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SIOP President 1976-77

Several of the autobiographers in this series have commented on how chance helped shape their careers. Decisions made for one reason turned out to have unexpected results years later. I share that experience. I didn't know what industrial psychology was when I started to college and almost went into consumer psychology instead. The move of a professor prevented that, and the chance to take a couple of summer jobs sealed the decision to be an I-O psychologist. There are many examples here, and in other biographies, that raise questions about how far one can go in planning ones career and choosing ones destiny.

Childhood

I was born on July 18, 1927 in Birmingham, Alabama, the fifth son of Hugh and Elizabeth Thayer. Dad worked for DuPont and was transferred constantly, so that only two of the total of seven children were born in the same location. Although I lived in Birmingham only about 6 months, my mother informed me that she took me to the airport and held me up so that I could see Lucky Lindy as he toured the area following his transatlantic solo flight earlier in the year.

We moved to Woodbury, New Jersey where the first daughter was born, and then to Richmond, Virginia, where the second was born and where I started my education in nursery school. Then to Buffalo, New York, where we lived a record 9 years and I went to elementary school. We were fortunate not to have been hit too severely by the depression, and I was able to start playing golf at age 8, winning the age 9-12 championship at age 9 and again at age 11. My dad was assistant plant manager at the Niagara Falls plant that produced the first artificial sponges and nylon stockings. My mother was directly involved in product testing both of these, and I remember her complaining that the nylons never seemed to wear out, even after daily wear and nightly washing.

Dad was finally transferred to the home office in Wilmington, Delaware in 1939. As Delaware had an income tax and Pennsylvania didn't, he joined many other DuPont executives who lived just across the line in towns such as Swarthmore, Wallingford, and so forth. (Delaware finally figured out a way to tax those incomes, but not until after World War II.) We lived in Swarthmore and I went to Swarthmore High School where I learned to hate Latin, chemistry, and physics, in part because I wasn't exposed to general science in eighth grade in Buffalo and always felt behind, an unusual experience for someone who was skipped twice in the Buffalo schools. It was that experience, however, that led me to reject my father's and older brother's choice of some sort of engineering for a career. I liked high school, played first chair solo trumpet in the band, was production manager for the school magazine, and enjoyed classes in German and math. (I really wasn't a good trumpet player, as I hated to practice. I found out how bad I was when I tried out for the Penn State Blue Band and received a don't call us, we'll call you rejection.)

College

While a junior in 1942-43, I discovered a special wartime program at Penn State whereby you could enter at the end of your first semester as a high school senior; you would receive your high school diploma if your first semester average at Penn State was "C" or better. That sounded exciting, so I entered Penn State at age 16 as a business major in January 1944. I got better than a "C" average, and graduated from Swarthmore High on June 6, 1944, D-Day. (You've heard speakers say they were throwing away their prepared remarks. Our commencement speaker really did!) I crammed four semesters at Penn State into a year and a half, switching to psychology in my second semester. (I was also elected freshman class president.)

I really had no idea what I wanted to be, but a friend of my father at DuPont told me about personnel work, and psychology looked like an appropriate major. I did know that I didn't want to be a chemical or electrical engineer like my father and brothers. During my sophomore year, I became aware of the approach of my eighteenth birthday in July 1945. As my two older brothers (two had died years before) were both officers in the Navy, I had to be one, too. All the officer training programs were filled or discontinued, except one, the U.S. Merchant Marine Cadet Corps. There, one could get ensign's commissions in the Navy and Maritime Service after 18 months of basic training and service aboard ship either as a deck midshipman, leading to a mate's license, or as an engineer midshipman, leading to an engineer's license. Despite all my earlier resolves concerning engineering, I confronted myself with the fact that any postwar personnel work would probably involve extensive interaction with manufacturing personnel, most likely engineers. I would enhance my credibility with them and be able to speak their language if I took the engineering track in the Cadet Corps. I didn't know it then, but that turned out to be a smart decision.

