

# Neal Schmitt

## SIOP President 1989-1990

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I was born June 13, 1944, in Decorah, Iowa, and spent the first eighteen years of my life growing up on a dairy farm in northeast Iowa and attending grade school and high school in St. Lucas, Iowa. The community was a very stable German Catholic farming community. One index of the stability of the community was the fact that all but one of the fourteen students who graduated with me from high school started first grade with me as well. The only new student moved to our town in the fourth grade. We attended a Catholic school as there was no public school in the community; in fact the whole community was Catholic and German. Surrounding farm communities were similarly homogenous--there was a German Lutheran town, a Polish Catholic town, an Irish Catholic town, etc. The education I received was probably far superior to that I would have received in other local communities served by public schools since at that time in Iowa, farming communities did not support education beyond what was thought to be essential to carry on farm business, usually no more than an eighth grade education. The Catholic schools had the advantage of highly educated, almost free service of nuns and priests. These people insisted that the better students at least consider post high school education, and a good number of the people in our community did go on to college. Most went to Loras College, a small liberal arts college in Dubuque, Iowa.

I thought at the time that I was going to a large institution (some 1200 undergraduate students attended Loras at that time) in a large city (60,000). I doubt I knew what Psychology was when I first went to Loras, but at the end of my first year there I had declared it as my major. For most students at Loras at that time, Psychology was synonymous with Clinical Psychology, and I suspect that I intended to pursue that course as well. However, in my sophomore year, I volunteered as a Big Brother. This meant establishing a relationship with a five to nine year old at a home for disturbed children. We took them out every Saturday afternoon for about four hours. I remember being extremely upset at watching a perfectly charming child turn into a demon who called me and others every four letter word I knew and some I did not. I realized I could not work as a clinical psychologist and began exploring other applied fields in psychology.

In the meantime, I seemed to take every undergraduate course that looked like it might be interesting. I ended with a major in Psychology, but enough credit hours for a major in Philosophy and minors in English and Biology and a secondary school teaching certificate. Upon graduation, I taught junior high math and science for a year while applying to graduate school in Industrial-Organizational psychology--the only applied non-clinical area of psychology about which I was aware. My exposure

to I-O was by way of reading (skimming) various introductory I-O textbooks available in our library.

### **Graduate School and the Military Experience**

Having applied to Purdue, Iowa State, and Ohio State, I went to Purdue because I had been told it had the best academic reputation in I-O Psychology and because their graduate stipend was the largest. Purdue was obviously a much larger institution than my undergraduate school and perhaps my biggest misperception was that I was moving "east." I was also introduced to Big Ten football and an incredibly large number of I-O Psychology students. The program at that time had 67 graduate students; there were 22 in my entering class and 25 in the class that directly preceded mine. A rather large number of these persons have made an impact on our field. Dick Jeanneret, Rich Klimoski, Alan Nicewander, and Frank Schmidt were in the class ahead of me. My immediate colleagues included Bob Mecham (Utah State University), Hurley Hendrix, (Clemson), Jerry Olson (Penn State), and my future wife, Kara, who is now Director of the Office of Testing for the Michigan Department of Licensing and Regulation.

There were also a great many I-O people on the Purdue faculty at that time (Fall, 1967). They included Joe Tiffin, Don King, Bob Perloff, Bill Owens, Art Dudycha, Don Jewell, Ernie McCormick, Hubert Brogden. However, at the end of that year Don King had moved to the business school at Purdue, Bill Owens had gone to University of Georgia, Don Jewell departed for Georgia State, Bob Perloff went to Pittsburgh. Early in the fall, McCormick had a serious heart attack. This group was replaced with one faculty member, Jack Jacoby, who began to develop a research program and academic reputation in consumer psychology. Jim Naylor returned to Purdue from Ohio State as department head in the fall of 1968. This turnover and the shrinkage in the faculty resources meant there was a real scramble among graduate students for faculty time and it also meant that ours was the last very large graduate student class.

My entire time in graduate school was spent in a great hurry. As the military draft was a constant threat, I was always trying to meet a new deadline before I would have to leave. I finished my Masters thesis in the fall of 1968 with Art Dudycha. It was a study in which we tried to predict supervisory success with a situational test of leadership called the LEADS, the Purdue Adaptability Test, and biodata. I spent the following summer studying for preliminary examinations for the doctorate, but my draft notice came two weeks before we were to take the exams. To salvage the preparation for these exams, I enlisted for Army Officer Candidate School thereby delaying my induction date two months. So I went to the Army in October, 1969, ABD.

