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SIOP President 1997-1998

Several members of my family have pursued careers in education (my father, three aunts and my older sister have all taught), so a career in academics was a natural one for me. The biggest single influence on my choice of areas was probably my father. When I was growing up, he worked as a high school guidance counselor. When I was in high school, he became Assistant Dean and later Dean of Junior College of Albany. Administrators at that school were expected to teach a course from time to time, and he taught Introductory Psychology. As a result of leafing through a few textbooks, I was hooked, and became a psychology major as soon as I could (during my first year at Siena college, psychology was not yet an option, and I started as a sociology major. I changed to psychology at the beginning of my sophomore year).

Siena College is a small Franciscan liberal arts school just outside of Albany, New York. The school put a strong emphasis on a broad education, with a solid foundation in the classics. When I first started (1970), all students were required to take a 2-5 courses each in foreign language, literature and composition, philosophy, theology, math, and history, and between the college-wide requirements and the requirements of my major, I would not have had an elective course until my senior year. The curriculum loosened up a bit while I was in school, but not before I had been dragged, kicking and screaming, through a pretty good education.

I had excellent luck with a few professors at Siena, particularly Bob Woll, who as a new faculty member was expected to teach just about every course in a small department (I took Child Psych, Physiological Psych, History and Systems, Experimental Psych, Statistics, and Nonparametric Statistics from him). He was a talented and enthusiastic instructor, who set high standards and brought out the best in his students. I was not a great student in high school, but once I had the opportunity to work with really good professors, my interest, motivation, and performance took off. It is a lesson I have tried to incorporate into my own career, and I have worked hard to be as good in the classroom as some of my professors in college.

There are lots of advantages of a small liberal arts school, but one downside is that the faculty had relatively little experience with the range of areas in psychology. Relatively few students in this program had gone on to graduate school by the time I was finishing up (mine was the second or third class to graduate with psychology degrees), and the advice I received about graduate school was not really on target. My professors were either older clinicians or brand new experimental psychologists (like Bob Woll), and the career advice I received was not all that accurate. Bob

convinced me that I-O psychology was really human factors stuff, and that sounded pretty boring. Other faculty convinced me that the only real career for me was as a clinician. I was skeptical, but decided that they knew better. My first rude awakening was when I applied to graduate school. I sent applications to 20 top clinical schools, and was turned down by every one. The day I was turned down by my last remaining PhD program, I was also turned down by the Peace Corps, largely because I had no discernible skills. That was a pretty bad day!

One piece of questionable advice from the faculty worked out well for me. One professor suggested that I should apply to a local M.S. program, just in case nothing else turned up. The idea was that I could get a Master's degree in experimental psychology (which, as far as I knew was all that was available to me), and try again for clinical with a graduate degree in hand. I doubt that it would have worked, and I am glad that I never tried this route. I would have been a miserable clinician. In any case, the only opportunity I had to pursue graduate school was to get a MS at Rensselaer Polytechnic Institute. It seemed better than driving a cab, which was my next best choice, so off to RPI I went (1974).

My first day of graduate school was memorable in many ways. I was assigned to work with a research group in physiological psychology, doing brain surgery on monkeys. I could smell disaster (literally). I wandered into the graduate student offices, pretty much in despair, and ran into Bob McIntyre, who had been a year ahead of me at Siena. Bob and I were good friends, and became closer friends still during graduate school. Bob was in the I-O program, and once he told me what he was doing, it was clear that I-O was a much better fit for me than monkey surgery. Luckily, the department at RPI was pretty flexible, and I was able to transfer into the I-O program before the end of that week. Bob introduced me to Matt (Joseph) Madden, who became my advisor.

Matt had started out as a hard core personnel psychologist, doing job analysis work with Ray Christal in the Air Force, but had become an organizational psychologist, with interests in organizational development. Bob also introduced me to Reg Hendricks, who was a new faculty member with a PhD from Iowa State. Reg had tremendous talent in quantitative areas, and was a great teacher in statistics and in qualitative areas of I-O. Between Matt and Reg, I got a very solid exposure to different aspects of I-O Psychology.

In the early 1970's several RPI graduates had gone on for PhD's at Penn State, and had done very well (e.g., Skip Saal, Walter Freytag, Bob McIntyre, Janet Barnes-Farrell), and when I was finishing RPI, Penn State seemed like a natural choice. My final decision was between Penn State and Maryland, and I vividly remember calling Irv Goldstein to tell him that I was not going to accept their offer. He thought I was crazy, but when I told him that I was going to Penn State instead (Jim Farr was a pretty recent Maryland grad, and Irv couldn't bad-mouth the Penn State program too much), things calmed down and he wished me well. It turned out to be a good fit for me.

