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This autobiography is an edited version of a first draft prepared by Bill McGehee in 1978. The original has several references to "inserts" that cannot be found. It appears, however, that the inserted texts were rewrites of sections that were crossed out. I have kept the content of the crossed out sections, but have rewritten them for clarity and style. I also added the headings throughout the text to aid the reader. I believe I have been faithful to Bill's thoughts and words. (Paul W. Thayer, January, 2002)

An autobiographer should avoid writing an *apologia pro vita sua*, or a Horatio Alger story. He must decide what hills to climb again and what valleys to slug through again. Ross Stagner has set the direction he hopes will be taken by each autobiographer. It is to emphasize the events in each life which illuminate the growth and development of industrial psychology as a science and a profession. As I mentally reviewed my participation in the growth and development of industrial psychology, it seems to me my association with it could be used as evidence for a disordered universe. So, I hope too much thunder in the index will be tolerated by both the editor and the readers.

Paris, Tennessee

I had no awareness of psychology as an academic discipline or a possible vocation until after I had finished undergraduate college. In fact, I do not remember the word "psychology" being used or the subject discussed until I was 20 years old.

The term was not in the lingua franca of the inhabitants of the small West Tennessee town in which I spent most of my youth. This town, Paris, in no way resembled the more famous Paris. The major and perhaps the only cultural and intellectual event in the town was the annual visit of the Redpath Chautauqua. Parisians attended more to see the magician than to hear William Jennings Bryan speak on the "Cross of Gold."

The inhabitants of Paris were for the most part native born whites of North European descent. There was a certain amount of Puritan attitude toward pleasure, especially someone else's pleasure. The Protestant work ethic was rampant. Education beyond learning to read, to write and to figure had little status in the community. Males were expected to become productive members of their families at an early age, and the place for women was strictly in the home.

Paris in many ways was a good town in which to spend your youth. There was little wealth or poverty. Socioeconomic class distinctions were rarely present and never for children. Paris was only a short distance from the outdoors the intriguing valleys of the Tennessee and Big Sandy rivers. Most males became avid hunters and fishermen.

My father was a railroad conductor who had left his father's farm at an early age to find employment. My mother was the daughter of a self-educated lawyer. She had more than the usual formal education roughly two years of college. I am the oldest of four children, another boy and two girls.

Except for one event, my life was like that of other youngsters in Paris. The incident was an accident in infancy a burn which left my right hand practically useless for all practical purposes. As a result, I could not do the normal things most youngsters do until plastic surgery at age nine gave me reasonable use of my hand. Even after surgery, my manual skill was low. So, I developed little or no interest in any

activity requiring manual skill, or use of tools or machinery. I did, however, become an ardent hunter and fisherman. My hands allowed me to play football, at guard, of course. I played the game with success in high school, and with little success in college.

My mother, to occupy my time when I couldn't play with other children, told me stories, read to me and played games with me. I learned to read at an early age and have been a voracious reader ever since. I found as I grew older that while I could not compete in motor activities with others, I could compete, with success, at the cognitive level. I made good school grades and performed well in forensic activities. This, of course, endeared me to teachers, but rarely improved my status with my peers. It is possible that this led to my lack of interest in the physical sciences in contrast with liberal arts subjects.

My injury had a direct bearing on my attending college. Children from affluent families were usually the only ones in Paris who attended college. The financial situation in my family was such that sending a child to college was out of the question, yet my mother had determined early that I would have a college education to compensate for problems I might encounter in making a living due to my manual handicap. Early on she instilled the idea of college in me. I saved some money from part time and summer jobs for this purpose. Through the efforts of a friend of my mother, I was given a scholarship by the University of the South, Sewanee, Tennessee. I was also promised work in the dining hall. These, with some help from my grandfather, provided enough money to see me through my first year at Sewanee.

My maternal grandfather served as a model for me. (Of course, neither of us knew what that meant then.) He was a self-taught lawyer, born near the end of the Civil War. He was also an enthusiastic hunter. I spent a great deal of time with him at his home and at his office.

My grandfather gave me my first lesson in sportsmanship. He had given each of his four grandsons a shotgun when they reached age 12. He taught each how to use his gun safely. I was bird hunting (always doves in the South) with him shortly after receiving my gun. We came upon a covey of birds still on the ground. I was about to shoot when he stopped me. He said, "You don't shoot birds on the ground. Always give them a chance." He took my gun away for two weeks while I was to think about why I shouldn't shoot birds on the ground.

Grandfather was a confirmed skeptic. He once told me that if someone told me it was raining, I should go outside and get wet before believing him. He also had low tolerance for cheats and phonies.

Good and sometimes great teachers have had a profound influence on my life. The first was a Mr. Weston from New England, a Princeton M.A. I never learned what brought him to Paris, but he taught Latin and history there for over thirty years until his death. He made the hedgerows of Normandy as vivid as if you had been one of Caesar's centurions trying to see through them. He was a teacher who never segmented learning. A history test turned in with correct answers and mistakes in spelling, grammar or sentence structure was marked down and returned for the student to correct the language mistakes. I am certain his model made me a more effective teacher than any course in education I had to take to get a teacher's certificate in Tennessee.

I had especially good teachers in English and mathematics in high school, and an especially poor one in chemistry. As a consequence, I avoided all courses in the physical sciences in undergraduate school.

Thus, it was not until graduate work that I had any understanding of or appreciation for the scientific method of investigating phenomena.

College

I had no specific vocational aim when I enrolled at Sewanee, nor did I develop one during my four years there. I was also ignorant of what college life would be like, given my small town background. Thus, my adjustment to Sewanee was difficult, and I was not well accepted by other students until my junior year. By then I had competed successfully in cognitive activities, and my financial difficulties had diminished through an assistantship in the English Department, being head waiter in the dining hall and having four or five part time jobs, including night relief operator on the telephone switchboard. I made more money as a senior than I did during the first three years after graduation, twelve hundred seventy-five dollars.

The Sewanee curriculum was modeled on the classical one of Oxford. Four years of an ancient language was required for graduation. Philosophy, English, modern foreign languages and mathematics were emphasized. I avoided biology and the physical sciences except for the one required course. There were no majors in today's sense, but students had areas of concentration. Mine were philosophy, English, Latin and Greek. It is somewhat ironic that in spite of my mother's and my concern about a vocation, my education at Sewanee had little value on the job market.

So far as I remember, Sewanee offered no courses in psychology. The nearest we came was in a philosophy class in which we studied William James, but as a philosopher, not as a psychologist. Neither can I remember any discussion of psychology among students even in its popular sense in the late 1920's.