The first year of my Army life was spent in basic training and advanced engineering training at Fort Leonard, Missouri, and Officer Candidate School at Ft. Benning, Georgia. As I was ready to graduate from OCS, the Army offered our company the

option of remaining in OCS or a year off our enlistment and the remainder of our time in the duty station of our choice. As I felt every minute of Army life was inherently evil (and still do), I left OCS and spent the remainder of my Army time as an enlisted person assigned to the Department of Military Psychology and Leadership at West Point, New York. This assignment had several positive outcomes. My wife joined me and taught junior high school in Highland Falls, New York. I met Dan Ilgen who was an ROTC officer assigned to the same department. Since I had no discernible job duties, I spent a great deal of time in the library and taking various computer short courses learning how to use the computer and software packages available at that time. I also taught Introductory Psychology and a course called Social Problems at Dutchess Community College in Poughkeepsie, New York. In August, 1971, I left the Army and returned to Purdue University to complete my Ph.D.

When I returned most of the students were new. Paul Muchinsky was my new office mate and Bob Dipboye was one of four new graduate students that fall. My last year in graduate school was devoted almost exclusively to my dissertation research. The title of my dissertation was "The Effects of Cue Redundancy in Multiple Cue Probability Studies." My mentor during this last year was Dr. Art Dudycha, and like many of his students as well as those directed by Jim Naylor during this time, our research was inspired by the Brunswik lens model. My doctoral research was published in *Memory and Cognition* and *Journal of Experimental Psychology*, and a small research note which grew out of this research was later published with Art and Linda Dudycha in *Organizational Behavior and Human Performance*.

While the lens model research did not provide experience in doing field research and it probably would not be recognized today as Industrial-Organizational psychology, it was very important in defining my subsequent research efforts and provided excellent training in the use of multiple regression analyses as well as analysis of variance. I did complete several subsequent studies on multiple cue probability learning, most of them on the feedback issue (because I was unconvinced, and still am, that subjects were actually learning anything in most of these experiments). Lens model research conducted in a field context usually meant some kind of policy capturing study. I and various coauthors have done policy capturing research on interview decisions, assessment center ratings, and performance ratings and more recently, women's decisions about whether or not to take estrogen replacement therapy and business relocation decisions.

The other positive feature of the lens model research background was that it provided excellent instruction in the vagaries of multiple regression. Cue-criterion relationships were almost always expressed in terms of correlations and with multiple cues that were intercorrelated, so there was the problem of multicollinearity. A good portion of my time the first few years of my academic career was spent trying to discover or develop a statistical solution to this problem. These efforts included delving into such esoterica as ridge regression, but I finally concluded, as is described in an OBHP article with Ralph Levine, that there was no

statistical solution (I suppose it would take others a much shorter time to conclude this, but I was stubborn).

### **Initial Work Experience**

My first job search efforts were frustrating and disappointing. There did not seem to be many "good" jobs and those few schools that did have positions were not interested in me. I had no publications, little or no applied experience, and, because of the two years in the military, my vita looked like I could not make up my mind what I wanted to do. Complicating matters was the fact that my wife was also looking for a job, and we were trying locate in roughly the same area of the United States. We finally both took positions at Northern Kentucky University located in the metropolitan Cincinnati area. My wife soon grew disgusted with her half-time position there and took a job in Saratoga Springs, New York at Empire State College. At the same time, things were deteriorating in the Psychology Department for me. My colleagues were mostly new Ph.D.s and competent and congenial persons, but the chairperson and the school administration soon alienated all. Support for any form of scientific endeavor did not exist; in fact such work was actively discouraged.

By the beginning of my second year there I had concluded that I would either get a better job in Psychology or leave the profession. I applied for various jobs and for admission to medical school simultaneously. This time my job search efforts were more successful. I interviewed at Michigan State and Ohio State. Probably because Frank Schmidt championed my candidacy, I was offered a position at Michigan State and went there in the fall of 1974. While most of my tenure at Northern Kentucky was negative, I have maintained contact with many of my colleagues there both in and out of Psychology. I have continued to collaborate actively with Dr. Michael Colligan who is now at the National Institute of Occupational Safety and Health in Cincinnati. We have coauthored papers on various health and safety issues, and I have received several small grants to pursue various research issues from NIOSH over the last fifteen years.

