An Unmanaged Pursuit of Management
The Autobiography
of
Lyman W. Porter

Throughout my life I've always had trouble deciding what I really wanted to do when I grow up. I still do. As far back as high school, or even before, I always envied my friends who seemed to know exactly what they were going to do in their life's work when they grew up: One of them (Lou Doyle) very early wanted to be a doctor (and he is), one (Joan Samson) wanted to be a teacher (and she has been for most of her adult life), one (Kelley Carr) wanted to be a dentist (and still is), one (Bob Ryder) wanted to be an engineer (and has always been one since graduation from college), and so on. Alas, I was never blessed with any certainty about what I wanted to be partly because I was interested in many different things but a master of absolutely none, and partly because I simply hated to make a choice and close out other alternatives. Hence, as the title of this story indicates, my life has been composed largely of a series or string of unplanned, unanticipated, and frequently extremely fortuitous events. Consequently, writing about it is an exercise in retrospective sense making in the truest sense of that concept.

As I describe the various meanderings of my life in the pages that follow, I think that several themes will emerge. (Even if they don't, I've decided to make the data conform to what I think are those themes!) First and foremost is the fact that I am a product of a middle-class, middle-west upbringing. Even though all of my professional years have been spent in California, my roots and basic character formation were anchored in my first eighteen years growing up in a college town in Indiana. Secondly, since I've always felt I was never endowed with any particularly strong talents in specific areas of endeavors, whatever overall talents I possess have, unfortunately, been spread rather thinly over several areas I have had to make up for this fact by sheer dogged persistence: Persistence in the pursuit of certain short-term goals or objectives that I've set for myself from time to time. (Perhaps this explains one of my later deep interests, namely, motivation "what energizes, directs, and [especially] sustains behavior. For certain, it also explains my firm belief that performance is a function of ability times motivation.) Even though I've tended to have this stubborn streak of persistence, I've also had, as contradictory as it sounds, an urge to maximize variety in my life. This in turn has sometimes led to a desire for change for the sake of change. So, I guess I would have to say that this desire for variety, which has led to a certain restlessness at some periods, is a third theme that pops up at various places in this account. A fourth thread, as I look back on the past five or so decades, has been a constant association with education. Despite the best efforts of my parents, this association was never really planned or anticipated, but it has been with me since the age of five. Why it has turned out that I have been a sort of perpetual student and have had my life so intertwined the educational process is something of a mystery to me, but a fact nevertheless. Another constant has been a strong sense of enjoyment of working with others. Although at any given point in time I can be as independent and lone-wolfish as the next person, my natural tendency is to want to work together with colleagues on some collective task. I know that I think better when I am actively engaged in intellectual give-and-take with others rather isolating myself for any extended period. Furthermore, and important for me, I find it just a lot more fun to strive to accomplish something that represents a team or collaborative achievement. The final theme that has dominated my life over the years has been luck, sheer,

unadulterated good luck. I won't elaborate on that motif here, but I will identify in various later places in this treatise those occurrences of luck that have contributed tremendously to my life and my professional career.

So much for a prologue. Now, on to what happened and, hopefully, if I can muster a modest degree of insight, why. As I look back, my life so far seems to have been divided like Caesar's Gaul into three parts: the formative years, the Berkeley years, and the Irvine years.

The Formative Years

As I have indicated, my early life was dominated by the fact that I was born and grew up in a small-to-medium size college town in the heart of the middle-west. That town was West Lafayette, Indiana, on the banks of the Wabash. I was the last of three boys to be born to my parents. (As my next older brother was some eleven years older than me, I later as a teenager remarked to my father that I must have been something of an after-thought; in his usual candid manner he replied: You were.)

My early childhood years in the 1930s in West Lafayette consisted of the typical midwest boyhood activities, mostly involving some kind of sports-type activity or games with other neighborhood friends after school and on week-ends. Come rain or shine, we were outdoor playing ball. The major event of my early childhood was spending half a year in London with my parents in the latter half of 1938, less than 12 months before World War II started. I was eight at the time, just old enough to realize the significance of the various historical landmarks we visited throughout Great Britain and, for a month, on the Continent. That total experience of being abroad and living and traveling in a quite different environment than the U.S. middle west had a profound influence on me. Ever since, I have been (a) a confirmed internationalist, and (b) an avid travel enthusiast.