I entered basic training in San Mateo, California in June 1945, was a cadet officer in charge of a platoon, got busted for smoking in the wrong place, took advantage of my trumpet playing ability and became bugler so that I could regain the extra liberty I lost when I was busted, got 24-hour liberty on VJ Day and joined the throngs in San Francisco. I later shipped out on a Victory ship taking potatoes, Quonset huts, and blankets to Tokyo and Nagoya, Japan. From there we went to Calcutta, where we picked up bombs to bring back to the States, landing in Charleston, South Carolina, completing my global circumnavigation when I went home on leave. I shipped out again from New Orleans to Marseilles, and returned thinking I had about 6 months to go to get my commissions. I was then informed that I was to report to the U.S. Merchant Marine Academy (USMMA) at Kings Point, Long Island. The program was now a regular 4 year one, and I had 2 years to go. Being a compulsive finisher of what I start, I decided to do so.

By this time, engineering subjects were fairly easy for me (maturation?), and I enjoyed my classmates. I formed a barbershop quartet and a glee club. The former earned a lot of free beers in the family taverns of the time on Long Island, and the latter was featured on a 13-week Academy national radio show on the Mutual Network. (The student producer-director of that show was Hartford Gunn, later the innovative head of the public television station in Boston, WGBH.) I also had a history teacher and English literature teacher who opened my eyes to the excitement of those subjects. The latter turned me into a voracious reader. Upon graduation, I got a job as junior third assistant engineer on a Lykes Brothers ship that went to Helsinki. I took my graduation present with me, a copy of Thomas Mann's *Magic Mountain*. I planned to sail for 3 or 4 years and save some money for college. Unfortunately, our maintenance engineer wasn't

very good, the temperature in the engine room never went below 115 degrees, and I finished the trip with heat exhaustion. Ordered to stay on the beach for 6 months, I decided to return to Penn State.

I had the good fortune of taking industrial psychology from Kinsley Smith, test construction from Al Kurtz, and various courses in consumer psychology from Lester Guest. Les was my first mentor. He let me sit in on all his graduate courses in polling, scaling, sampling, and so on, and put me through all phases of polling as a worker in his own consulting firm. By the time I was graduated, I had built sampling frames; written and pretested questionnaires; interviewed, trained and supervised interviewers; done data tabulation; and drafted reports of at least three consumer studies. It was Les who convinced me to go on to graduate school and helped me decide which ones to apply to.

I took advantage of my work with him and Al Kurtz during the summers of 1949 and 1950, working with Ralph Bender in the Personnel Research office at DuPont in Wilmington. I analyzed some job attitude surveys and a validation study for the selection of lab assistants at one of the experimental stations nearby. (As an aside, I received another bachelor's degree from USMMA a week after receiving the one from Penn State. It had been accredited in the interim, and my class was the first to earn degrees. Thus the 1950 dates for the two degrees on my vita is not a mistake.)

Graduate School

I applied to Minnesota, Ohio State, Wisconsin (I don't know why now), Purdue, and one other, and was accepted by all but Minnesota. Ohio State offered an assistantship. (I'm fairly sure I was admitted because Paul Fitts thought I would become an engineering psychologist, given my background. My earlier decision paid off in an unexpected way.) I went there fully expecting to pursue my interest in attitudes and related methodological issues with Donald T. Campbell, but he had left for Northwestern. I also wanted to take courses with Harold Burtt, but I was his TA and graded his exams, so I couldn't. I took only one course from Fitts and renewed my disinterest in engineering-related issues. I took statistics from Bob Wherry and Herbert Toops, a course in leadership from John Hemphill, and one on occupational information from Cal Shartle. I spent much of my time taking various experimental courses with Delos Wickens and every course offered by Don Meyer, a physiological and comparative psychologist and one of the best and toughest teachers I've ever known.