I entered Penn State in 1976. The I-O program at that time consisted of Don Trumbo, Frank Landy and Jim Farr. Penn State had a good mix between solid scientific training and good application. One feature of that program was a practicum, in which teams of graduate students would do projects with organizations, supervised by one or more faculty. The practicum met each week to talk about the projects, and to show the students how to translate the classroom knowledge into well-grounded applications. It was lots of work, in part because Penn State is in the middle of nowhere, and many projects were 3-4 hours away, but it was a good experience. The more senior grad students served as team leaders, which was a great experience if you had a good team and a real pain if you did not (this is pretty good RJP for the rest of your life).

By the mid 1970's, Frank Landy had already built a reputation as someone who got things done. During my first year at Penn State, he took off for sabbatical at the University of Stockholm, with the idea of starting a regular exchange program between Penn State and Stockholm. Bob McIntyre was scheduled to be the first Penn State visitor, with the idea that he would work with Lars Nystedt in Stockholm. Pretty much at the last minute (about 10 weeks before Bob's visit was due to start) some of the funding fell through, and there was not enough money to send Bob and his wife Mary. There was, however, just about enough to send a single student, and Frank asked me to go.

Frank's call came while I was on a summer internship at AT&T, in Basking Ridge. This was during the heyday of personnel research at AT&T. Ann Howard worked down the hall, the Management Progress Study was in full swing, etc.. I had arrived about a week after all the other interns, and there were no cubby-hole offices left. However, Don Grant had just left, and his corner office was available. I ended up in a very nice office, and given the status-consciousness at AT&T at the time, was treated very well by everyone. It was clear that I had to be an intern (if the car I was driving did not give it away, my attire sure did), but still, the office makes the man. Just as people were starting to wise up to the fact that I was just a grad student, I started getting calls from Frank, the Fulbright agency, the State Department, etc., all trying to arrange things at the last minute. My status went back up, almost to the level of the office I was camped in.

I ended up going to Stockholm during the Fall semester of 1977. I did not speak a word of Swedish, had no real preparation, and was a pretty pathetic sight on arrival in Sweden. I was, however, interested in some of the work Lars was doing, and I had developed the mix of computer programing, data analysis, and data management skills needed for the projects he had in mind. My lack of preparation (and their lack of preparation for me) turned out to be a godsend. I was generally free to use my time any way I wanted. Lars was a great person to work with, and while we were pretty busy, there was still lots of free time, something you normally did not get in graduate school. The department had a very good library, and I used the time to read journals. I took about 10 years of each journal I was interested in (JAP, Psych

Bulletin, Ed Psych Measurement, Psychometrika, and a few others), and read just about every article in each. I learned more by doing this than by just about anything else I did in graduate school.

Lars was interested in the question of whether people have insight into their own strategies for making judgments. The standard paradigm at this time was to use policy capturing and compare the accuracy of a policy equation based on self-reports (i.e., I think the cue X is twice as important in my decisions as cue Y) with regression equations. The near-universal finding was that regression equations performed better, leading researchers to conclude that people could not describe their own policies. There is, however, a serious flaw in this line of research. Most studies compare a subjective description of a policy with a statistically optimal regression equation, and evaluate the two in terms of which one produces the largest R2. There should not really be much suspense about the outcome of such comparisons; regression equations have to do at least as well and usually do better than any other linear equation (including your own description of how you make judgments), and the relatively poor performance of self-described policies is not evidence of a lack of insight, it is evidence that statistical optimality really works as advertised.

Lars and I did several studies in which we changed the ways of asking people to describe their decision policies (allowing for non-linear descriptions), and showed that these policies could outperform simple regression equations. This led directly to my dissertation, which looked at criteria that could be used to determine whether self-described decision policies really captured how decisions were made. As it turned out, my dissertation was another of those bad decisions that turn out well, as I will describe below.

The Spring before I left for Sweden, I had an experience that turned out to be momentous, but certainly did not seem so at the time. Jan Cleveland visited Penn State during one of State College's typical early spring days (clouds, snow, rain, etc.). She was fresh from California, had a great tan, and was dressed for real spring (I think she was wearing sandals). I was nearing the end of my hippie days, and had a beard, hair well below my shoulders, and was wearing a leather Fringe jacket. I had to clean up my act for the AT & T internship). We did not make much of an impression on each other (Jan remembers thinking that "there is one of these in every program"). I wrote this shortly after celebrating our 18th wedding anniversary; something that neither of us envisioned when we met.