By the time I had reached high school my four prominent interests were girls, sports, journalism and school work, in that order. Although my athletic talents were modest at best, my proudest moments in high school were earning letters in football and track. (In the case of the latter sport, I ran the 440-yard dash and was lead-off on the mile relay team. It seemed that before each race I was always complaining to the coach that I had some minor ailment twisted ankle, swollen toe, cold, etc. because he wrote in my graduation-year yearbook: Porter, if you ever told me you were totally healthy before a race, I would have been really worried!) My journalistic interests had been kindled by my father, who was a college professor in biology. These interests led to working my way up to be the editor of my school paper in my senior year in high school. They also led to a then-hobby that probably no one else in the entire country had: since 10 or so I had acquired a collection of one or more different newspapers from each of the cities we had visited on vacations or other trips. Since my parents liked to travel, that meant that I had acquired a large stack of papers from around the U.S. by the time I had graduated from high school. (To illustrate the extent of the traveling we did as a family when I was growing up and my own travel in my first several years in college, by the time I was 21 I had visited every one of the then-48 states.) My academic studies in high school had progressed rather well, such that I was eager to go on to college when I graduated in 1948.

Given my strong interests in journalism at the time, and also given that I did not want to live at home while I was going to college (too confining), I chose Northwestern University even though most of my high school friends were electing to stay in West Lafayette and attend Purdue. My choice of Northwestern (N.U., as it was referred to by students) turned out to be a great decision on my part,

looking back in retrospect. It was the right distance from home, the education I received was first-rate, and I made a lot of good friends during my four years in Evanston. Although my initial reason for deciding to go to N.U. was because of its highly-rated Medill School of Journalism, I decided in my second year of college that I didn't really want to have to write every day for a living. (How ironic, as it has turned out, since my ultimate career involved writing almost every day for a living.) I switched to psychology at the beginning of my junior year, but not before the two years of being in the journalism school with its intensive writing exercises and even more intensive feedback on that writing had considerably improved and sharpened my composition skills. (This had not been achieved without a certain amount of pain, since my weekly required papers in J-school often came back with a great amount of blood [red ink] all over them.)

By my senior year at Northwestern I was reluctantly having to face up to what I was going to do when I graduated. (Gee, don't know, was my typical response when asked the question by my friends.) Early in that year, my chief undergraduate mentor in psychology and the very model of what a professor should be Benton J. Underwood, called me aside and said he wanted to talk to me about applying for graduate school in psychology. Until that moment, I had not really thought about the possibility of entering graduate school and becoming a professor myself, even though that was my father's own occupation. Underwood told me in no uncertain terms that I should only apply to certain schools (his personal list of the best psychology programs at that time) and not others. Thus, I applied to Yale, Stanford, Wisconsin and Indiana, and was accepted by all four (no doubt because of Underwood's very helpful letters of recommendation). Even as far back as those days I had always had a desire to spend some time in California, so I was strongly motivated toward attending Stanford. However, I heard from Yale first and, since the acceptance was coupled with the most generous of the financial support packages, I decided to go there. Again, it turned out to be a wise choice, but I am sure that I would have been happy at any of the other three schools, too. Also, during my senior year at N.U., I took a psychology honors class from Carl Duncan and for my class project conducted a verbal learning experiment. Out of this project, and with great assistance from Prof. Duncan, came a co-authored publication: "Negative transfer in verbal learning," that was subsequently accepted for publication in the Journal of Applied Psychology. This was my first-ever publication, and to say that it had a positive motivating effect on my desire to become, if possible, a scholar, would be a considerable understatement. (My concurrent thought: "Well, at least I have one publication but, will there ever be a second one?)

