

# Wally Borman

## SIOP President, 1994-95

### A Professional Autobiography

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This autobiography will pretty quickly fast-forward to post-graduate school activities. But, first, a few relevant earlier experiences that may have influenced my professional life.

I was raised in a small rural town outside of Chicago; it's now part of the sprawling suburbs! I didn't attend a one-room grammar school, but it was close, four rooms, two grades per room. This comfortable, small-class school experience changed dramatically when I graduated to a N = 5,000 high school. I actually adjusted quite well, and my main academic interests in high school were math and physics. These interests continued into the university setting. At Miami (Ohio), I had a Naval ROTC scholarship which at the time was a full-ride, and I began majoring in physics. I actually won the Culler Prize my sophomore year, the award for outstanding achievement in physics, and felt I was on my way to a career in this field.

My junior year derailed this scenario. Two things happened: (1) my advisor thought I could handle taking three physics courses rather than the standard one or two and (2) I enrolled in Introductory Psychology with Professor John Jahnke. The physics courses turned quite theoretical and I didn't do very well in this area. Dr. Jahnke was a fantastic instructor, charismatic, Harvard PhD at 24, totally positive and supportive, and knew our names the second week of the course (with a class of 75 or so!). Decided I better switch. I crammed 30 hours of psychology into my final two years at Miami, and graduated with a dual degree in psychology and Naval Science.

Now it was time to serve my country. My four-year commitment to the Navy turned into a five and a half year stay because of Viet Nam. Serving at the pleasure of the President was the catch. During my Navy experience, I was very concerned about my mind deteriorating, read incessantly, bonded with other graduate school bound squadron members for support, and dreamed of getting on with my professional life.

My last year of active duty I applied to several graduate programs in I-O and counseling psychology. I thought I wanted to be a consultant, although I was quite naive about what that might involve. I was accepted at most of my choices including the University of Minnesota in the counseling area, with a generous fellowship. But I hadn't heard from Berkeley, which is where my wife wanted to go (shes no dummy).

So I called Bill Graham at Berkeley and asked about my application. He said he didn't have my name on the list of applicants and that he would get back to me. He called the next day after he found my file, mentioned he liked my GREs, and said (rather casually) that I should come on out and join the I-O graduate program.

I did that. I started at Berkeley in the Fall of 1968. Ed Ghiselli, Bill Graham, and Milton Blood comprised the faculty. Shelly Zedeck would join the group in 1969. The Berkeley graduate school experience was incredible intellectually. The I/O faculty were wonderful, but I also got into the IPAR (Institute for Personality Assessment Research) crowd. Gough, Mendelsohn, Craik, Walter Hall, Lew Goldberg (visiting, 1971-72), and others were doing great things in personality assessment, and I was drawn to this work. I spent a lot of time at IPAR during my last year or so; I was especially interested in Personal Construct Psychology. I actually elicited the personal constructs related to personality (and their definitions) from several of these prominent personality psychologists. They represent rich depictions of these person's view of the important folk personality constructs (e.g., Grizzled Veteran vs. Green Kid).

Anyway, I made it through graduate school. The job market was terrible in 1971 so I stayed on an additional year because I had an assistantship. The next year (1972) was not much better, but at least there were two good jobs, an academic position at Purdue and the PDI (Personnel Decisions, Inc.) opportunity. It turns out my competition at Purdue was Dan Ilgen so I came in second in that job search. Dr. McCormick was characteristically diplomatic about the decision: he said it was tweedle-dee-tweedle-dum between us (but Dan was the dee, and I was the dum). However, for the PDI job, I apparently received good recommendations from the faculty and was invited to Minneapolis for a job interview. The initial experience in Minneapolis was magical. Marv Dunnette greeted me at the airport. We proceeded to the baggage claim and, while waiting at the carousel, began talking about substantive research issues in person perception and personnel selection. This was heady stuff! I was talking to the authority in these areas on a collegial, researcher-to-researcher basis.

At any rate, the job interview went pretty well. I was somewhat intimidated by Lowell Hellervik and Bob Heckman. I saw them in action doing critical incidents workshops with patrol officers in Minneapolis, and they were so smooth and polished in running these workshops. But whatever, I got the job!

My job at PDI was primarily doing assessment centers, preparing for them, acting as assessor in the sessions, writing the reports, and delivering the feedback to assessees. These activities took up about three weeks of every month. That other week, I tried to get a research program going, as I was still very interested in research. I did get my dissertation published in OBHP and a study on behaviorally anchored rating scales in JAP, both in 1974. I also snuck in a couple of research studies as part of consulting projects with the United Services Automobile

Association and the U.S. Navy, and got reports of those studies published in JAP in 1975. I loved this publishing thing and was very motivated to keep it up.

