1963 I did. For the following, seven years, I had grant money from a whole variety of federal and state agencies, one after another. I was in charge of a program known as the State of Illinois Cooperative Test Research Program. I had a salaried staff of about seven people and supported about six to ten graduate students. We conducted a large research program on employee selection in merit systems. Out of that research we published many papers, on issues such as the effects of read-ability on test performance, on selection for police, for clerical jobs, and many others. For selection for clerical jobs we did job analyses and developed a differentially-scoreable battery for use in selection for ninety clerical occupations. Out of the program came an omnibus-purpose item analysis computer program, and prototype tests for a wide variety of occupations, using innovative test formats, including the job element technique pioneered by Ernest Kromoff at the U.S. Civil Serice Commission.

In 1972, Ruth and I were divorced, and I married Judith Cates, a Yale Ph.D. in Sociology, a past member of the faculty at Case-Western Reserve, and, when I met her, Associate Director for Policy and Planning at the American Psychological Association and extremely knowledgeable in both sociology and psychology. Now we work closely together professionally.

In 1974 Judy and I lined up a sabbatical at the University of the Witwatersrand, in Johannesburg, South Africa for 1975. In December of 1974, I broke my hip, however. I ended up in the hospital for a month and in recuperation for a couple of months. My wife went ahead to South Africa to set up a household, and to start teaching. When I arrived, as a Visiting Professor of Sociology, I gave lectures on the sociology of deviance and on intergroup relations, and we conducted a number of joint research projects, most notably one on the American transilient and his/her adjustment to South Africa.

During my later years at Inland Steel, I had begun consulting with other companies in a small way; this expanded greatly after joning the University of Illinois. Most of my consulting--our consulting now, since Judy is my principal collaborator--is in three areas: individual assessment for management and security positions (we are phasing this out); development of selection procedures for individual companies or whole industries; and participation in Title VII litigation as an expert witness. In the last connection, I guess I have about equally alienated both colleagues in industry by testifying for plaintiffs in such cases as EEOC v. AT&T and Douglas v. Hampton, and plaintiff-oriented colleagues by testifying in defense of companies where they represent the plaintiffs.

The most interesting (and lucrative) part of our consulting business has been in the development and validation of selection batteries, for such companies as Smith-Corona Marchand (salesmen, typewriter repairman, office machine technicians), Victor Comptometer (similar occupations), Greyhound Bus Lines (bus drivers, service personnel, mechanics), Greybar Electric (clerical workers, salesmen, managers), and Commonwealth of Virginia (state police). Our biggest project, that extended over six years, involved the development and validation of a battery to select entry employees for banking. Supported by the American Bankers Association, the study involved a sample of about 10,000 in an elaborate predictive validity design, and resulted in a multi-test battery for which we devised a multiple-hurdles approach of Occupational Aptitude Patterns (modeled somewhat after the GATB) for thirty-five banking occupations. The battery is merchandized to the nation's 14,000 banks by the ABA.

In 1968, I began research on a little-investigated area: the assessment or prediction of employee honesty. Employee theft is a major problem in retailing, and honesty a critical issue in security work, both public (e.g., police) and private money transfer or guard work) positions.

Working with a paper and pencil test--The Reid Report--I have since completed and published eight or nine studies* which show that it is possible to predict probable applicant honesty with a high degree of reliability and validity. The test is now widely used in retailing, in a few security-conscious industries (e.g., drug manufacturers), and in selection for a wide variety of police and other security work.

*See Bibliography

I had started out to become a psychiatrist, but my experience in assessment suggests that that would have been a poor choice, because I really don't like to listen to people's problems. I like to "look at the data." The accidental process of getting where I am was more selective and more valid than my own original preferences.

This test, a collection of 90 yes-no items of which x 70 are scored, measures two dimensions--Punitiveness, or the extent to which the respondent is willing to punish someone else for different crimes of theft; and Projective Attitude, of the extent to which the respondent himself/herself "thinks criminal" (e.g., "I get a kick out of it when a smart lawyer gets a crook off the hook.") It has been demonstrated to be practically unfakable, and to have no adverse impact against members of minority groups or females. Against verified theft history criteria, it has a validity of about 0.60.