In the fall of 1952, I arrived in New Haven. My first challenge was not, however, my first-year, Ph.D.-level classes in psychology. Rather, it was the necessity of passing reading exams in two foreign languages, a (then) Yale Graduate School requirement to be met no later than the end of the first year. Fortunately, I had taken enough German at Northwestern to pass that exam without too much difficulty; French, however, was another matter, but by a lot of cramming during the summer preceding my entry I was able to squeeze a pass out of the exam taken early in my first semester. What a relief, since I had visions of spending my whole first year at Yale bogged down in a miasma of French language courses while I was struggling to get started on my psychology graduate studies. At any rate, I escaped that fate and was able to concentrate on matters at hand relating to psychology.

I had gone to Yale on my (and the Yale Psychology Departments) assumption that I would be concentrating in the area of experimental psychology. But, just as in my undergraduate years, my interests began to shift once again. While I found my courses and my research assistantship work for Fred Sheffield and Neal Miller to be both interesting and intellectually stimulating and challenging, I

came to the conclusion that if I continued to pursue that subfield of psychology, experimental psychology, I would end up spending too much of my time in laboratories, frequently isolated from the real world of other people. Regrettably for me, I did not come to this conclusion until near the end of the third of my (eventual) four years at Yale. What to do? I was too far along to shift totally my subfield within the Yale program, so I ended up doing a human learning experiment involving the administration of an adversive stimulus -- a very mild, tingling electric shock administered to the wrist to study its effect on recall of verbal materials. This study was designed with great assistance and encouragement of Fred Sheffield, to whom I have always been grateful for his help in this regard. Meanwhile I had been serving as a research assistant in the animal learning laboratories of the widely-renowned psychologist Neal Miller; from him (through both the assistantship and his courses that I took) I learned the fundamentals of the scientific method in a way that has stood me in good stead ever since. Nevertheless, I still faced the issue, in my fourth year at Yale, of my gradually changing interests and what to do about them. I turned to one of Yale's most famous psychologists of that or any other time, Carl Hovland, and asked him for his advice since he was highly familiar with experimental psychology but more oriented toward social and industrial psychology. I told him of my possible interest in but total lack of knowledge of the latter field, so he suggested a reading course with an adjunct professor (Paul Burnham). Thus somewhat tentatively launched in what basically would be my future field of professional endeavor, I proceeded to read a series of basic texts in industrial psychology throughout that final year at Yale. It was not exactly an in-depth introduction to the field, but it at least got me started.

As an aside, and to demonstrate what I said at the beginning of this autobiographical piece about my inability to decide what I really wanted to do when I grow up, I had in my first year at Yale flirted with the possibility of transferring from the psychology program to either the Harvard MBA program, about which I had learned from a friend, or to the Yale Law School. Although I seriously considered both possibilities, especially the one closest at hand (Yale Law School), I never took the ultimate step of applying. To this day, I think that I could have been just as happy in my professional life if I had in fact carried through with either possibility (on the rather vane assumption that I would have been accepted). I have no regrets about passing up these could-have-beens, but likewise never have I subsequently devalued them as once-viable options.

As I approached the latter part of my last year at Yale in the spring of 1956, I was again faced with the somewhat fearsome problem of what I was going to do after I graduated, a problem similar to the one I had faced four years previously when I was preparing to finish up at Northwestern. The only difference was that this time it merely involved the question of what was I going to do for a living. With the end, so to speak, rapidly approaching, I was the beneficial recipient of one of the many lucky breaks I have received over the years: A highly desirable job possibility appeared almost out of thin air around March of that year. Edwin Ghiselli, one of the country's premier industrial psychologists, and at that time also chairman of the psychology department at UC Berkeley, wrote to Hovland saying that Berkeley had a faculty opening in the area of industrial-social psychology and, furthermore, they wanted someone who was broadly-trained in psychology in general, and not someone who was narrowly trained only in industrial psychology. Since I more than fit the latter part of that job specification and to a degree fit the first part, Hovland wrote back and suggested me as a possibility for the position. Within a few weeks I was being interviewed by Ghiselli who was on the East coast on other business, and a few weeks after that received a telegram that would forever change the course of my life. It said: Department votes unanimously to offer you position as Instructor in industrial-social psychology at salary of $4296.