Another very important event for me, to establish some identity at PDI as a researcher, came from a project we were doing with the Army Research Institute (Steve Motowidlo and Marv Dunnette had written the proposal). I worked with Rod Rosse, a brilliant statistician, on an analysis of job satisfaction, organizational commitment, and related variables. Rod didn't seem very interested in the way I was handling the analyses but was good about doing most of what I asked him to do. I had placed the data into a multi-construct, multi-method analysis framework. This approach resulted in considerable evidence for the convergent and discriminant validity of several of the measures, along with guidance as to the appropriateness of forming unit level scores for these constructs. When I presented the findings to the PDI group, including Marv and Rod, I received extremely positive feedback. Rod Rosse looked at me and blurted out, Oh, so that's what you were doing! I felt I had arrived as a researcher to be reckoned with in the PDI family.

Also in 1975, my career received a big boost when Marv asked Leaetta Hough and me to join him in starting up a new enterprise. The idea was to form a not-for-profit organization which was required to obtain a grant from the National Institute on Drug Abuse. In early 1975, I became the first paid employee of PDRI. We began work on the NIDA grant and on a job analysis project with George Hollenbeck at Merrill Lynch. Early in 1976, we brought on Norm Peterson and Janis Houston, who proved to be invaluable members of our team.

At this point, I was still very interested in performance measurement. I wrote a proposal to the Army Research Institute to conduct some basic research work on accuracy in performance ratings. Incredibly, we won the contract and began the first of our Borman tapes studies. The initial contract was for \$35,000, I believe. This was extremely important for me. It launched my research program, provided me considerable autonomy to do exactly what I wanted to be doing, and proved (to myself and to Marv) that I could obtain research funding for more basic research. I published the first paper based on the project in 1977 (OBHP) and the second in 1978 (JAP). These papers introduced the videotaped ratee paradigm, demonstrated the stability of individual differences in rater errors and rating accuracy, and showed that even under quite ideal rating conditions, interrater reliability and accuracy are far from perfect. The interrater reliability findings provided a kind of benchmark high-end estimate against which field research interrater reliability findings could be compared.

In 1978, I was able to secure a follow-on contract with ARI to study rating format and rater training effects on rater errors and rating accuracy, using the same videotaped ratee paradigm. This work resulted in a 1979 JAP article on format and training effects and an Applied Psychological Measurement article the same year on individual differences correlates of rating accuracy.

The year, 1978, was important to my career in another critical way. Marv was asked to do the Personnel Selection chapter for the 30th volume of the Annual Review of Psychology. He asked me to join him, and of course I said yes. The review and writing process was an intense learning experience. Marv and I divided the task down the middle, with each of us having the same number of topics. However, I learned so much in the editing process and in making decisions about what to include and what not to include. Marv's ideas and rationale around treatment of topics were scientifically compelling, but at the same time artistic from a literary perspective.

In 1980, we at PDRI received a visit that would significantly change our organization and my career. A vice-president from the Human Resource Research Organization (HumRRO) made essentially a cold call on Marv, Leaetta, Norm Peterson, and me to propose that we join them and the American Institutes for Research (AIR) in bidding on a soon-to-be-released request for proposal to do Project A. This was to be the largest scale selection and classification project in the U.S. military since the huge effort to select and classify soldiers in WWII. Teaming with other organizations was a new concept to us but I, especially, thought it was a great opportunity to win part of a fabulous project.

The proposal writing effort was, again, an intense learning experience. I was more enthusiastic about this project than my PDRI colleagues, so much of the PDRI-assigned writing I took on. Proposal writing and subsequently responding to ARI questions about our proposal was a lengthy process, with many HumRRO, AIR, and PDRI researchers involved. However, in the spirit of an autobiography, here are my personal critical incidents around the process.

First, at a large meeting at HumRRO, with all organizations represented, the discussion on Task 5, the job-specific performance measurement task, was centered on job knowledge and work sample tests as the criterion measures to propose. Hadn't these people ever heard of performance ratings?! I found myself arguing that supervisor and peer ratings on behavior-based scales were absolutely necessary to capture the typical, will-do performance domain. I'm sure others on the team (e.g., John Campbell) would have eventually ensured that ratings were included in the criterion mix, but I'll never forget the incident.