Toward the middle of my first year, I began to reconsider my choice of Ohio State because I really felt that it was the wrong place for me without Campbell. Again, Les helped think that through. In addition, I met and married another graduate student, Bjorg Skoklefald, a Norwegian exchange student who had gone to Western College for Women in Miami, Ohio, and who had worked with John and Bea Lacey at Fels Research Institute. It was the strong experimental background I received at State that made it possible for me to later join Bill McGehee in writing *Training in Business and Industry* (McGehee and Thayer, 1961).

I got through my course work in good time, Bjorg and I taught the two introductory courses, we cleaned the rat lab over vacations when others were gone, and designed and ran a few rat studies

of our own. I also spent my first summer as an RA at the Personnel Research Board where I had the good fortune of having factor analysis explained to me by another graduate student, Ben Weiner.

Our son, Scott, was born in 1953. Bjorg and I taught courses back to back in the same classroom and would hand him off there. Years later, at an APA convention, I met someone who was then a graduate student at Ohio State. When he learned when we were there, he asked if I knew that nutty couple who carted their kid all over campus. I informed him that I did, indeed! (How times have changed!)

In spring 1953, Burtt asked me if I would be interested in going to North Carolina to work with someone named Bill McGehee. (Who?) Bill wanted to set up a consumer panel and needed someone (cheap) who could set it up and run the first study. We negotiated a little for more money, as we had a son to care for. (Burtt objected that I ought to pay Bill for the privilege of working with him. He was right, of course, but one must eat.) Thats how Bill and I got together and how I came to know my second mentor.

The summer in Leaksville, Spray, and Draper, North Carolina with Bill and Hortense was an exciting one. Bill got full value for his dollar. I set up the panel and ran the first study of consumer acceptance of Fieldcrest's first fitted sheets. In my spare time, I did a packaging study necessitated by the change from clerk to self-service hosiery selling. I also didn't run the formal training course that Bill and I wrote about a few decades later (Thayer and McGehee, 1977).

That same summer, I developed my first dissertation proposal, a consumer study to be done at Fieldcrest Mills. Bill helped get approval from Marshall Fields, the owners. Unfortunately, the company was sold just as I was about to start, and the new owners had no interest in supporting it. Next, I almost had approval of a study of the role of corporate executive wives in a large insurance company, but the wife of the vice-chairman killed that. A motivation study involving rats was dropped on the advice of Wickens, as it was based entirely on my own thinking and hypotheses. If the first study yielded no results, I would have to redesign and do a few more to demonstrate that the results were not merely design artifacts. As I had a job at the University of Pennsylvania in September 1954, 1 couldn't afford the lengthy delay of doing study after study.

I finally returned to one of original interests, attitude change. Having kept up on the literature, I wrote my proposal in 48 hours, got approval, and started collecting data from undergraduates a few weeks later. When my original plans for analyses proved faulty, Bob Wherry came to the rescue as he did for so many of us. Anxiety levels dropped, the psychosomatic palm rash disappeared, and I received my degree in August 1954. (As I came back down the aisle at commencement, my one and half year-old son tackled me around the knees and almost sent me tumbling, to the amusement of all.)

The Career Begins

I was really looking forward to teaching at Penn. During my 4 years at Ohio State, I had been socialized and convinced that working in industry was a poor second choice if I could get a good teaching job. Here I was at an Ivy League school in the same department with Morris Viteles.

Unfortunately, I was given a very heavy teaching load (12 different courses in 2 years, including evening classes for which I got a supplement to my $3,800 salary). Viteles was also very busy with his consulting and we rarely saw each other. I finally expressed my disappointment to him, and he responded by setting aside an hour or so a week for discussions of current industrial psychology issues. That worked for a short while, until the outside pressures forced him to cut back. Even though it didn't work, I still appreciate that generous gesture.