Congratulations. Please reply within a week. It took me about five seconds to decide on my response, but I coolly waited several days before sending my affirmative answer. At least, for a couple of years I would not have to worry about how I would obtain a paycheck, and I was going to a part of the country, California, that had always had an attraction for me. Little did I know, of course, that I would still be in California some 35 years later. There was only one small problem: I would be teaching courses and doing research in an area in I which I myself had never had a formal course!

The Berkeley Years

Before arriving at Berkeley in August of 1956, I had spent the summer working for Bell Laboratories in New Jersey and New York City on a project involving interviews of managerial and technical personnel regarding sources of job satisfaction and dissatisfaction. This project had been arranged through Hovland, who at that time was a high-level consultant to AT&T. It turned out to be an excellent experience for me both with respect to honing my interviewing skills but also for getting me acclimated to real organizational situations. It was a perfect transition phase between the conclusion of my Yale years and the beginning of my Berkeley years.

I finished the Bell Labs project early in August and headed west. On the way I stopped in West Lafayette to buy a new car which I used to drive to California. Since this was before the days of the Interstate system of highways, it took me five days, during which time the only persons I spoke with were waitresses in diners and motel clerks. I was glad to see Berkeley, but was Berkeley equally glad to see me? As it turned out, I felt quickly accepted by my new colleagues on the faculty in the psychology department and forthwith turned my attention to preparing my first set of lecture notes for the first course I ever taught anywhere, Industrial-Social Psychology. Since, as noted, I had never taken a course in this area, I had no available notes of my own. What to do? I consulted with my other chief colleague in addition to Ed Ghiselli in the industrial psychology area, namely, Mason Haire. Since he was the only person on the faculty who had previously taught the course, he seemed liked the logical source of good notes. Unfortunately, Mason told me that he lectured only from a very sparse set of notes, since that was his preferred style. Mason was excellent with this highly extemporaneous approach, but I knew that that would never do for me, at least in the first few years. Although Haire had no available set of notes from which I could begin to build my own, he directed me to a senior psychology major, Jeff Keppel, who had been a student in his (Haire's) most recent class on this topic and whom Haire knew had taken comprehensive notes. Luckily for me, Keppel had indeed taken detailed notes which proved to be extremely useful as I struggled in my preparation for my first few classes. (Jeff and I became good friends; the following year he went on to graduate school, subsequently going on to a distinguished career as an experimental psychologist and expert in verbal learning and serving as a long-time faculty member at Berkeley.) With the help of Jeff's notes and my own reading of as many books in the area as possible over the summer and that fall, I was able to keep about one lecture ahead of where the students were in their reading. Two good things happened as a result of teaching that first class: one, I survived the course and even received decent student evaluations of my frosh-like teaching; and, two, I met a student, Andy Moreland, with whom I became a good friend and through whom I was later (the next summer) to meet the girl who was to become my wife: Meredith Moeller. (For the record, Andy went on to medical school and for the past several decades has been an anesthesiologist in Santa Cruz.)

Almost my total first year existence at Berkeley was taken up with preparing for my teaching assignments for obvious reasons. My need to feel at least acceptably prepared for each class, in the

context of my relative lack of familiarity with field, meant that little time was left over to think about research let alone actually to carry out a research project. Once again, fortune smiled on me in the form of that prince of colleagues, Ed Ghiselli. Toward the latter half of my first Berkeley year Ed dropped around my office and said that he had some data that he had collected recently that he hadn't yet had a chance to analyze or write up. Would I be interested, he asked, in looking at the raw data and see if there was anything there, and, if so, to do an initial draft of a co-authored manuscript? It was Ed's gentle way of offering to help get me started on doing scholarly work in the industrial-social psychology field. Of course, I accepted his offer with alacrity and soon I had analyzed the data and prepared a first draft of a manuscript that became my first publication in my newly-adopted field: The Self-perceptions of Top and Middle Management Personnel, by Porter and Ghiselli and published (in 1958) in Personnel Psychology. (By that time, I already had five previous journal publications from my undergraduate and graduate work, but all of them were related to issues in experimental psychology.) That article with Ghiselli helped shape my early interests in focusing in on management and managers as the object of my research efforts. I knew that my lack of formal training in industrial psychology and related areas such as labor relations would probably preclude me from ever gaining any sort of high-level expertise with respect to rank-and-file workers, especially those in unionized work situations; hence, my decision to concentrate most of my attention on management and managers. The right decision for me, as I look back with hindsight.