To carry the Penn State-Stockholm exchange program along, Lars Nystedt came to Penn State for a semester (Spring of 1978), and I continued to work with him. When he left, there were not any faculty who were really interested in the sort of work I was doing, but Jim Farr (my advisor) was gracious enough to help me out and let me pursue my interests. I expected to build a research program in judgment and decision making, and on completing my PhD (1979), struck out in this direction. "Struck out" turns out to be a pretty good description.

My first academic job was at Rice University, with Bill Howell and Bob Dipboye as colleagues in the I-O area. As I started to write up my decision-judgment work for publication, I got a rude awakening. I had worked pretty much on my own in this area, and it turned out that I did not know nearly as much as I thought I did about the topic. This is not the first or only time that this has happened, but it came as a pretty distinct shock at the time. I was not able to publish any of this work, and at the end of two years at Rice, I still had no new publications.

I realized that judgment and decision making were not the areas where I was going to make my mark. I had done some research on performance appraisal at Penn State, and decided to switch to this area. I had the bright idea of studying accuracy in performance appraisal, and worked for almost a year on methods. I finally realized that Wally Borman had already done the same thing, using better methods, and that I had missed the paper because I was concentrating so heavily on judgment research. This time, I learned my lesson, went back to the journals, and learned everything I could about the current state of performance appraisal research. Frank Landy and Jim Farr were working on their review, which essentially reshaped the field, while I was at Penn State, and for once, I was able to get ahead of the research curve instead of behind it. I started doing cognitive research in performance appraisal just at the time that this was becoming a hot topic, and my research career took off once I made this switch.

Jan and I were married between my first and second year at Rice. She stayed at Penn State to finish her degree, and we had a long-distance engagement and first year of marriage. Our phone bills were unbelievable, and this was difficult for both of us, but it worked out in the end. As Jan was finishing up, we realized that there were few dual-career opportunities in Houston, so we both went on the job market. In the end, she had a very good offer from Bowling Green and I had a similar offer from New York University. This was a very tough decision, especially because her offer was so appealing (Bob Guion had been Frank's advisor, Jan was Frank's first PhD, and there was instant rapport with the Bowling Green job). However, the dual career opportunities were just better in New York. I joined the faculty at NYU in 1981, and worked with Ray Katzell, Madeline Heilman, and Rick Guzzo (Jack Kennedy joined the program later).

At NYU, I continued doing laboratory research on performance appraisal. This turned out to be perfect Assistant Professor work, with quick turnaround and relatively high hit rates at good journals (at the time, this was still pretty hot stuff). I also got my first insights into how SIOP works (although it was not yet SIOP). Ray Katzell was chair of the Scientific Affairs Committee at the time that the Frontiers series was launched, and we had many discussions at I-O program meetings of the rationale and progress of this project. This was also about the time that the potential split of APA , the birth of APS, and the start of SIOP were all beginning to take off, and it was a great time to be an I-O psychologist.

My job at NYU was a good one, but Jan's was not such a good one, especially at the start of a career. She started at Baruch College, City University of New York a year after I started at NYU. The Baruch PhD program was brand new, and the demands on new faculty were very high (heavy teaching loads, relatively few resources for research, all of the start-up work involved in launching a new program). After a couple of years, we decided to hit the job market again. There were a number of schools with multiple openings, and we ended up interviewing for jobs at Colorado State University and at Purdue. CSU was a very good fit, and we joined the faculty in 1984 and have been there ever since.

Soon after SIOP was formed, I started to serve on committees, and was asked by Paul Sackett to serve as chair of the Scientific Affairs Committee. This was my first opportunity to see in detail how SIOP worked, and I was able to get a pretty good sense of the Society by attending meetings as chair of this committee and later of the Fellowship committee. During my tenure on the Scientific Affairs Committee, we wrote a report on banding that served as a model for some of the initiatives I later proposed when I became SIOP President.

Jim Farr notified me that I was nominated to run for SIOP President. I agreed to run, largely because I know that everyone else on the slate was better qualified, and I was confident that someone else would win. My election was a shock to me, but again, I think that timing worked very much in my favor. I became SIOP President during a period of unprecedented prosperity and vigor in SIOP, and my biggest responsibility was to not get in the way of a smoothly functioning Society.

Several interesting and important things happened during my term (1997-98). We launched what I hope will be the first in a series of reports on areas where the interplay between science and practice in I-O could be improved. The Job Analysis and Competency Modeling Task Force, chaired by Jeff Shippman, launched an assessment of the scientific basis for and ways of improving the practice of I-O psychology in the area of "competencies". This is an area where great opportunities for applying our science exist, and where some of the practices (e.g., redesigning your personnel management system around poorly-defined competencies) that exist do not take advantage of our research base. I hope that this report will become a model for reports in other areas (e.g., selection, OD) where science and practice can and should meet.