Desperate for money and some research I could use to ensure tenure, I began doing work with Eliot Danzig, director of the Institute for Research in Human Relations, a nonprofit group in Philadelphia, now defunct. I did aviator night vision studies for the Navy and a disaster study following Hurricane Diane, sponsored by NAS/NRC.

I also developed a relationship with the curator of small mammals at the Philadelphia Zoo because of my knowledge of Phil Ash's paper on the sensory capacities of infrahuman mammals, a paper he did while a graduate student at Penn State (Ash, 1951). (We I-O psychologists have varied backgrounds.) Being able to answer drunken calls to settle bets about cats vision and dogs olfaction was a boon to the curator. He loaned his only armadillo to me, and my RA, Betty vanLaer and I demonstrated that an armadillo could acquire a visual discrimination about as fast as a rat. As an armadillo usually begets identical quadruplets, we thought it might be a useful experimental animal. Unfortunately, the professor in charge of reviewing young faculty performance failed to listen to my explanation and wondered aloud to other faculty whether I had the appropriate training for research in genetics.

That, plus starvation wages and an onerous teaching load that interfered with some research I wanted to do, encouraged me to consider a new job. (The only relief I got from my teaching was the help I received from my TA, a young graduate student named Florence Denmark, later APA president.) My first-year roommate at Ohio State, Rod Bare, was then in charge of the Placement Service at APA. I wrote describing my ideal job: an opportunity to do research on training in a realistic setting, with no teaching duties and the support system for my work. I had no desire to spend my life writing proposals. He wrote back that there were no such jobs. But, in March, 1956, he urged me to go to EPA in Atlantic City, as my job was listed there. I met Joe Weitz, was enthusiastic about the opportunity, went to Hartford, met Rains Wallace and returned to our home in suburban Narberth. As I opened the door, Bjorg took one look at my face and asked when we were moving to Hartford.

LIAMA/LIMRA Days

Before he moved to RBH and then NYU, Joe Weitz was my mentor at the Life Insurance Agency Management Association (LIAMA). He was a green thumb researcher who could simplify a cumbersome design and make it work better than anyone I know. When he left, I took his job, jumping over the heads of several people, including Leonard Ferguson. (Len left a year or so later to edit the Murchison journals, and finally went to Ohio University.)

Of course, it was a privilege to work with Rains. He was an imaginative boss who could communicate the meaning of research to our member companies in a fashion never duplicated. He taught me much about selling new studies and the results of completed ones. He helped an arrogant academic learn that a lot of executives were smarter than he and that their special knowledge and skills were worthy of respect. And, he was instrumental in getting me involved in APA and SIOP affairs. Between McGehee and Wallace, I had little choice.

My 21 years at LIAMA (later LIMRA, the Life Insurance Marketing and Research Association) were good ones. I had the chance to do some exciting research on training, work with Bob Nuckols, a brilliant consumer researcher and later head of research, learn more about research design and statistics from John Antoinetti, hire people like Mike Gordon and Larry James and then send them back to academe, teach in the many management schools put on for the industry around the world, work on industry task forces that really were concerned with serving the public better, and work my way up to vice president research when Rains went to AIR, and later to senior vice president.

During that time, the insurance industry went through many changes. Selling became marketing, market research became marketing research, life companies became multiple line companies and then full financial services companies. Companies that would sell only through their own career agents began to sell through brokers, and group insurance became a respectable product. These changes were accompanied by much furor, and friends in the business often became enemies over them.

Companies owned by minorities were admitted to membership, and with prodding from equal employment state and federal agencies, companies hired minorities as agents. As women got little attention from these agencies, they were of particular interest to me. I spent many hours working with the officers of the Women Leaders Roundtable developing data and arguments for increased utilization of women in the sales forces. Success was slow, and in a study I did for a major company in the early 1980s, I still heard the standard MCP arguments against hiring them: We train in the evening. I can't be seen having a cup of coffee with some young woman. Or, The buying public won't accept women as people who know something about money and insurance. (That was partially true, as it was easy for women to get appointments but tough to establish credibility with some male clients. Male agents, on the other hand, had a tough time getting appointments.) Or, A man will see the woman agent and his wife as ganging up on him. Today, many companies have leading agents who are women, but there are still battles to be won. Minorities, too, have overcome some of the barriers, but have a way to go.