My early years at Berkeley progressed well largely because of the help and encouragement I received from Ghiselli and Haire. They became close, personal friends and served, in effect, as my mentors. If I have achieved any subsequent scholarly success in fields related to industrial psychology, it is directly attributable to the tremendous influence these two had on me at the beginning of my career. They literally taught me the field, helped me formulate my views of issues in the field, introduced me to major figures in the field, and provided guidance that could not be equaled anywhere. No junior faculty member could ever be as fortunate as to have two senior colleagues of the caliber of Ed Ghiselli and Mason Haire.

As my first year at Berkeley drew to a close, I was well into several incipient and small-scale research projects on which I hoped to make progress in the next few months, especially before I had to start teaching again in the second session of summer school in 1957. That particular summer teaching period turned out to be fateful for me because my former student and now friend, Andy Moreland, introduced me to a particular group of girls that were in the Berkeley environs that summer. One of them, the most attractive (of course), was the one that turned out to be the love of my life and (about 10 months later) my wife, the aforementioned Meredith Moeller. Fortunately, she had not been around to attract me, and distract me, during my prior first year, because otherwise I never would have been able to prepare my first-time lectures, read voraciously in my field to catch-up, and try to launch some research efforts. There would not have been enough time. (Others could probably do all of those things simultaneously and still have time for a lively social life, but I'm not that agile.) Since, however, the chain of events that led to matrimony started after my first year, I was better able to juggle the diverse set of activities that occupied my second year at Berkeley. Meredith, first as fiancé and later as newly-married wife, was very understanding as she has continued to be for the 33-plus years since then.

Because of the fact that I had arrived in the psychology department at Berkeley as both the newest member of the faculty and also the youngest (at 26), I had one great advantage: no one really expected anything from me for a while. Consequently, whenever I did something of modest note, such as making

a half-way cogent argument in a faculty meeting or getting an article accepted for publication, everyone was pleasantly surprised. Unfortunately, however, one can only be the newest and youngest for a very short period of time. (I remember with dismay when an even younger faculty member was hired during my second year.) This meant that if I wanted to have the opportunity to stay at Berkeley it would be wise to keep up an aggressive program of research. This was what I wanted to do, and intended to do, anyway, so it was no particular burden. (To be frank, during my Instructor and Assistant Professor years at UCB I never thought much about tenure. My view was that I was going to do what I was going to do, and if that was enough, then fine. If not, so be it. It would hurt my pride, no doubt; but I figured, perhaps somewhat cavalierly, that there were plenty of other places to work, even if not with the prestige of Berkeley, and I believed that I could be happy professionally in any number of institutions. As things turned out, I never actually had to test that belief.) The one disadvantage of being the newest kid on the block was that the several Ph.D. students that were already working on their degrees in industrial psychology in the department were not exactly, and automatically, desirous of working with me. They were very friendly, but they correctly sensed that I could contribute relatively little to their knowledge of the field. In fact, when I arrived in 1956, all of the doctoral students obviously were way ahead of me in this regard. Given this situation, I turned to working with a couple of new, entering students after I had been there for a couple of years. Fortunately, they were not as aware of my relative newness to the field (nor to the faculty). Thus, due to our reciprocal naiveté, as it were, I was able to begin my first intensive work with doctoral students, a challenge and pleasure that I have enjoyed ever since.