I found my courses in philosophy interesting, but they left me with a sense of incompleteness. What were the answers? I know, of course, that there is no categorical imperative. But each philosophic system seemed to have the answer. What was the evidence? What was the proof? Or, was philosophy just a word game, and were distinguished philosophers merely individuals with high linguistic ability? (I am, of course, phrasing this in 50 years of retrospection; not the way I might have at Sewanee.)

With few exceptions, I had excellent teachers at Sewanee. Teaching was their major mission, not research or publication. Classes were small; total enrollment was about 300. You knew your teachers as individuals as they knew their students. Skepticism and inquiry were encouraged, as well as originality. Few limited their teaching to their subjects. One learned history in foreign language courses and philosophy in mathematics courses. H. L. Mencken's *The American Language* had a place of honor in an English class. In addition to broad general knowledge, I acquired and internalized a code of behavior that has remained with me.

In June, 1929 I was graduated from the University of the South. Having no marketable skills nor vocational aim, I thought vaguely of writing as a vocation, so I got a job as a cub reporter on a metropolitan daily. I lost that job after two months for failing to meet a deadline.

Teaching in the Public Schools

Through an uncle who was a county school board member, I was offered a job teaching in the Puryear, Tennessee, High School. (Even then, it was difficult to find a Latin teacher.) I accepted the job with little

enthusiasm and no investigation. On reporting for work, I found I was also to teach mathematics, and to coach basketball and baseball. As I had played neither sport and had little interest in them, I got a book on how to coach these sports. My teams had little success, but that had been true under the previous coach.

I found teaching interesting, but had no desire to continue in it. Early in 1930, I began looking for other jobs. As the depression had hit Tennessee early, there were few opportunities. Whoever had a job did everything possible to hold onto it. I decided I had better secure the 27 hours of Education required for a permanent teaching certificate. I managed this by attending Murray State Teachers College in summer sessions of 1930 and taking a correspondence course. The latter was my first course in psychology and was strictly Titchnerian. This introduction to JND's and watered-down neurology failed to fill me with enthusiasm for psychology.

I got my permanent certificate and taught until the Fall of 1931. I moved to teach Latin and coach at Donelson, Tennessee, a town about six miles from Nashville, where I remained until February, 1936. This move had several results. I was paid ten dollars more a month (\$90) in cash, not in county script that was discounted 10 percent at the bank when you needed real money. It allowed me to coach the sport I knew something about, football. Also, I met Hortense Ambrose of Nashville, and we were married in 1934.

Graduate School

That same year I began graduate school. I had discussed my future in education with my principal and school superintendent. They suggested school administration and encouraged me to get a master's degree in educational supervision.

Starting graduate work at George Peabody College in the Fall of 1934, I happened to select a course on the psychology of the exceptional child taught by Dr. Paul L. Boynton. He was one of the great teachers I have had. Although his lectures were somewhat dull, he conducted most classes as discussion sessions. He was especially effective one-on-one, and often led you to the answer to your own question. His undergraduate training had been in physical and biological sciences. His Ph.D. was under Joseph Peterson and centered on rational learning. His major areas of interest were individual differences, psychological measurement, child psychology and learning. He had published an excellent text on intelligence and was finishing one on child psychology when I began my graduate work.

In the Spring term, I took a course from him on the psychology of learning. I was finding that psychology filled the void that philosophy had not. No longer were you completely dependent on ratiocination. You collected data, used controls, and used reliable and valid measuring instruments. From the two courses with Boynton, scientific thinking went to the top of my value system, indeed, with almost a religious quality. Since the pursuit of knowledge in psychology did not depend on prior training in physical or biological sciences, I saw psychology as a possible vocational and intellectual life. Vocationally, I was thinking of teaching and research.

Near the end of the second course with Boynton, I discussed the possibility of graduate work in psychology. He indicated it wouldn't take much to get a masters in psychology. As to work following the Ph.D., he indicated that college teaching or work in a mental health center were the most probable work settings. It is interesting to note that he did not suggest industry as a site for a psychologist nor

any training in industrial psychology. I do not remember that employment in industry was ever mentioned during my graduate work. Nor was any attention given to industrial psychology as part of the discipline of psychology. Only a few texts had been published, and very few psychologists were employed by business concerns.

After talking with my wife, we agreed that the Ph.D. in psychology was the direction to take. Boynton accepted me as a graduate student and I completed my master's degree in August, 1936.

The research for my thesis was an investigation of free word associations of elementary school children. Boynton urged me to publish my research, as he believed it was one's obligation to share his findings and to submit the research to the judgment of his peers. I published two articles on my thesis, one in 1937 and another in 1938. I became rather prolific and published 13 articles between 1937 and my entry into the Navy in 1942. Only two of those were related to industrial psychology.

My opportunity to enter college teaching came sooner than expected. I was offered a job as temporary instructor in the Psychology Department at North Carolina State. Neither Hortense nor I had heard of NC State, and thought it was the University of North Carolina at Chapel Hill. I accepted the job even though it meant resigning the job at Donelson, and might mean unemployment in June, 1936. Boynton pointed out that I could take courses at Duke or Chapel Hill to complete requirements for my Ph.D.

I had satisfied the residence requirements at Peabody, and took over half of my course work for the Ph.D. at Duke and Chapel Hill. I returned to Peabody for two summer sessions, completed my dissertation and was awarded the Ph.D. in August, 1939. My dissertation involved a national sample of 7,986 retarded children in elementary schools. I had data involving 25 variables, and this before electronic data processing equipment.

North Carolina State, Duke and Chapel Hill

My introduction to industrial psychology came at NC State in February, 1936. My teaching assignment was 18 contact hours per quarter, or six three-hour courses. Among these were two sections in industrial psychology, and three sections of introductory psychology. An instructor-sharing arrangement added a section of introductory sociology.

We used Viteles' encyclopedic *Industrial Psychology*, 1932 edition. The majority of my industrial psychology students came from the Textile School, with a scattered number from Engineering and Agriculture. The Psychology Department was strictly a service department; there were no psychology majors.

I doubt my students learned much about industrial psychology when I first taught it. I had no industrial experience and it was difficult to find illustrations from real life to illuminate Viteles' treatment of the subject. The research and literature of the field was unknown to me. I am sure I studied the class assignments harder than most of my students. I learned a lot even if my students didn't. Anyway, I got by enough to be re-employed as an instructor in 1937.

At Duke, I became aware for the first time of the controversy between schools of psychology. I had several classes with D.K. Adams who introduced me to Gestalt psychology and the topological psychology of Lewin. Lewin's field theory made a profound impression on me. It has had significant influence over time in my approach to the behavior of individuals in work organizations.