APA has been revising their Testing Standards, and as a result of participating in this process, I proposed another initiative that I think will be useful to SIOP. One complaint about the APA standards is that there was no apparent scientific basis for some of the proposed standards. This observation led to the establishment of the Taskforce for Initial Review of SIOP Principles for Validation and Use of Personnel Selection Procedures (3rd Edition). The idea here was to take a hard look at the SIOP Principles, and identify the scientific support (or lack thereof) for each principle, as well as identifying areas where methodological progress is likely to call for changes or updates in our principles. SIOP will almost certainly want to update

their principles once the new APA standards come out (at least to correct cross-references), and by looking at the science behind our principles now, I think SIOP will be better equipped to avoid the problems faced by APA in their standards revision process.

Timing led to another initiative that I think could be important for SIOP. In May of 1998, voters in the Republic of Ireland and in Northern Ireland approved the basic legal structure for a wide-ranging agreement on the future of the island. One of the key provisions is the creation of commissions in both the North and the South that will deal with questions of employment discrimination. This is an area where SIOP has a great deal of expertise and experience, and SIOP has offered their pro bono assistance to both governments to help them in developing standards and principles, to assist in designing policies and procedures for defining, detecting, and evaluating discrimination, etc.. It is too soon to tell whether they will take advantage of our offers, but this is an area where we have a good deal to share, and where our technical assistance might prove useful to both governments as they develop structures for dealing with questions of discrimination in the workplace.

The most important initiative during my term as president was also more a matter of timing than of creative ideas on my part. SIOP has continued to grow, has established a SIOP Foundation (designed to solicit and administer gifts to the Society, including a number of bequests that have funded new SIOP awards), and is in a very strong financial position. As we started to discuss things that SIOP might do with all of the resources we now have, the Long Range Planning Committee quickly realized that we did not have a good strategic vision of where we wanted the Society to go. During our Winter Executive Committee meeting (January, 1998), the SIOP Executive Committee, the Committee Chairs, and eleven Past Presidents met for a 2-day strategic planing session. This meeting led to several initiatives, most of which are continuing under Elaine Pulakos (1998-99 SIOP President). I was in charge of one of these, developing an "identity statement". The goal of this initiative was to provide a short but reasonably complete answer to the question "what is an I-O psychologist." On the basis of suggestions from the committee chairs, executive committee and membership, my answer to this questions is:

"Industrial-Organizational (I-O) Psychologists are social scientists who use research, theory and data to understand and influence the behavior and experiences of individuals and groups in organizations. They are committed to doing research that is relevant to solving real-world problems and to practice that is firmly grounded in research and data. They have training, supervised experience and competencies in most or all of these areas: (1) the design and analysis of research, (2) the analysis and evaluation of jobs and work systems , (3) performance measurement, (4) assessing individual differences, (5) personnel recruitment, selection and placement, (6) training and structured learning (7) career development, (8) job attitudes and work motivation, (9) development and management of work teams, (10) leadership and supervision, (11) organizational theory and organizational development, (12) health and stress in organizations"

Finally, I had an experience that I think is shared by many SIOP Presidents. Upon hearing that I had been elected, my first two thoughts were : (1) this is a great and unexpected honor, and (2) what am I supposed to do for a presidential address? The presidential address turned out to be a lot of work, and a lot of fun. I took a topic I was somewhat interested in ("the criterion problem" and spent about a year thinking out it and putting together some (hopefully) coherent remarks about what I think I-O psychologists should be doing about the definition and measurement of "success" at the level of the individual job, the career and the organization. I had the good fortune of having a large number of family members (my parents, in-laws, children, sister, sisters-in-law and their husband, brother-in-law and great aunt) in attendance for my address, and it was a very special moment.

I suspect that the most lasting legacy of my term in office will be a change in the format of the SIOP conference. Last year, we bit the bullet and dumped the SIOP luncheon. It had become a gigantic affair, with thousands of members crowded into a ballroom (and spilling out into the hallways), eating cold meals and completely unable to see or hear what was going on (we have used the luncheon to present new fellows and Society Awards). This is one SIOP tradition that will probably not be mourned; we now roll awards, fellows and the Presidential address into a single session, and we are able to free up huge chunks of time and space for the SIOP program. I think this will improve what is already the crown jewel of SIOP - the Society Conference. All in all, it turned out to be a pretty eventful year, and with some luck, things we did during my term as president will have some lasting benefit for SIOP.