Following my first article in the organizational (industrial psychology) field with Ghiselli, I had proceeded over the next couple of years to write several articles (#s 7, 9, 10, 11 and 12 on the attached bibliography) that utilized the extensive data set that had been collected originally by Ghiselli for other purposes relating to the development of his Description Inventory. My interests were in understanding the structure of organizations, particularly the management sector, and how that structure was related to the types of individuals who were most likely to be found in the different component parts of an organization. This in turn led me to thinking about how job attitudes might vary throughout the typical organizational structure and the possible implications of those patterns of attitudes. Due to the fortuitous circumstances of the fact that the Ford Foundation had taken an interest in business education issues relating to it, I was able, in 1959, to obtain a Ford Foundation Faculty Research Fellowship. This provided a relief from teaching responsibilities for the 1959-60 academic year and furthermore allowed me to employ a graduate research assistant (R.A.), since the department of psychology did not have the resources to provide such funds to junior faculty. With these funds, I hired my first R.A., Mildred Henry (now the president of a small college in the San Francisco Bay Area). With Millie's willing and tireless help, I was able to develop a need satisfaction instrument based on (but not testing) Maslow's need hierarchy theory. Concurrently, and with the help of contacts provided by Mason Haire, I was able to persuade the American Management Association (AMA) to help sponsor an extensive study of the job attitudes, including need satisfactions, of a large nationwide sample of managers and executives. This was another piece of good luck, because this one single, but very large, data collection effort provided the bases for a set of six future articles, all published over a several year period in the Journal of Applied Psychology, as well as a summary monograph (#28) Organizational Patterns of Managerial Job Attitudes, published in 1964 by AMA. In the later stages of this particular project, I was greatly assisted by another graduate student R.A., I. R. (Bob) Andrews. Meanwhile, also during this period, I worked on several other smaller-scale projects relating to work group characteristics and behavior with several other graduate students including Tom Lodahl (later to be the Editor of

Administrative Science Quarterly). Tom (who was really Mason Haires student but whose dissertation I directed during Haires sabbatical leave) and I had a lot of fun working together designing and implementing a research study that was carried out on small work teams at the United Airlines maintenance base in South San Francisco just prior to the introduction of jet engines on commercial aircraft (which goes to show how old Tom is, if not myself) .

One student who entered Berkeley's Ph.D. program in industrial psychology in the late 1950s (1959, to be exact), but who only stayed one year before transferring to Indiana University, was one Larry L. Cummings. Through a combination of totally inadvertent circumstances, Larry and I had little contact with each other during his Berkeley year -- we were both total unknowns at that time and only got to be very close and personal friends around the time he was finishing his doctoral work at I.U. The following year (1960) at Berkeley, however, another new Ph.D. student entered the industrial psych program. This person I also did not get to know during his first semester or so, but from that time on we formed an intense partnership -- in the best sense of that word -- that persisted for many years after he received his degree in 1964. That student was Edward E. Lawler, III. By the beginning of Ed's second year in the program we found that we had strong common interests in a number of intriguing intellectual issues in our field. Furthermore, we also found that we had a number of other common non-work interests, particularly sports (I'll never forget, for example, watching Loyola of Chicago win the 1963 NCAA basketball championship game on TV in Ed's recreation room in his home) and bridge, among others. We first published together in 1963 in an article (#21) on Perceptions Regarding Management Compensation, and two years later our third co-authored paper (#30) was one that was published in the Psychological Bulletin -- it had been a joyous moment when we two relative rookies received the acceptance letter for that article from the editor on Properties of Organizational Structure in Relation to Job Attitudes and Job Behavior. That literature review article gained a certain amount of subsequent visibility and was an important early career boost for both of us.

It was during Ed's last year or so at Berkeley that we began to formulate our ideas regarding the variables that act together to determine motivation and job performance. Our initial thinking was mightily influenced by the publication of Victor Vrooms classic book, Work and Motivation, in the spring of 1964, because we immediately saw that Vic's explication of an expectancy theory approach had great applicability to our own data and ideas. We proceeded to spend much of the summer of in many discussions about how we might extend the theory in ways that made sense to us and about which we had collected previous data that could be analyzed from that perspective. Thus was born the so-called Porter-Lawler model of motivation. It took us another two years or so to develop the model fully and to test with our data set some of the relationships between and among its variables, but the end result was our jointly authored book, Managerial Attitudes and Performance, published by Irwin in 1968. Our two names have been linked ever since, though Ed, of course, has gone on to establish a highly prominent and visible position in our field -- quite independent of our early joint collaborations -- through his numerous and extremely valuable contributions. Regrettably, I don't have the space to go on in more detail about Ed's many abilities and attributes, but I will mention one that stands out for me, and one that has a certain relevance to our motivational model: Ed Lawler, far and away, is better at converting effort to outstanding performance than anyone I have ever met in my entire life.