I ended up writing most of the Task 4 (Army-wide performance measurement) proposal and about 1/3 of the Task 5 proposal. This was a major effort over several months. I remember at one point spending a week in a cheap hotel in Alexandria, VA near HumRRO, leaving the HumRRO offices each day around midnight after writing all day.

At any rate, the proposal was submitted to ARI in Spring 1980, I believe, and some months later they sent us a long series of questions, asking for clarifications and requesting considerably more information on the topics of personnel selection and classification, performance measurement, various types of predictors, and so

on. By this time, my PDRI colleagues were growing weary of this proposal process, and so I ended up responding to about 20 of the 23 questions directed to PDRI. I will say that when we eventually won the contract, Marv, Norm, and others were extremely happy we persevered.

The proposal process took more than a year, and constant attention to it distracted from our other business. PDRI ended up taking salary cuts, executives about 40% down to 10%, I believe, for clerical. This was financially painful, but just when our prospects were getting quite bleak, ARI announced that our consortium had won what would prove to be an \$18M+ contract, overall \$4.5 M to PDRI.

As mentioned, this contract profoundly affected PDRI and my career. Getting to work on this high profile, research-rich project for nine years made a huge difference professionally to several of us, including Norm, Leaetta, Janis, Mary Ann Hanson, Glen Hallam, and others. The project also meant a lot to PDRI. We were now squarely in the upper echelon of major research firms. (The reader might notice I identify strongly with PDRI; in fact, it was, and continues to be, an incredibly important part of my life).

So, in the early 1980s, Project A was a constant for me, with some sort of research planning, instrument development, or analysis work under way. I was also doing projects with Norm Abrahams at the Navy Personnel Research and Development Center (NPRDC), with General Electric and Florida Power & Light on job analysis and selection issues, and with ARI on Army recruiting problems.

The most scientifically interesting event for me in the very early 1980s was the opportunity to design and test an assessment center targeted toward Army recruiters. We developed a center to predict performance in this job. The project was successful in that performance on the assessment exercises predicted performance in recruiter training. But more important from my perspective was that we demonstrated that consensus overall ratings correlated minimally with first impression evaluations and physical attractiveness ratings of these assessees. This suggested that assessors do not use relatively superficial initial information on assessees to make overall assessment ratings on assessment center participants. It spoke to the question Klimoski & Strickland (1977) addressed regarding what assessment center ratings were measuring.

Another very important event for me in the early 1980s was attending a conference on performance theory and measurement Frank Landy and Shelly Zedeck hosted (in 1981, I believe). I was thrilled to be asked because many of the top I-O psychologists were among the scheduled speakers. I was the first speaker, and I talked about personal construct theory, implicit personality theory, and the person-situation debate in personality. The idea was to integrate some of this thinking into performance appraisal research. After my talk, I remember vividly Dan Ilgen approaching me excitedly and telling me that he and Jack Feldman were going in a very similar direction, bringing cognitive, personality, and social psychology

concepts to bear on issues in performance ratings. This was heady stuff! I felt part of the movement to view performance appraisal as more of a person perception phenomenon, bringing much more psychology into the topic area. At the conference, I had wonderfully stimulating conversations with Dan, Frank, Kevin Murphy (who was still a graduate student), Bob Guion (who was quite skeptical about this approach, always was, right through his 1988 Annual Review chapter with Gibson), and others. Of course, Frank and Jim Farr had just published their classic 1980 Psychological Bulletin article on performance ratings, Jack's wonderful 1981 paper was about to come out, and Dan and Jack were working on their great chapter for the Cummings and Staw JAI series. And several others were committed to the movement, including Chris Banks, Angelo De Nisi, and others. So a number of I-O psychologists were thinking about performance appraisal in these new and exciting ways. I was just happy to be a player in all of this.

Beginning with our Project A subcontract, PDRI began to thrive again. We grew to as many as 40 staff members. Wonderful think-tank like atmosphere, so much good (great) applied research happening. Project A continued apace. I was most involved with development and testing of army-wide rating scales, a set of dimensions that was to serve as relevant criteria for any enlisted job (first term) in the U.S. Army. I didn't realize it fully at the time, but this work suggested a division of the criterion space into technical proficiency and non-technical proficiency elements of job performance, and that the non-technical proficiency part could be for the most part generalized to any job. The idea of contextual performance that Steve and I later became interested in was beginning to emerge. In fact, Steve and I developed a model of soldier effectiveness very early in Project A (1981 or so) that contained all of the elements that subsequently emerged as our 1993 taxonomy of contextual performance. I also led a team of PDRI researchers who developed MOS-specific (i.e., job specific) behavior based rating scales for nine Army jobs. PDRI's main contribution was led by Norm Peterson. A group of PDRIers developed the extensive predictor battery used very successfully in the validation research.