During this period, I became heavily involved with research on biodata items as predictors of success as agents. While we continued to rely heavily on raw empiricism, we developed some rational bases for scoring, too. Don Peterson originated the development of keys for applicants of

different ages. Ed Sweeney worked with Lyle Schoenfeld with the clustering programs available at Georgia to see if it would be possible to identify types of applicants based on large numbers of items. Although this initial effort did not pay off, there may still be some utility in the approach.

Working with Don Peterson, we also began to look for moderators of validity relationships across companies. Specifically, we looked for indicators of management quality, trying to test the hypothesis that companies with good sales managers would be able to demonstrate higher validity of our biodata selection device than those with poor managers. We were able to demonstrate this in pilot studies, but could not replicate them because of the unreliability of our management quality measures. Since then, Steve Brown has done research in support of the hypothesis (Brown, 1981).

Gene Mayfield and I started a program of research on the selection interview, and were later joined by Bob Carlson. That program resulted in better interview guides, decision forms, and interviewer training (Carlson, Thayer, Mayfield and Peterson, 1971).

Rains Wallace, Don Peterson, Ray Dry, Bob Carlson, Gene Mayfield and I were all involved in the development of the Career Analysis Procedures (LIAMA, 1968), an intensive career guidance system designed to assist companies in directing experienced agents into a sales or sales management career. Agents were tested for their insurance knowledge and ability to analyze agent records, and were then interviewed by four different home office executives concerning their background, experience as an agent, knowledge of the sales management job, and their analysis and recommendations stemming from their review of some agent records. Based on this information, the executive team was to recommend the most appropriate career path, sales or sales management, and then tailor a development program for the individual. Unfortunately, the hunger for sales management was so great that many companies used the procedures as a management selection device and paid little heed to the training and development aspects of the program.

With Phil Welsh and John Antoinetti, I became involved in some of the early research on programmed learning (Welsh, Antoinetti and Thayer, 1965). For those who dont think you can use good experimental designs in the field, we used a 3 x 3 x 2 factorial design with random assignment of subjects, replicated in eight companies by mail!

Those were interesting times, and I might have finished my career there, but for a series of events which led to my departure. I was vice president of research after Rains left, as noted earlier. I reported to the president and the senior vice president who were close friends. In the early 1970s, the senior vice president died suddenly, even though he was in his mid-40s. The president asked me to move up to senior vice president. I was reluctant to do so, as the position seemed meaningless. At the same time, I recognized that a move would open up opportunities for younger people under me in the research division. I finally negotiated a deal whereby I would become operating officer internally, while the president capitalized on his strengths developing better relations with our member companies. Unfortunately, he couldn't let go, and I spent the next several years doing work of little interest. When he retired, a new president came in, and it became clear that he agreed with my assessment of the job I had. As he wanted to flatten the

organizational structure and was about to undo a number of things I had established, we agreed that I would have plenty of time to find a new job.

Although it was scary to go on the market after 21 years, and I regretted ever leaving the job as research vice president, I'm now glad that the changes occurred. My new career has been a good one. I have grown and become more my own man.

One other event occurred while I was at LIMRA that is of some significance. I had been appointed to the editorial board of *Personnel Psychology* (*PP*) in 1967 when it was being edited by John Hornaday. He stepped down in 1970, and Fred (Fritz) Kuder, principal owner reluctantly took over, despite the fact that he was trying to devote full time to the creation of an "atlas" on the Kuder Preference Record. In 1972, I became uneasy about the quality of some of the pieces being published in *PP*, and wrote Rains and Bill McGehee to scan the last two volumes and give me their reaction. I didn't say why, as I didn't want to bias them. They both told me that they guessed I detected a decline in quality and that they agreed. I asked Rains if he would consider being editor, if Kuder were interested. He said he would if Fritz were genuinely interested in giving up the editor's job.