At Duke, I had two seminars with William McDougall. I was impressed with his broad erudition, his courtly manner, and his courtesy to me as an outside graduate student. I remember little of the content of the seminars, except the title of one: The Place of Instinct in Psychology. Another course at Duke with Karl Zener made a lasting impression on me. It was called the Neurological Basis of Behavior. There was, at that time, only sparse research on the relationship of behavior to neurological structure. Nevertheless, I came away from this course with a firm conviction that an understanding of the neurological basis of behavior was essential to understanding individual behavior.

At that same time, I was also taking classes at Chapel Hill. I was fortunate to have another great teacher there, John Dashiell. I had a course in experimental psychology and one in the history and theory of psychology with him. Dashiell had moved from his extreme behavioristic position (Fundamentals of Psychology, 1927) to a broader position (General Psychology, 1936). He was an excellent classroom teacher, but, as was Boynton, he was at his best in one-to-one interaction with students.

The breadth of Dashiell is illustrated by two incidents. First, in spite of his behavioristic view, he allowed me to take an independent study of Lewin and Hoppe's concept of level of aspiration. I did an experiment on the relation of level of aspiration to judgment. Second, after I became head of the Psychology Department at North Carolina State (1940), I developed a proposal for a master's degree in Industrial Psychology. It had no chance unless Dashiell would support it. When I explained it to him, he said something like this, "Bill, I know nothing about Industrial Psychology, but if you say it is a legitimate field for psychologists, I will back your proposal." He did, and the proposal was ultimately approved, even though its implementation was delayed by World War II.

Becoming an Industrial Psychologist

I completed my work for the Ph.D. in 1939 at George Peabody College. By then, I had decided on college teaching as a career, but had not developed a specific field of interest. My doctoral studies had given me more than casual acquaintance with the following areas: individual differences, measurement, motivation, learning and social psychology. Later, when I decided upon industrial psychology, I found I had an excellent background for the field except for statistics and what is now called experimental design.

Statistics learned as a graduate student were the statistics of large N's. Small sample statistics were just beginning to be used, so I gave myself a course in small sample statistics by reading Snedecor. Later, I went to Siegel for information about nonparametrics. Research methods in industrial and experimental design I learned from reading Thorndike's Personnel Research, Guion's Personnel Testing, Dunnette's Personnel Selection and Placement, Jahoda, Deutsch and Cook's Research Methods in Social Relations, and Campbell and Stanley's 1963 book, Experimental and Quasi- Experimental Designs for Research.

After receiving the Ph.D. degree, I continued teaching at North Carolina State. I was able to get rid of the sociology course by teaching a course in social psychology. I taught three sections of industrial and one of social psychology. I also began a student guidance center.

Viteles' Industrial Psychology was a difficult text for most students. In searching for something more readable, I came across Jenkin's Psychology in Business and Industry (1935). My thinking about the application of psychology to industry was heavily influenced by Jenkin's book. It is interesting to note that his text didn't have a chapter on employee training. He dismissed the topic with a footnote saying

there were not enough generalizations of known dependability concerning training to justify including the topic in his text.

Jack Jenkins was the first industrial psychologist I met personally. It was a casual meeting at the Southern Society for Philosophy and Psychology in Washington. (I eventually came to know Jack Jenkins well, as he was my commanding officer in the Navy.)

As there were very few industrial psychologists in the late 1930's, I had no continuing contact with any until after World War II. I had to rely entirely on the literature to learn about the field. I read most of the books available, both lay and scientific: Hepner, Bruce Moore, Burt, Link, Meyer and Munsterberg. Each had an influence on my thinking, but not as much as Viteles, Jenkins, and later Mayo, Roethlisberger, Hartman and Newcomb.

Many of my students had worked in industry, usually textiles. I started drawing them out about their experiences. The textile industry was by far the biggest in North Carolina, and NC State had one of the larger and more highly respected schools of textiles. I decided to learn about textiles by attending courses. The faculty made me welcome. As most had industrial experience before joining the faculty, they were kind enough to share experiences and the kinds of employee problems they encountered.

Through the Textile School faculty, I met several mill managers who also shared employee problems with me. I appeared before several industry groups to discuss tests for selecting employees. These led to some small consulting assignments on selection, adding to my knowledge of managerial concerns as to employees.

In an unsystematic way, I put the knowledge from these experiences together. It became apparent that there were more dynamic and central aspects of the behavior of individuals in organizations than those of selection and training. They centered around employee motivation, rewards, group relations and problems of authority. In my own thinking, I began to call this the social psychology of industry.

I began to devote more time to these topics in my courses. In early 1940, I was asked to teach a course in Industrial Psychology for a government sponsored management manpower development program. The program was designed to prepare personnel for the increasing number of industries manufacturing war materials. Enrollees were more mature than my college students, and the majority had industrial experience. Given my concerns about the social psychology of industry and the content of current texts, I decided I had to take a different direction.

I developed a syllabus that touched on the major areas on industrial psychology, but stressed the social psychology of industry. I drew on some of the early work of Mayo and the Hawthorne studies that were just appearing in print (Whitehill, 1937; Roethlisberger and Dickson, 1940). I used material from an as yet unpublished manuscript edited by Hartmann and Newcomb, *Industrial Conflict: A Psychological Interpretation*, (1940). I had investigations of employee attitudes by Houser (1927, 1928), Kornhauser (1938), Uhrbrock (1934), as well as Mathewson's 1931 study of restriction of output among unorganized workers.

As a framework for all this material, I used Lewin's topological psychology without the geometry. The concept of life space seemed highly acceptable to the students in this course. The here-and-now theory of the etiology of the behavior of employees helped pull them away from half-baked psychoanalytic approaches, and concentrated their attention on what happened to employees in the workplace. In

developing this framework, I found the text I used in social psychology useful: J.F. Brown's Psychology and the Social Order, (1936).

I taught this course three or four times, constantly expanding and revising it. I began to write a text on the social psychology of industry in early 1940, but my entry into the Navy terminated the project.

During this time, I wrote my first article in industrial psychology and submitted it for publication (McGehee, W and Owens, E.B., 1943). Like most of my research efforts, this one arose from a problem Owens was having at his job supervising clerical workers. Employees were taking unauthorized as well as authorized rest pauses. I suggested research as an approach. Among other things, we found the major reason employees objected to authorized rest pauses was the fact that rest room facilities were inadequate if all employees took pauses at the same time. This would not be the last obvious but overlooked problem my research would uncover.