At this point I need to backtrack to note one other major is project on which I worked during my Berkeley years (in the period between 1956 and 1967). In 1960, primarily through Mason Haire's initiative, the threesome of Haire, Ghiselli and Porter submitted a research proposal to the Ford

Foundation to undertake a cross-national study of managerial job attitudes. The proposal was funded, and we proceeded to carry out the study in 1961 and 62. Data were collected in situ in various countries by ourselves singly or jointly and with the help of several other colleagues around the world, most notably Frank Heller (who, at that time, was working in Chile for an agency of the United Nations). Not the least of the benefits of this project was the fact that each of the three principal investigators got to live abroad for a period during that time. In my case, my wife and I decided to spend a six-month period in Copenhagen, where I could concentrate on data collection activities in Scandinavia and Germany. With our then-two-year-old daughter, we lived in a penthouse suite atop a modern apartment building on the outskirts of Copenhagen that was rented for the grand sum of $108 a month. We never had it so good, before or since. The experience provided a chance to learn about another culture as well as to participate in an active way in our research project. As luck (again) would have it, on one of my data forays to Germany, I happened to arrive in Berlin two days after the East German government began the construction of the wall in August of 1961. Watching the wall actually being built brick by brick and block by city block has made for an unforgettable experience. (The fact that it suddenly started to come down on November 9, 1989, exactly on my wife's birthday, added a coincidental conclusion to my personal Berlin Wall saga begun some 28 years earlier.) By the finish of our data gathering efforts we had obtained extensive questionnaire responses from more than 2,000 managers from 14 countries in Europe, Asia and South America. Our findings were published in book form by Wiley in 1966, the title: Managerial Thinking: An International Study. While some of our data analyses could be considered somewhat primitive by today's standards, many of our findings and conclusions have in fact held up rather well for these past 25 or so years. A number of other large-scale cross-national studies of managerial attitudes have been conducted since the publication of our book, of course, but we took a small amount of pride in being one of the earliest group of researchers to carry out this type of study in our field. Besides, we had a lot of enjoyment in just doing the study and working on it together.

Before closing out the account of my years at Berkeley, a few other facets that were especially memorable for me during that 11-year period might be worth noting. One was my rejection for membership in (then) Division 14 -- the Division of Industrial Psychology -- of the American Psychological Association. (Division 14 later was re-named the Division of Industrial-Organizational Psychology and even later came to be called by its current name, the Society of Industrial-Organizational Psychology.) I had applied for membership in 1959, but had received a polite, but firm, note from the Membership Committee saying that my application had been denied because there was insufficient evidence that I was committed to this particular field (despite the fact that I had already had two articles published in the field, three more in press, and had been recommended by two senior members of the Division, Ghiselli and Tom Harrell of Stanford). I did get admitted two years later, and I must say that I permitted myself a small measure of self-satisfaction when I later was elected president of the Division in 1975, sixteen years and some 40 journal articles and two books after having been turned down for not demonstrating that I really was actively involved in this field. (This trail of events might help to explain some of my interest in studying motivated behavior.)

A second experience, of a different type, was serving during the early 1960s, as the Faculty Representative to the student government. My confidence in my effectiveness in this role was somewhat shaken by the eruption of the Free Speech Movement in the fall of 1964, and the subsequent arrest of several hundred students for invading Sproul Hall and effectively closing down the university for several days. Aside from that minor incident that affected the Berkeley campus for the next 10 years

or so, I found the experience of being in the faculty representative role to be fascinating and even enjoyable at times. It cemented my belief that university service is an integral part of any faculty member's basic set of responsibilities.