Talk about chutzpah! I didn't know Fritz, had never talked to him, and didn't know of his desire to work on his atlas. After much careful planning and script writing, I called him and gingerly approached my concern. What a scholar and gentleman. He immediately warmed to the idea of a new editor, and was elated by the prospect that Rains might take over. They negotiated, and Rains was took over. Unfortunately, Rains died of spinal cancer soon after that, but Milt Hakel was ready and willing to step in as editor. And later, he bought the journal to become publisher. So a nosy guy who was concerned with a fine journal jumped in where angels fear to tread. I'm glad I did.

NCSU

When it came time to look for a new job, I looked locally first so that Bjorg could continue as an associate professor at Hartford College for Women. Nothing that fit my talents or interests turned up, so we made the decision to look at other companies in the industry, other industries, consulting, and academia. I had good offers in the last two and decided that becoming department head at North Carolina State University offered the best opportunity. The faculty wanted to put the department on the map, were talented, the dean had considerable integrity, and the Raleigh area offered good climate and golf year round.

People considering such a move should be prepared for a little culture shock. Basically, the university provides you with a phone and an office, but asks that you limit your long distance calls. If you need anything else, you get it yourself. In addition, you may have to contribute to your pension, pay for your group life and income disability insurance, and fund a lot of your own travel. (My personal travel budget at LIMRA exceeded what I had for the entire faculty at NCSU.) In return for these benefits, you get considerable freedom in choosing the areas you want to research and teach. The class schedule can be confining if your university takes a dim view of missing classes, as mine did.

At NCSU, I tried to lead by example. I published and/or gave major papers at national meetings every year. My students and I did some interesting research on behavior modeling training, women in nontraditional occupations or majors, job design, procedural justice, and assessment centers. I've become interested in giving I-O psychology away, and have spoken to other psychologists about what we really do (Thayer, 1988), and to high school, college. and business groups about psychology in the workplace. I've worked up one popular piece for business groups and conventions based on the literature on work motivation, and plan to do three or four others. It's fun giving such talks, and there are a lot of people who have new respect for the field as a result. Some say they no longer think that what we do is just common sense. In my view, we need to be doing a lot of this sort of thing.

At NCSU, I believe we did much to strengthen both our graduate and undergraduate curricula. Four dissertations in 5 years won national or regional awards. (Mike Campion won the S. Rains Wallace Dissertation Award.) Our I-O students have all done well in the job market. I'm glad that I didn't have to spend more than 6 months as Interim Dean of my college.

After David Martin arrived to take my place, I served 1 year as Interim Head of Occupational Education, in return for 3 months leave to be a Visiting Professor at the Armstrong Laboratory at Brooks Air Force Base in San Antonio, Texas. I did a review of the climate for transfer of training literature while there (Thayer & Teachout, 1992), expanded the Rouiller and Goldstein climate questionnaire, and developed another to assess the extent to which a training program incorporates various principles of transfer. This work has been used by a few people, and Tony Machin has included his version of our transfer model in his chapter in Kraiger (2002).

I retired formally on August 31, 1993. Since then, I have continued my involvement with SIOP (Fellowship Committee and the SIOP Foundation) and APS (see below), have written some papers and presentations for journals and conventions, spent some time each day on the Net, traveled to foreign lands with Bjorg, and played a lot of golf, also with Bjorg. I also serve on the Technical Advisory Board of ePredix (now Previsor), am helping Laura Koppes edit the history of I-O, and writing a chapter on developments in training with Irv Goldstein.

Most recently, a former student, Leslie Joyce, did me the great honor of establishing the Leslie W. Joyce and Paul W. Thayer Doctoral Fellowship in I-O Psychology in the SIOP Foundation to recognize the mentor relationship we have had during graduate school and her subsequent career.