### **A New Faculty Member at a Research Institution**

My first years at Michigan State were challenging and productive. I welcomed the opportunity to work with other Industrial-Organizational psychologists and graduate students, but I soon realized that neither was quite the great advantage they seemed prior to my arrival. Two months before I moved to Michigan State I found out that Frank Schmidt was moving to the Office of Personnel Management in Washington, D. C. This left three I-O faculty members and Jack Wakeley (also I-O), who was the department chair. The other two, Drs. Carl Frost and Fred Wickert, were both in their sixties and were no longer actively engaged in research though both continued to contribute to the department and the university in many ways. Further, these individuals as well as those in other interest areas in the department were busy with their own projects and did not have the time to engage in new efforts with a junior faculty member. I never did develop any collaborative work

with faculty at MSU till much later and then it involved writing (e.g., Staffing Organizations with Ben Schneider) rather than research. I soon realized that if I were to "make it" at MSU, it would have to be at my initiative. I suppose that is not a startling discovery, but it seems to be one that we need to make in various transitions (high school to college, college to graduate school, parenthood, etc.).

There were twenty-two graduate students all of whom were looking for a research collaborator. The first year, I also taught graduate courses in personnel selection and psychometric theory and undergraduate courses in I-O Psychology (twice), Measurement, and Personnel Research Techniques. Needless to say the time to launch a research program was minimal; in fact, my teaching commitments were greater than they had been at Northern Kentucky.

The large number of eager graduate students and the relative lack of competition from other faculty meant that the first few years at Michigan State involved relationships with some very productive graduate students. Bryan Coyle, Larry King, Bruce Saari, John Ogilvie, Tom Hill, and I worked together on a large number of projects which were subsequently published in various journals. I believe I used graduate seminars very effectively at this time. One seminar on decision making with nine graduate students generated seven published papers not to mention a couple of dissertations. During these first years I had no funded research or consulting, but pursued laboratory research, some Monte Carlo work, and field surveys. Expenses associated with surveys or laboratory research were either picked up the department or by simply spending the money and worrying about payment later.

I did have plans for research when I came to Michigan State and that was probably fortunate because the schedule of work there soon precluded any form of systematic planning or reading. As indicated above, I developed a series of studies on feedback in multiple cue probability learning which I and several of my students conducted and published mostly in *Organizational Behavior and Human Performance* in the late 1970's. At the same time that I was doing these studies, I tried to develop various field studies as well (given the great deal of time and effort spent in gaining entry in organizations and collecting meaningful field data, I think doing both lab and field work provided the research issues are amenable is an excellent strategy for new faculty members interested in academic survival).

My first attempt at field research was a study of how the placement interview and what transpired in the interview (subjects were MSU students applying for jobs at our placement center) influenced subjects' responses to an interviewer and the organization he or she represented. This study was a policy capturing study and my intent was to compare the decision strategies of interviewees and interviewers. We never did get to the comparative part of our work, but the article describing interviewees' reactions (JAP, 1976) has had a significant impact in that it focused on the reactions of the job applicant. There have been a series of similar studies--now more broadly considering the whole recruitment process that have cited our study.

During this first year at MSU, I also reviewed the interview literature in preparation for the placement center research and published that review in *Personnel Psychology*.

Two other research projects developed rather by chance out of questions by graduate students in a class on psychometrics. One involved a question on how cross-validation was actually done. I thought this was a simple question but found it was much more complicated in practice. The end result was a Monte Carlo study on the relative accuracy of formula-based as opposed to empirical estimates of cross-validity (*Psychological Bulletin*, 1977). The other question involved how one determines whether the Campbell-Fiske criteria are met in a given multitrait-multimethod matrix. In developing an answer to that question, we wrote a review comparing various ways of examining such data. That review and an empirical comparison of various techniques was published in *Multivariate Behavioral Research*. In terms of reprint requests (certainly several hundred), this paper has been my most popular paper.

In the summer of 1975, I began collaborating with Ken White of the MSU business school and several students on a project funded by the Michigan Department of Labor. This project was supposed to examine what motivated various job change behavior. I am not sure we ever developed information that was useful to the State of Michigan, but we did do several large-scale surveys conducted across time. Several papers on work motivation and retirement decisions resulted from this work.

In the spring of 1976, I received a very helpful reprieve from my teaching responsibilities. During that term and the following summer, I and my graduate students completed at least ten different papers that were subsequently published. This mini-sabbatical came at a critical time—data had been collected and analyzed and I was ready to devote a considerable period of time to writing.

These various efforts had a positive impact on the Psychology Department faculty and in 1977, they voted to promote and tenure me. This came as a surprise to me as I had always assumed that I would receive no credit for time spent at Northern Kentucky before coming to Michigan State. A second promotion to Full Professor came in 1980.