On another front, in 1984 Milt Hakel gave me a wonderful opportunity to get involved in SIOP. He appointed me as Chair of the Scientific Affairs Committee during his presidential year. I don't know exactly why this happened, but I did not take my assignment seriously and, as a result, did a very poor job during my year on the Executive Committee. I missed a couple of the meetings, and, in general, greatly underappreciated Milt's effort to give me a leadership opportunity in SIOP. Thankfully, I was given one more opportunity about seven years later, and this time took the assignment much more to heart.

In 1985, Rich Klimoski asked me to join the Ohio State I-O faculty as a visitor while Milt was away on sabbatical. Probably to his surprise, I accepted his offer and took a leave of absence from PDRI. I taught graduate classes in the I/O program for two quarters and found the experience a whole lot of fun. Rich, Bob Vance, and Bob Billings were great colleagues, but I especially enjoyed working with the graduate students. The group at that time was very talented, with several going on to

distinguished careers as academics or practitioners. The ones I remember are Adrienne Colella, Tom Becker, Laura Koppes, Bob Jones, Martha Sanders, Larry Inks, Lisa Scherer, and Scott Martin.

In 1990, a job opportunity at the University of South Florida (USF) intrigued me. I think I always admired Marv's moving back and forth from PDRI to the university, and to an extent coveted his dual professional life. Also, I had been doing essentially the same thing for 15 years at PDRI writing proposals, doing project work, managing other researchers, writing technical reports, and trying to publish on the side. Time for a change.

Lou Penner, a social psychologist and Chair of the Department of Psychology at USF, did a great job of recruiting me, and I joined the USF faculty in August 1990. I still worked on PDRI projects and retained my position as President (since 1982), but I really threw myself into this new academic role. I certainly picked up my publishing pace, with more time to write. The performance criterion chapter for the Handbook was completed in 1990. But two specific themes began to drive my research program.

One was inspired by Jack Hunter's 1983 chapter in the Landy, Zedeck, & Cleveland book (from the 1981 conference). The general idea was to identify factors or cues that supervisors in organizations use when making global overall performance, overall effectiveness, or overall worth-to-the-organization judgments. This work resulted in a couple of Journal of Applied Psychology papers and ended up tying in nicely with the other theme.

As mentioned, Steve Motowidlo and I had worked with a model of soldier effectiveness (and Steve had published a 1986 paper with Art Brief on prosocial organizational behavior). We brought all of this together for a chapter on contextual performance that we wrote for Neal Schmitt's and my 1993 Frontiers Series book on personnel selection. The chapter laid out (what we thought at least) a parsimonious model of contextual performance, summarizing work on organizational citizenship behavior (Dennis Organ's and colleagues' work), Steve's and Art's prosocial organizational behavior concept, and Steve's and my model of soldier effectiveness. How this fit with my first theme was that Steve and I, in separate papers, found that supervisors tend to weight contextual performance about as highly as task performance when they make overall performance judgments. Others have found pretty much the same result.

At any rate, research on and interest in contextual performance really took off. Steve gave many talks on the topic at U.S. universities and in Holland and Poland. I made similar presentations at business schools and psychology departments in the U.S., as well as in Australia and China. In addition, it became a popular dissertation topic.

My read on all this is many times we I-O psychology researchers (maybe all scientists) don't get enough credit for our own work. People forget we came up with the idea, very good insights on our own part don't find their way into published reports because of the journal review process, and so on. In this case, I believe we (especially I) have received too much credit. A lot of the basic notions underlying our task/contextual performance distinction built on others' work. Maybe the reason the topic became so hot is that Steve and I take a more I-O psychology rather than OB approach in this area. We tie the constructs into traditional I-O topics such as selection and performance appraisal. Whatever. I'm sure at some point interest in the topic will wane.

Regarding PDRI, beginning in 1993, I began to get more involved in the organization again. In fact, we decided to open a Tampa office in 1994, and Jerry Hedge (then Chief Operating Officer), Mary Ann Hanson (an excellent researcher and good friend), and Ken Bruskiwicz (a young Research Associate) moved to Tampa to help open and staff the office. We convinced Elaine Pulakos to re-join PDRI in 1995, and she opened a Washington DC office, quickly hiring Gary Carter (also a former PDRIer).