The Navy

After Pearl Harbor, I attempted to enlist in all the military services. I had difficulty getting accepted due to my age, the injury to my hand, and my occupation. Finally, I was offered a commission in the Coast Artillery, and another in the aviation psychology section of the Navy. I accepted the latter and reported for duty in September, 1942.

My first assignment was to an instrument instructor school based in NAS Atlanta. The school was just starting and was pioneering a new teaching method of instrument flying that used a full panel of instruments and was called "attitude flying." I became involved in developing the training program and evaluating its graduates. I spent many hours flying with students and instructors, analyzing the instructor's job. The job analysis revealed that some aspects of teaching were being over-emphasized at the expense of other, more important ones. One change in the program was a course I taught on the psychology of instrument flight. It was aimed at a major problem, pilots not believing what their instruments told them.

I was involved in developing curricula for Waves who were to become Link trainer operators, for tower control personnel, and for mechanics. Even though it violated Navy regulations, for a brief time I was commanding officer of the control tower school and the Link instructor school. I did a study of instrument panel design on aviator performance and pilot fatigue. I used an old PBY5A for the study. My work was frequently interrupted because the Marine executive officer repeatedly ordered it on special trips without regard for my research schedule. In defense, I finally obtained the following order from the Chief of Naval Operations: "Lieutenant William McGehee is designated as Commanding Officer of PBY5A (73860) and it will be flown only on his cognizance." Thus, I think I became the only psychologist to become the commander of a Naval aircraft.

The Navy program for selecting flight trainees was a clear success in terms of the number of trainees who could complete training and become aviators. We wondered, however, whether it was selecting those who would become good combat pilots. To secure research evidence, we needed to confront the criterion problem. Pass rates from school were not adequate to the task, and we struggled to come up with some alternative. Chester Bennett suggested we go to fellow pilots for judgments using Moreno's nominating technique. Jack Jenkins, commanding officer of Naval Psychologists, tried it in the fleet under combat conditions and concluded that it worked.

Accordingly, several aviation psychologists were sent to the Pacific Fleet in 1944 to collect peer nominations from pilots in combat squadrons: Verne Lyon, Chester Bennett, Jack MacMillan and William McGehee. I was assigned to search, antisubmarine and air-sea rescue squadrons. Even though our work had strong endorsement from the Chief of Naval Operations, the mission required some selling on our part. Early on, I was challenged by one commander who said if I wanted to know what a good pilot in combat was like I should be on the flight line a 0400 and fly a mission with him. I did, and flew for 16 hours to Borneo, Pallawan, Mindanao and back to Morati, our base in the Netherlands East Indies. Eventually, I flew with all the squadron commanders and believe my willingness to fly with them had a significant effect on their cooperation in my research. Only four of over 100 pilots I interviewed refused to make peer nominations.

In my opinion, the technique did uncover those pilots who were exceptionally good or exceptionally poor. The procedure tapped vital and realistic experiences of combat aviators. The efficiency of a fellow pilot could make the difference between survival and being shot down. Pilots could be picturesque in describing other pilots. When asked why he did not want to fly with a particular pilot, one said, "Christ, he is so uncoordinated that he walks in a skid!" Another pointed out that he wouldn't fly with another who had shot down ten enemy aircraft because he had lost more wing men than he had shot enemy planes. Combat flying from a carrier is a team affair, not a place for individual stars.

When I attempted to use peer nominations in industry following the war, I had little confidence in the results. I believed they were based on judgments that were much less critical to the employees, compared to the stakes involved with combat pilots. Employee judgments seemed too much like a popularity contest. In any event, the combat criterion study was not completed until near the end of the war with Japan. Although the results were not available to me, I later heard that the only good predictor variable consisted of biographical inventory items.

What did my four years in the Navy mean? It came at a time when I was beginning to establish my career, and should have been the most productive of research. It didn't hasten the war's end or save lives. In spite of little professional or scientific development, I gained many things from naval service. I learned that the authority of competence could deal successfully with the authority of position. This has been helpful in my later interactions with plant managers and corporate vice presidents. I also learned that acceptance as a professional was greater if you participated in activities of individuals with whom you are working, and approached problems from their point of view.

If I made no significant contribution to the war effort, wasn't my naval service a waste of time? No. What I had to do had to be done by someone. I find today that my concept of a citizen's obligation to his country is out of date and somewhat quaint. It is my firm belief that a citizen owes his country his service and his life if it is required. This is true despite the frequent asininity of its leaders. Our country is one of the few in the world where I could write the last sentence without fear of governmental reprisal. Four years of your life is not too much to try to preserve the United States of America and its way of life.

Through my naval work, I did expand my acquaintance with psychologists of different interests. These continued after the war, to my benefit. Among those psychologists were Lowell Kelley, Jack Dunlap, Ben Underwood, George Kelley, D.N. Fiske and Bob Selover, all from the aviation psychology section. From the Army, there were Art Melton, Paul Fitts, Frank Geldard, Ed Henry, Erwin Taylor, and especially Rains

Wallace. Many of these men had a significant influence on my thinking about psychology as a science and profession.

Back to NC State

Near the end of the war, I was stationed aboard a seaplane tender at Okinawa. I returned to the States aboard this ship and was assigned to the Naval Air Station at Jacksonville, Florida. I had intended to stay in the Navy until I sorted out what I wanted to do: return to NC State, stay in the Navy, or seek industrial employment. However, I received an urgent request from NC State's chancellor that I return. The college was flooded with returning veterans, and State now had a contract to establish a vocational guidance clinic for veterans. I was released from active duty in December, 1945 and returned to NC State.

I was inundated by administrative work. The increased student load meant I had to find four additional faculty. The vocational guidance center needed 18 counselors that we would have to train ourselves. This hectic schedule continued as we tried to cope with the massive load of students and veterans.

In the Spring of 1947, I received a phone call from Ed Michael of Fieldcrest Mills in Spray, North Carolina. He asked me to send a supply of the Otis Mental Ability Test, the Bennett Mechanical Comprehension Test and the Bernreuter Personality Inventory to him. He wanted to use them to select supervisors based on conversations he had with a psychologist at Allegheny College. As he had no training in testing or statistics, I refused. I was blunt, a little rude and forgot about the call. Three weeks later, he called again asking if he might meet with me in Raleigh to talk about using tests. Even though I was swamped with administrative work, I reluctantly agreed to meet with him. I described what was necessary to validate a test, and he asked if I would undertake a study for Fieldcrest. I said I would if I could first meet with the executives to explain what would happen, their responsibility, and the likelihood of success or failure. I really wanted to gauge the enthusiasm for the project.