### **Post Tenure Academic Life**

During these years my wife had continued to work for the State Department of Education (she originally found her job by consulting the yellow pages of the phone book). In 1979, we became the parents of a son (Kevin Craig) who died at birth. My wife went back to work immediately, but I had arranged to be free of teaching that term and spent much of it outdoors. Work did not seem as important, and I decided to buy a small "farm" (ten acres) partly to force myself to take time off work in the future. It didn't work. The following year we became the parents of our daughter

(Krista) who is now nine years old. Work is now permanently secondary, though both my wife and I feel doubly overloaded as well as guilty about our inability to balance work and family appropriately.

In 1979, I was asked by Paul Hersey to validate an assessment center that his organization (National Association of Secondary School Principals or NASSP) and Division 14's Public Policy and Social Issues Committee had developed to evaluate the administrative and interpersonal skills of candidates for principalships of schools. This request started a long relationship with NASSP that continues to the present. We completed and published the results of the validation study (JAP, 1984), but also developed additional assessment exercises that were an outgrowth of the assessment center development. Several graduate students (Mike Fitzgerald, Ronni Merrit, Ray Noe, Jeff Vancouver, Mary Doherty, Scott Cohen, and Cheri Ostroff) have been supported by NASSP funds or have used the data we collected to complete masters and Ph.D. theses. The collaboration with NASSP has been pleasant and productive. The assessment center which was developed as a small demonstration project by Division 14 people has spread to over 40 states and five foreign countries. Over 11,000 have been assessed and nearly 6000 have been trained as assessors (in many cases, assessor training is treated as a developmental exercise).

After several years working with NASSP, I was asked to serve on a task force on school climate organized by Dr. James Keefe of NASSP. School climate research actually became school effectiveness research and during the next six years we did two very large scale studies in which we explored the relationship between various job and school attitudes and a variety of outcome variables such as achievement test scores, vandalism, dropout rates, teacher turnover, and other indices. In our last effort, we collected data in 360 schools from teachers, principals, and students. Since we have both subjective and "hard" data from a large number of schools, I believe several publishable papers remain to be written. Cheri Ostroff wrote her dissertation investigating the degree to which personality and organizational climate interact to produce various job attitudes. Jeff Vancouver completed his doctoral dissertation on goal congruence and Mary Doherty is now pursuing a dissertation on an organizational model of turnover using these data.

During the past several years, I have found it increasingly difficult to find time to write data-based research papers even when I know that papers ought to be written and published. I think there are three major reasons for this. One is the increased temptation to write invited chapters which allow greater freedom in format and are usually not reviewed. I have also completed two books, one with Ben Schneider titled Staffing Organizations and a second due to be published in 1990 with Rich Klimoski titled Research Methods in Human Resource Management. The second reason is the necessity to continue to generate funds for graduate student support. Dr. Carl Frost had provided a significant amount of funding for graduate students at MSU. After his retirement, much of that obligation fell to me. This has led to the pursuit of a rather large number of consulting-research projects. While I have been successful in some instances in doing research papers based on these efforts, most

of the them are done purely for the financial support they afford. While this is not all bad (i.e., students gain valuable experience in the practice of I-O Psychology), the sheer number of the projects that are necessary to support our graduate students generates a bewildering array of conflicting responsibilities. The third reason that makes continued writing of research-based articles difficult is my increased involvement in professional activities.

In the paragraph above, I mentioned the difficulty I have had in continuing to do publishable research in the face of practical organizational demands that come with consulting projects, In some instances, I have been successful in this regard. For example, the paper with Cheri Ostroff in *Personnel Psychology* (1986) on the development of selection procedures for the position of emergency telephone operators, the paper with Scott Cohen on job analysis ratings (JAP, 1989), a paper with Jeff Schneider and Scott Cohen now under revision for publication in *Personnel Psychology* were all the outgrowth of projects that were initially started to provide for student funding.

Successfully combining research and practice involves several critical elements. First, one must know what the research issues are - teaching, reading, and writing are critical in staying current. Second, one must plan the research elements. If there is no planning, it is almost certain that the critical variables will not be collected (it is often difficult to collect the research variables when you do have a plan because of client resistance). The planning is critical and you almost never have adequate time, because of the need to act now. Third, one must have graduate students who are interested in research issues. I continue to have excellent research-oriented colleagues-most recently, Elaine Pulakos, Ray Noe, Cheri Ostroff, Mary Doherty, Scott Cohen, Keith Hattrup, Steve Gilliland, and Jeff Schneider. These students have more time (hard to believe) than I and the enthusiasm and motivation that pushes these projects to completion.