The Tampa office has been great for me. We have had between four and eight USF graduate students working with us on projects at any one time. Accordingly, it has been very good for USF as well. Our students get to do interesting I/O research and practice, and PDRI funds a fair percentage of our graduate student population. Pretty ideal set up for me. I became the Chief Executive Officer in 1996, as Marv wound down his PDRI activities. We named Jerry President and Chief Operating Officer, also in 1996, and he returned to Minneapolis, which made more sense organizationally.

Back to SIOP. I became active again in the organization when I was elected to the American Psychological Association's Council of Representatives in 1991. I can't say I enjoyed my three year stint in Council, but I certainly liked being part of the SIOP Executive Committee. What wonderful, dedicated volunteers for the Society. I remembered clearly my performance (or lack thereof) in 1984, and tried to be much more engaged this time.

I was astonished in 1993 to be nominated for president-elect of SIOP. Paul Sackett won the election, but the next year I was nominated again and this time actually won. This was incredible! I mean literally! I loved Angelo De Nisi's first presidential column in TIP (1999) he said something like I asked for a recount of the ballots because this just couldn't be. I felt the same way about my being elected. I remember a 1990 or so lunch with Herb Meyer and Mike Coovert to honor Mike's near miss of the McCormick Early Career Award. Mike, in his characteristically kind and diplomatic manner, said he was honored to have as colleagues a former SIOP president and a future one. I distinctly recall thinking there was no way! I couldn't even imagine being President. Now I was going to be.



The thrill of being elected was the main emotion surrounding the event, but then there was the talk. What to do about a topic? The presidential address was an issue very soon after Paul Sackett called with the good news.

In a way, I copped out with my topic. I was uncomfortable thinking about doing a broad viewpoint piece about testing policies, the past or future of I-O psychology, or similar topics. I'm not very good at pontificating. So, my idea was to report on the empirical results of a survey of SIOP members on types of practice work they do. The goal was to be as inclusive as possible across academic and practitioner SIOPers. My thesis going into this was most academics consult at least to a limited extent, and our practice work is a unifying theme to our profession. About the only thing wrong with SIOP in 1993 (in my view) was that there was some sentiment that academics and practitioners had very different interests and agendas and that, at some level, we were two different societies. I thought I might be a person (because I had been a practitioner for 18 years before also taking on an academic role) to try to help bring these groups together by pointing out empirically and rhetorically that we (almost) all have some practice interests. At any rate, the notion was to celebrate the practice work of practitioners and that of academics doing consulting work. Again, I wanted the talk to be inclusive, with both scientists and practitioners comfortable with the theme.

So, with the help of a talented graduate student, Gena Cox, the survey got designed, returns came in, data were analyzed, and I prepared the presentation. We were also working on the Department of Labors Occupational Information Network (O\*NET) project in 1993-94, so Andy Rose of the American Institutes for Research helped develop a demo to be inserted in the talk to represent a futuristic job-person match system, a model of how I-O psychology might help the unemployed find occupations that fit their capabilities, students to learn about the world of work, and employers to recruit qualified employees.

The week or so before, and the morning of the conference, I found I was not really that nervous. The talk was totally prepared, I had rehearsed it (privately) three or four times, and if all else failed, I could read the thing! On the other hand, I was fully cognizant that this was by far the most important hour of my professional life. After the talk, I thanked Gena for all her help, Mary Ann for her support and help, and then Marv for helping me so much with my career. These thanks were heartfelt, but particularly mention of Marv's help became very emotional and I had a semi-breakdown (a few choked words). I was so happy to be done with my presidential year and ready to turn back to my other activities and challenges. But I also felt tremendous pride in our Society and thankful I had the opportunity to actually lead the Society for that year. It will always be the highest point of my professional career.

My remaining goals as of late 1999 are: (1) to have PDRI be the best research group in applied I-O psychology research (we believe we already are pretty much the best but constantly improving is important); (2) to help bring USF into the top

three or four graduate programs in I-O psychology; and (3) to continue personally doing research that furthers the science of I-O psychology.

Because serving SIOP as president was so important to me, I will end my professional autobiography here. As I just mentioned, I hope to continue to contribute substantively to the literature, PDRI, and the USF I-O graduate program, but personally speaking, I feel no pressure to attain anything more professionally. That's a very good feeling.