The meetings went well, and the top executives asked the kind of penetrating questions that made me believe they were serious. In addition, I was assured that security would be tight and that no one in the research group would be jeopardized by his test performance. I would have the test scores.

We started the study in June, 1946. It was a simple concurrent study, although it wasn't called that until Thorndike did in 1949. I had not heard of cross-validation, moderator variables, or suppressor variables. I was soon confronted with the problem of vanishing N. When I controlled for extremes in experience and eliminated supervisors whose jobs were totally different from the one under study, my N went from 80 to 55. The three tests were administered on company time, and all executives took the battery as an example for the foremen. (I am not certain which group experienced the greater tension, but I had scores on all management personnel when I later joined Fieldcrest as a full time employee.)

This neophyte failed to collect criterion data in advance, or even to decide on same. I tried a graphic scale of overall performance with disastrous results, and finally used paired comparisons in which raters compared every shift foreman with every other one under their supervision. Where possible, I got two raters for each foreman. Coefficients of agreement between raters ranged from 0.68 to 0.95, with a median of 0.81. I split the group in half based on criterion scores and obtained biserial correlations from 0.36 to 0.43, with a multiple of 0.65. (When asked years later why I never published study, I said I didn't remember, but I thought it was because the correlations were so low!!)

While management was not greatly impressed with the improvement in selection the tests would make possible, they believed that any improvement was worthwhile. Thus, I trained a member of the personnel department in test administration, scoring, and use in selecting foremen.

I was asked to continue as a consultant to validate tests for key production jobs. For mechanical jobs, I tried to use objective output data, and also tried to avoid criterion contamination we didn't call it that then by rotating worker machine assignments. That created both administrative and personnel problems, as workers liked their own machine and didn't want to work on equipment of other employees. Again, I used paired comparisons with success, but my attempt to use peer ratings as a criterion was a disaster.

I Join Fieldcrest Mills

In December, 1946, I met with H.W. Whitcomb, Manufacturing Vice President, and Macon Miller, Director of Industrial Relations. They wanted to move faster on test validation for production employee selection. Whitcomb added something like this, "Bill you and I have discussed what you call the social psychology approach to employee relations. As you know, I have not agreed with you fully. But in the future, I think management is going to have to take different approaches to employees if we are going to have efficient employees. We are going to need someone who keeps us informed about psychological approaches and to guide us in our use of them."

This was pretty new territory for a psychologist. Few were employed full time in industry and none were employed in the textile industry or in the South. It meant giving up my academic job and its security, as well as possibly losing my identity as a psychologist. I would have to abandon plans to write an industrial psychology book. On the other hand, I would gain industrial experience that would help if I returned to academia. I could learn if what I knew really worked. I would have a modest financial increase at first, but would raise the ceiling well above what was available in the academic world. After receiving assurances as to professional affiliations, research publication and job duties, I accepted the offer. When asked for a title, I suggested "Industrial Psychologist." Fieldcrest wanted one that would indicate my status in upper management. Further, they feared that hiring a psychologist might indicate to employees a concern about their sanity. Miller suggested "Director of Personnel Research." I don't where he got the idea, as there were few if any such in industry in 1947. In fact, I may have been the first psychologist with the title.

Personnel research as a distinct function in American industry grew out of World War II. Many servicemen had been exposed to psychological and behavioral science research, and took their interest in them back to their companies. About 1949, the American Management Association ran its first seminar on personnel research. I attended it, and conducted some of the subsequent seminars.

One outgrowth of that experience had an indirect, if not direct, influence on the future of industrial psychology. Several individuals who attended these seminars organized a group of personnel researchers to meet and discuss behavioral science applications and developments in their companies. This group, the Dearborn Group, was limited to representatives of 16 of the larger corporations in the country. They began their semi-annual meetings in 1950, and continue to meet today. Initially, several disciplines were represented, but the majority of members have been psychologists. With one exception, none of the members were full time academicians or consultants. The exception was Douglas MacGregor, who served as advisor and commentator for the group. I was among the first

members. Interaction with members greatly influenced my thinking about the application of psychology to industry.

It is difficult to document the influence of the Dearborn Group on industrial organizational psychology. However, MacGregor's Harvard Business Review paper, "An Uneasy Look at Performance Appraisal" (1957) could have been influenced by a full day's discussion by the Dearborn Group of problems with appraisals that were occurring in their companies. Herb Meyer, a member of the Group, was possibly influenced in his research on performance appraisals he reported on in, "Split Roles in Performance Appraisal," Harvard Business Review, (1965). MacGregor's Human Side of the Enterprise was influenced both by Maslow's thinking, and by MacGregor's interaction with the Group. It is also noteworthy that six past-presidents of Division 14, APA, were members: Ed Henry, Herbert Meyer, Doug Bray, Paul Thayer, Rains Wallace and Bill McGehee.

Validation studies of tests for use in selection of key production employees continued after I joined Fieldcrest. The question of selection and development of upper level managers never came up at this time, perhaps based on the belief that such decisions were infallible. But then, this was true in other industries and companies, too.

Stogdill's (1948) review of efforts to find traits discriminating between leaders and nonleaders suggested the futility of the effort. It was the early 1950's that saw the efforts of the Michigan Survey Research Center and the Ohio State University group to describe leadership in terms of leader behavior. Fielder's (1967) situational approach was nearly 20 years away.

It may be understandable that little work was being done on the selection of managers, as there was little agreement as to what was being sought in managerial selection. Flanagan's critical incident technique was a breakthrough for analyzing significant behaviors of managers. Hemphill's (1959, 1960) heroic attempt to develop a taxonomy of managerial behavior suggested additional techniques for studying managerial behavior. Even today, procedures for analyzing managerial jobs leave much to be desired. Part of the problem stems from concentration on one aspect of the role leadership or increment of social influence at the expense of the cognitive aspects of the role. Perhaps the failure to identify differential traits arises from considering that all managers are leaders.

It is in this context that I became involved, in the early 1950's, with problems of selecting and developing managerial employees. From time to time, members of Fieldcrest management came to me for advice in regard to their subordinate managers who were not performing to their supervisors' satisfaction. This, in turn, led to requests for recommendations with regard to promotions, and selection of candidates from outside the company for managerial jobs. In recommending candidates from outside, I used clinical assessment procedures. In responding to requests for advice, I made it clear when I was giving advice based on sound psychological data, and when the advice was based on instant wisdom.