### **Professional Activities**

I also mentioned above that professional activities have taken up an increasing portion of my time. Beginning in 1976, I have served as chair or member of a large number of Division 14 committees including Public Policy and Social Issues (we initiated several collaborative activities with various labor unions and did a job analysis of I-O psychologists' work), Scientific Affairs (we organized an Innovations in Science day at a SIOP convention), Ad Hoc Testing Issues (we revised the Society's Principles on the Validation and Use of Selection Procedures), and Long Range Planning (we conducted a survey of the membership).

In 1988, I was elected President of SIOP. In that year, I also began a six year term as editor of *Journal of Applied Psychology*. I am now "settled in" as editor (I have been taking manuscripts for two years) and find the work interesting and challenging, but time consuming (at least twenty hours a week). It is also a constantly demanding job



making even short diversions into other work or vacations nearly impossible or certainly very punishing.

Perhaps the most frustrating professional experience over the last fifteen years has been the continuing concern with the licensure and/or certification of I-O psychologists. My experience with this problem began in Michigan when the clinical psychologists here managed to pass a law which had the practical effect of excluding all but their own. After some ill-advised efforts (as President of the Michigan Association of I-O Psychologists) to get a change in this law, I abandoned the project and swore I'd never be involved again. However, the issue simply will not go away and is the subject of much debate within the Society again as we evaluate our posture with respect to licensing issues. I have no tolerance for the turf-protection motivation that inspires most licensing efforts and have real difficulty dealing with this issue and the people interested in it in an even-handed manner. My wife, who is now head of the Office of Testing for the Michigan Department of Licensing and Regulation shares this view and probably serves to polarize me further. The licensing issue, continued legal issues having to do with employment discrimination, and a change in SIOP's bylaws to open membership in SIOP to members of the American Psychological Society undoubtedly have had the greatest impact on SIOP during my presidency.

### **The Present and the Future**

At the present, I believe our program at Michigan State is a strong one (in addition to myself, Dan Ilgen, Kevin Ford, Steve Kozlowski, and Mike Lindell are the faculty) and our interest area is respected within the department and the university. It wasn't always this way, and it has been satisfying to see the change. We attract the best applicants to graduate school here, and we do continue to support them financially. We have purposely kept our program small (15 to 20 students) so as to provide adequate guidance and mentoring as well as continued financial assistance.

I also believe the field of Industrial-Organizational psychology continues to be healthy. Our major professional organization has the enthusiastic support of a large number of members who give considerable amounts of time to various intellectual and professional concerns. Programs throughout the country continue to attract a large number of undergraduates and graduate students. Organizations continue to recognize our contributions and request our assistance.

There are some areas though in which I believe we have reason for concern. The first is that it is becoming increasingly difficult to maintain a graduate program in I-O psychology. I-O psychology has always been seen as a fringe application of psychology - one that can be afforded and tolerated when funds are plentiful, but cut in times of financial distress. As there seems to be a continuing need to evaluate academic programs in the face of steeply escalating educational costs at many state institutions, the temptation to cut the I-O psychology program will remain. Second, we continue to compete with business programs in Organizational Behavior who

can pay faculty at starting salaries \$10,000 to \$30,000 more than is true in psychology programs. With the increased emphasis on internships and practical experience of I-O graduate students, I think the new Ph.D. is less prepared to take on a role in a scientifically-based, "publish-or-perish" department so that even after recruiting a young faculty member to a psychology department, we run the risk of losing the promotion and tenure battle several years later. We have been reasonably successful in helping organizations and individuals to be more productive workers, but I think we should expand our focus and efforts in other areas relevant to the work experience of individuals. Since this is the theme of my presidential address, I have done some thinking about what else we might do and would suggest that we ought to spend more time on the development of work values in youth (socialization of responsible young adults), the utilization and adaptation of retirees, the problems of single parents and dual career couples, and the incorporation of minorities and women into jobs not traditionally held by these groups.

Since my tenure in this profession has been relatively brief (fifteen years), I am not sure what impact I have had (or will have) on I-O psychology. I have indicated some research papers which seemed to be well received - interestingly most of them were written my first three years here. Hopefully this can be attributed to the passage of time not my early obsolescence or senility. I do hope that one of my major contributions will be the continued vitality of I-O psychology at MSU and the educational and professional experiences that program offers its graduate students.