Of course, we delight in visiting our children and grandchildren. Scott and his wife Carol have two boys and live in Connecticut, and Lisa and her husband Andy have four girls and live in Massachusetts. Chris is developing teaching aids at the Harvard Business School.

Retirement is great. It's nice to do what you want, and especially, when you want.

SIOP, APA and APS

From 1956 until now, I have been actively involved in SIOP, APA, and APS. For SIOP, I have served as member and chair of the Committee on Committees and Fellowship Committee, and

member of the Scientific Affairs Committee, the Task Force on History and Centennial and have been elected to all the offices: member-at-large, secretary-treasurer, representative to Council (elected twice and appointed once), and president. Ive also been on the advisory boards for the *Principles* and *TIP*. I was on the Executive Committee for 18 consecutive years and went back on during 198991 to complete Frank Landy"s unexpired term on Council. As Bill Owens pointed out in his autobiography, I was responsible for giving plaques to past-presidents, and I also organized the Dutch-treat past-presidents dinner, formerly held each year at APA, and now moved to the annual SIOP meeting. Since 1999, I have been on the Board of the SIOP Foundation, first as vice-president, and now as president.

In APA, I was on the Insurance Trust for 9 years, serving as chair for two. I was a member of the Finance Committee and Board of Convention Affairs, and member and chair of the Policy and Planning Board and the Membership Committee. In almost all those roles, as well as on Council, I have watched the healthcare practitioners wear away the scientific base of psychology and turn APA into a practitioner organization. As many know, I spent countless hours trying to help solve their problems with solutions that would not harm their colleagues. Whether the issue was credentialing, licensing, accreditation, dues, assessments, graduate training, or whatever, this group of dedicated practicing clinicians patiently dug away. One had to admire their tenacity. If they didn't win one time, they would be back with a different approach next time. Inch by inch, then foot by foot, and finally mile by mile, they took over. And now the takeover is complete. I fought for what I thought best for psychology and for I-O psychology, for good science and science-based practice.

When reorganization failed, I helped organize ASAP, which became APS, and then served as treasurer and chair of the Finance Committee. I've been involved in the development of the APS journals and convention and find them stimulating. The thing I like most is that both journals and convention programs are pitched at a level that any non-specialist can understand what s/he is exposed to. At conventions, I go to presentations on social, clinical, developmental, biological, cognitive, an so on. It's fun and keeps me a psychologist, as well as an I-O psychologist. I am optimistic about its future and the future of psychology.

Some Final Thoughts

As I look back over these years, I am quite content with what I have been able to do. When I started out, it never occurred to me that I would write a book that would be referred to as a classic. Neither did I think I would be asked to serve in so many capacities. I basically did the jobs that had to be done as best I could, and people seemed to appreciate that. I wasn't looking so much for the return as I was doing what was always expected of me since I was a boy. I owe my parents a good deal for inculcating a strong work ethic, as it has served me well in terms of monetary and career rewards and considerable satisfaction. It probably also made me more acceptable to Bjorg, as she has similar attitudes. And for that, I am especially grateful.

As for the field, I believe we have come farther than I thought possible in 50-odd years. The pleas for more theory and less raw empiricism have been heeded. The sophistication of our research and research designs has improved significantly. We are less and less the tools of management, and more and more the advocates of the organization, its employees, and the

consuming public. We have been a rational voice in the debates over equal employment opportunity. We are breaking new ground in many areas and are earning the respect of our basic science colleagues.

As for SIOP, it is a most effective organization. Our annual meeting is fun and exciting, *TIP* is without equal, and the services to members improve each year. The Frontier and Practice series are a substantial contribution to theorist and practitioner alike. I can see only more of the same, as we face the challenges of a changed APA and relationships with it and APS.

Would I choose this field again? You bet! I might not do everything the same way, but I sure would do most of them. It is a good career.

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