The requests for recommendations in selecting managers increased. I believed we might be overlooking talent already employed by the company. General management agreed that a manpower audit should be made. The first audit occurred in 1961. There were two immediate consequences. We found that qualified internal candidates for shift foreman were in short supply. At least twenty new shift foremen would have to be appointed each year to maintain present staff levels and to provide for expansion. Given that a separate study had shown that foremen hired from outside had to be replaced twice as

frequently as internal appointees, we decided to develop a pre-supervisory training program. We also shifted emphasis in hiring production employees to those with foreman potential.

At the higher levels, there was also a shortage of potential replacements. It was decided to step up college recruiting and to develop a management training program for college graduates. These programs became my responsibility. I did the college recruiting for the company, and monitored the training program until 1969. At that point, I secured an assistant with responsibility for executive and college recruiting. I continued to be involved in management training. I must admit that I did little to formally evaluate the recruiting and training programs. When I retired, however, I noted that ten of the fourteen vice presidents had come through the programs.

As time passed, managers called on me more and more to not only give advice concerning individuals, but to advise on general organizational problems and behavioral science approaches to managing the human resources of then company. Much of my time in the last years of my work at Fieldcrest was spent in these activities. I also had administrative duties, including supervision of a large staff responsible for salary administration, job analyses, production employee training, executive recruiting, and related issues. I became less a psychologist and more a manager.

In the late 1950's and 1960's, consultants "crawled out of the woodwork" with panaceas for human resource problems. The panaceas were allegedly based on behavioral science research, even though the consultants were usually not behavioral scientists. Fran Tarkington, the Minnesota quarterback, was peddling a behavior modification program. A former industrial engineer was hawking sensitivity training, while another was selling "rational training," and a minister offered transcendental meditation at five hundred dollars a session.

Managers are often suckers for personnel panaceas. Perhaps this reflects their unwillingness to spend the necessary time and effort in effective personnel administration. Since consultants had to sell their panaceas to me before the company would buy, I saved Fieldcrest a substantial amount of money by not buying. I don't condemn all consultants, as many offer worthwhile services. From time to time, for example, the American Institutes for Research and the Psychological Corporation have provided valuable services to Fieldcrest.

One of my internal consulting services created something of an ethical problem for me, the conflict between responsibility to the individual versus responsibility to the company. Individuals came to me from time to time to discuss personal job problems. It was understood throughout the company that this was an acceptable practice, and that discussions about personal problems would be confidential. I never experienced any pressure to reveal these confidences.

On the other hand, I might discuss a personal problem with someone, and then months later be asked to make a recommendation on a personnel decision involving that person. While I could not reveal a confidence, that confidence might influence my recommendation. I attempted to handle this by telling anyone who wanted to discuss a problem that I would not reveal any confidence, but that they might affect any recommendation I had to make in the future. I then left it to the individual as to whether or not to continue the discussion. I am not sure this settled the ethical problem of the psychologist's obligations to the individual and the corporation. My presidential address to Division 14, "Esau Was An Hairy Man," dealt with this problem.

I did not adopt the role of clinical psychologist in these sessions, and made no attempt at psychotherapy. If I thought deep emotional stress was involved, I recommended help from qualified professionals, as well as assistance in locating same.

Training at Fieldcrest Mills

Let me turn back to the beginning of my time at Fieldcrest. A few months after I joined the company, I was asked to assume responsibility for all training. That was agreeable to me, as I had intensive exposure to learning theory in graduate school, and found the work on training Navy pilots to be instrument instructors both challenging and interesting. Further, I learned that Fieldcrest's training activities needed substantial improvement. (I learned later that the status of training at Fieldcrest was not atypical of training in American industry.)

The responsibility for training production employees was the shift foreman's. Training usually consisted of showing the employee how to do the job, and checking back occasionally to see if he had caught on. For jobs requiring long learning times, the new employee was assigned to an older employee for training. The trainee usually received little instruction as teaching interfered with the trainer's own production, resulting in a loss of pay. Thus, a new employee not only had to learn how to perform the job, but also had to learn how to learn.

There was a minimum of formal management training, usually restricted to shift and department foremen. Such training consisted of courses in how to deal with employees along the lines of the "human relations" approach, a product of the Hawthorne studies. There were also courses in work simplification and safety. Many foremen had had a course in Job Instructor Training during the war, but its effect was not apparent in their training efforts with production employees.

Interestingly, all supervisory training was carried out on the employees' time. All supervisors had to participate, regardless of the need for training. No effort was made to evaluate training results.

There were companies that ran more sophisticated training programs, but they were in a minority. As part of taking over training for Fieldcrest, I visited about 25 well known companies to discover how they trained. In general, training for production employees was informal and rarely organized. The management training I found involved following fads and fashions. There was little effort to determine the organization's training needs, or the needs of individuals. There were no adequate attempts at evaluation of training. Managers seemed to regard training as a necessary evil rather than a means to achieve company objectives.

A Research Approach to Training

I found no suitable model on which to build the training program at Fieldcrest. Therefore, I developed my own. It involved an initial determination of organizational training needs, and the most effective way of meeting them. It included evaluating training in terms of the effectiveness in meeting organizational goals. I called it the research approach to training.

My first public statement of the model was at the annual meeting of the personnel division of the AMA, later published in the Personnel Series of AMA (1948). In subsequent years, I presented this approach in several publications.

The research approach to training was presented to general management at Fieldcrest in late 1947. It was foreign to their ways of thinking about training. It was accepted with little enthusiasm, but with a strong statement of support from H.W. Whitcomb. In implementing the model, I encountered no serious opposition, and only a rare bit of foot-dragging. In retrospect, I am amazed at its acceptance and the lack of serious opposition, as it departed radically from managers' and supervisors' ideas of what training was. It also contained some threats to their concepts of their own expertise. And for supervisors, it meant giving more time and effort to training than they had in the past. Whitcomb's support was crucial for initial acceptance. An early demonstration of the efficiency of the approach in reducing training time for two important jobs, and increasing production through retraining in a couple of other instances convinced many of the "Doubting Thomases" about the efficiency of the approach.

At the beginning, we were confronted with a horrendous task. James Gardner joined my staff as an assistant in June, 1947. He had just completed his masters' degree in industrial psychology with Jack Jenkins at Maryland. He became heavily involved in implementing the model. He made many suggestions that made the research approach more effective. I regret that Jim didn't continue to the Ph.D., as I think his research would have made significant contributions to industrial psychology.

The company had 925 different production jobs, some with as few as three employees, and others with a hundred or more. We consulted the managers to determine priorities for developing training programs for the various jobs. Our top priority included jobs which, in terms of organizational indices, were not being performed satisfactorily. The other high priorities were for jobs requiring the highest skills and unusually long training times, and those involving the largest number of employees. Once plans were completed for these three categories, there were only about fifty jobs left requiring plans.

The first priority led us into retraining, and that led us into identifying employees whose job performance was below standard. Once an employee was identified, an analysis of his specific training needs was required, and that necessitated a thorough job analysis. I found little guidance in the literature for carrying out job analysis. Viteles' job psychographs and the procedures used by the U.S. Employment Service were of little help, since they were primarily developed for use in selection. Job descriptions based on time studies lacked the necessary detail for training purposes. Jim and I had to devise our own procedures.

The collection of job analysis data led to the most serious foot-dragging by department and shift foremen. As they were our primary source of information, providing it required additional work on their part. What job analyses were available would now be called job centered. As these did not provide data needed for training, we turned to analyzing jobs in terms of required behaviors of employees. We collected data on required activities, perceptual cues, knowledge requirements and the kinds of decisions required.

All the training plans called for the use of trained instructors. Again, we encountered problems as supervisors were reluctant to release efficient employees, even if that would ultimately reduce production problems. Many experienced employees did not want to serve as instructors, as there was no additional compensation for instructing; sometimes it resulted in loss of pay, especially on a piece rate job. These problems were solved in part by getting approval of two new corporate procedures: (1) a department would receive training cost credits on their manpower budget only when trainees were trained by an instructor approved by the Training Department; (2) employees serving as instructors were guaranteed an hourly wage fifteen percent above their average earnings.

One fundamental aspect of a research approach to training is the evaluation of training results. Our efforts here were not outstanding. We didn't ignore the problem, but our research designs left much to be desired. Sometimes evaluations were simply a matter of counting; were enough adequately trained employees furnished the department from the training program?

We also compared data as to training time under the new system compared to time under the old. Obviously, such case studies have many threats to internal validity. We would point out these threats to supervisors, but they continued to be impressed when training time for new employees was reduced by 25 to 50 percent, or when waste was reduced as much as 40 percent (McGehee and Livingstone, *Personnel Psychology*, 1952 and 1954).

It took several years to install this training approach in the company. Perhaps one major conclusion that can be drawn is that no matter how poorly organized the training may be, it gets better results than unorganized training. Many training programs installed by consultants seem to have positive results because they are compared to a situation in which there had been no prior organized training.

As to management training, priority was given to the first two levels, department and shift foremen. The first step again was an analysis of the two jobs. Flanagan's critical incident method was used, along with time sampling of supervisory behavior. These analyses revealed that the jobs required primary skills in addition to human relation skills. There were heavy demands for thinking and cognitive skills because of the many managerial controls supervisors had to use on the job. A program of short courses was developed for these skills, while outside resources were used for technical training. Fortunately, a supervisor was enrolled in any course only when it was clear that the individual needed training, a large departure from previous practice.

Our concern for evaluation continued. One approach involved a consumer satisfaction study to find out if managers were getting the number of qualified employees required, and were the trainees receiving the kind of training they thought they needed and could use on the job. Accepting the weakness of such evaluations, consumers seemed satisfied with our work, and occasionally made suggestions to strengthen the supervisory training program.

Occasionally, we were able to use more sophisticated methods, as when Gardner and I used a Solomon four group design to evaluate training of supervisors in time study skills and knowledge (McGehee and Gardner, *Personnel Psychology*, 1955). We learned that with a little planning and foresight, experimental designs could be used in field studies of training. That was especially true if all members of a job classification participated in the training.

Serious efforts to train and develop upper level managers did not occur until the late 1950's. These efforts were closely tied to performance appraisals. The initial work was to improve managers' skills in coaching subordinates. We were unable to do an adequate evaluation of this in-house program. We also sent managers to programs provided by universities in the area, or to AMA programs when a need was indicated.

Perhaps because I was one of the few psychologists with a major responsibility for training, and because I had a few publications on the subject, I was asked frequently to participate in conferences and seminars by both national and local organizations. I have not kept records of such meetings, but I know I participated in programs on training sponsored by the American Society of Training Directors, the

Conference Board, the American Management Association, and the American Psychological Association. I also served on boards or commissions concerned with training and training research for the Department of Defense and US Labor Department, among others. These experiences added to my conviction that training in American industry was being carried out inefficiently. I was, therefore, much interested in collaborating with Paul Thayer to write a book on training. It was an opportunity to protest the misuse of training, and to suggest a research approach to the subject.

Our book, *Training in Business and Industry*, came from another fortuitous incident. At an annual meeting of the APA (I think it was 1957), Thayer introduced me to an editor who was seeking someone to write a text on industrial psychology. I didn't feel capable of doing that and told the editor so. I indicated, however, that I would like to write a book on training if Paul would co-author it. Paul reluctantly agreed, and the book was published in 1961 by John Wiley & Son.

During the writing of the book, Paul was in Hartford, Connecticut, and I was in Eden, North Carolina. We saw each other only at the annual APA convention. All other interactions were by correspondence. We exchanged chapters each had agreed to write, revised them and wrote about the revisions. The last involved protests of changes made in draft chapters, and reasons for retaining the original. Paul kept copies of the correspondence and told me that the protest letters were greater in magnitude than the final manuscript. The repeated rewrites resulted in a book that is truly the product of both of us. It became difficult to say who wrote what. Remarkably, Paul and I remained friends throughout the process.

The text sold fairly well, and provided enough royalty for several bottles of whiskey a year. It was well received by psychologists both here and abroad, and was translated into Spanish and Farsi. I think it was less well received by professional trainers. At least 15 years later, when I was preparing a chapter on "Training and Development: Theory, Policies and Practices" for Volume V of the ASPA Research and Industrial Relations Handbook, I found little change in industrial training practice along the lines suggested by our training book.

Since I had retired when I wrote that ASPA chapter, I assumed it would be my final statement on industrial training. However, Paul and I had a note in *Personnel Psychology* in 1977. As I am writing this (February, 1978), I have an article on training in press (McGehee and Tullar, *Personnel Psychology*, 1978), and am considering collaboration on another text on industrial training.

After publication of our book, I conducted little or no research on training. My administrative duties increased, including all salary administration and market research. I was also becoming more involved in the affairs of APA. As Treasurer of APA for six years, I spent approximately thirty days a year on APA business.

New Directions

I also found my interests shifting to aspects of employee behavior I considered more central and dynamic than selection and training. The work of Likert and his group on employee attitudes revived my interest in employee motivation and job satisfaction. From the early 1950's onward I had conducted sporadic employee attitude surveys. Organizational behavior was emerging as a discipline, and I tried to keep up with developments through the literature and through the Dearborn Group.

The social changes in the 1960's focused the attention of both psychologists and managers on the behavior of individuals in organizations. Docile and willing work forces were disappearing; turnover and absenteeism increased; work quality declined; good employees were harder to recruit. Title VII of the Civil Rights Act substantially changed the personnel procedures of many companies. Vietnam was challenging governmental wisdom. Big business and big government became the big villains. Democracy was good, and bureaucracy was bad,

Fieldcrest, like other companies, struggled to cope with all these threats to organizational survival. Consultants of all persuasions descended on American industry with a host of remedies for employee alienation and declines in work quality and quantity. As with selection earlier, research caveats were ignored.

As managers had neither the time nor the expertise to evaluate these panaceas, it was my job to do so. One manager was confronted by increased absenteeism in his plants. Many of the jobs appeared monotonous and boring, and an outside consultant recommended job enrichment to reduce absenteeism. When asked for my opinion, I collected information in a quick attitude survey and found employees liked their jobs and experienced little monotony. Further, the absenteeism increase was the result of a change in work scheduling. Another investigation of pay dissatisfaction showed that the amount of pay was not the problem. There was a lack of understanding of the pay administration system and concern over pay secrecy.

From 1960 on, I conducted about 35 attitude surveys at Fieldcrest, mostly on an ad hoc basis at the request of managers. My role included designing the study and the attitude instruments, and interpreting the results to managers, and helping them report results to their subordinates. As a rule, survey results went first to the immediate supervisor and employees, and then by steps up the chain of command. Only once have I violated this procedure. Early in my employment, I asked a woman employee if there was anything she disliked about working at Fieldcrest. After some hesitation, she replied, "Dr. McGehee, I'm a little embarrassed to mention this, but there is only one toilet in the rest room for 70 women on this floor. In addition, the toilet seat is cracked. Every time I sit on it, it pinches me, and I say, 'God damn Fieldcrest Mills.'" I sent this comment directly to the president of the company. Within a couple of days, there was a crew redoing the restroom and putting in several toilets without cracked seats.

One of the advantages, and possibly disadvantages, of being a psychologist in a small organization is the opportunity it offers to utilize psychology in several aspects of the business. For example, I became involved in marketing research and human factors engineering. In 1953, I was asked to conduct consumer research on a new product. I employed Paul Thayer for that summer, while he was finishing his work at Ohio State for his Ph.D. Paul had full responsibility for the investigation and my contribution was getting hurdles out of his way. The results were well received by Marketing. He also did a quick study of hosiery packaging that was useful. The following year I did several small studies, including one on forecasting sales of specific products. By simulating the purchasing decision, I came within a few percentage points of actual results when the products went on line. I must confess, however, that my relations with Marketing went up and down over the years.

As to human factors engineering, I did a study of the design of tables for inspecting cloth. Cloth to be inspected was pulled mechanically across the table surface. It was believed that several table constriction factors affected the accuracy of the inspectors in finding fabric faults: speed of cloth

movement, angle of the table top, direction of movement, and others. I developed a factorial design to identify factors affecting inspector performance. As a non-profit research firm supported by the textile industry was interested in the same question, I turned over the design and served as a consultant on the project.

Another of my investigations had machines as subjects rather than humans. It was a study an engineer probably should have done, and, in fact, the Vice President of Manufacturing told me as much following the study's completion. A mill manager was interested in the maximum speeds for machines that would not result in inferior quality. After determining the other factors affecting quality, I designed another factorial experiment. Not wanting to risk all 100 machines in the plant, we selected 10 at random. We found we could increase speeds without affecting quality so that they would produce 25% more. The Vice President refused a request to speed up all the machines on the basis of too small a sample. We repeated the study with 50 machines with the same results. All machine speeds were increased. We later learned that a university textile school ran a similar investigation with comparable results. Perhaps the moral here is that machines treated kindly are as good experimental subjects as people.

Title VII of the Civil Rights Act of 1964 had a profound effect on personnel practices at Fieldcrest, as it did in most companies. There were no confrontations, as Fieldcrest had already integrated white and black workers. The communities in which Fieldcrest had plants had no "hard core unemployed ghettos" often found in large cities. Further, President Whitcomb made it clear that the provisions of Title VII would be obeyed both in spirit and letter not only because the provisions were the law, but because they were right. He indicated that anyone who couldn't live with integration at Fieldcrest should get other employment.

The major problem created by Title VII was the demand for mountains of paper work. We had to add staff for this purpose. In anticipation of problems at the supervisory level, we examined the programs of several consultants to improve supervision of minorities. There was no program I examined that could produce reliable evidence that they improved the integration process. Accordingly, in our supervisory training programs, we simply re-emphasized that sound supervisory practices worked with both white and black employees.

Fieldcrest began to upgrade both minorities and women to supervisory and managerial jobs. Under Title VII, we were not able to use a valuable tool for upgrading, psychological tests. The validation of tests we were using was done nearly 15 years earlier, and I made the mistake of not updating the validations. We discontinued the use of tests in employment and promotions until we could revalidate them. By the time I retired, we had revalidated tests for several jobs.

Retirement

In 1973, I reached the mandatory retirement age, 65. I was appointed a Visiting Excellence Professor of Business and Psychology at the University of North Carolina at Greensboro. This gave me an opportunity and reason to review industrial and organizational research. My review did little to increase my confidence in many generalizations about managing human resources of organizations. Participative management, consideration, job enrichment or laboratory training hang on thin reeds. It is also clear to me that the trend of subordinating the individual to the organization is wrong. Writers seem to anthropomorphize organizations without realizing that individuals, not organizations, plan, maximize, decide, etc.

I agree with Paul Thayer that organizational variables and individual variables must be investigated if we are to understand the behavior of the organization's member. In his recent presidential address to Division 14, he put it succinctly: "I hope to remind those who emphasize organizational variables at the expense of individual variables, and those who emphasize individual variables at the expense of organizational variables that neither can make much progress." (Thayer, P.W., *Personnel Psychology*, 1977.) The next generation of psychologists will have to face the problems of meshing organizational and individual variables.

Voltaire wrote in his autobiography, "Although I think nothing is more insipid than details of infancy and time spent in school, yet it ought to be mentioned." For me, these details are basic to understanding my *apologia pro vita sua*. I probably should have found an area of psychology and mined it thoroughly with research, thereby contributing more to our knowledge of human behavior. Perhaps I should have been a scientist instead of a professional. But I had a hell of a good time being a professional with scientific leanings.