Still another, and clearly more consistently enjoyable, vignette from those years was the series of touch football games in which I participated on Sunday afternoons every few weeks each fall. For the record, some of the other players included Stan Nealey (now at the Battelle Institute in Seattle), Ray Miles (now professor and former dean of the business school at Berkeley) and Ed Lawler. We were all young and vigorous, but I was a 5-10 pygmy among those 6-3-or-so tall giants. I made up for lack of height with what I considered blazing speed, but Ray always used to say of me: He's small but slow. (I think he was kidding, but maybe he wasn't.)

Last but by far not least among events of my Berkeley years: our two children, Anne and Bill, were born there at the end of the '50s and early in the '60s. As young children, they made sure that their father did not lose track of what the real meaning of life is all about.

The Irvine Years

In the Fall of 1965 I had received an invitation from Don Taylor, then chair of the Department of Industrial Administration at Yale, to spend the 1966-67 academic year as a visiting professor at Yale in that department. Although this represented a very appealing proposition for a number of reasons, especially because Ed Lawler was now at Yale as a junior faculty member, it was not an easy decision for me. This primarily was because I was heavily into a number of research activities at Berkeley and also because I was enjoying working with the then current set of graduate students, including Karlene Roberts and (from the School of Business) Ed Miller and Vance Mitchell. Nevertheless, the opportunity to go to Yale for a year was too attractive to turn down and, in addition, it would provide Ed and me a setting where we could work together in close proximity (down the hall, so to speak) to finish up our manuscript on Managerial Attitudes and Performance. So, together with my family (including, by now, two young children ages seven and four, I made plans to spend from September of 1966 to June of 1967 in New Haven.

In the spring of 1966, however, as we were making travel preparations, my friend Scott Myers, then employed as an industrial psychologist on the staff of Texas Instruments, invited me to spend the intervening summer months between Berkeley and Yale in Dallas at TI. I accepted his invitation, and the resulting three-month working interlude turned out to be especially informative and interesting. Most of my time was spent interviewing a number of TI managers and executives regarding various facets of their jobs and their views about managing, as well as having the opportunity to observe first-hand a number of TIs advanced (for those days) human resource practices.

The year at Yale was all that I had hoped it would be. Ed and I completed our book manuscript work as well as wrapping up several research papers on which we had been working. One of the major delights of the year was getting to know one Richard Hackman, who had only recently arrived to join the Industrial Administration faculty. (The department, incidentally, chose that year to change its name to Administrative Sciences, and it was to become the forerunner of today's School of Organization and Management.) Among other activities that year, Richard and I formulated and conducted a research study of expectancy theory predictions of work effectiveness, and this experience showed me what a superlative scholar Yale had hired when they had recruited this brand-new Ph.D. psychologist from the

University of Illinois. Also around Yale that year was another young faculty member who became a good friend, namely, Tim Hall. Furthermore, Chris Argyris had become department chairman and this allowed me to get to know someone whom I had admired from afar for his pioneering work in our field but whom I had only met one time previously. All in all, as might be imagined, it was an exceptionally stimulating year and it had a major positive impact on my subsequent career.

One other event, however, happened that year that had even greater impact: I made a fateful decision to leave Berkeley and go to a two-year old university, UC Irvine. It would take too long here to go into all of the details regarding the reasoning behind my decision. Many people, including my wife, were perplexed by this decision. Suffice to say, it was agonizing. I was just being promoted (during the 1966-67 year) to full professor at Berkeley and had greatly enjoyed my work and all of the friends we had made in the psychology department and elsewhere around the campus. Thus, there were absolutely no push reasons. The pull reasons were several but not all fully explainable, even to myself. Probably the biggest factor was simply a sort of inexplicable feeling on my part that it was time to (in the words of John Gardner) repot myself. I had been at Berkeley for eleven years (counting the visiting year at Yale) and had achieved far more than I ever expected or even had hoped for. But, I was restless, and this brand new campus with its infant Graduate School of Administration (GSA), held a sort of frontier allure for me. I had been thinking for several years that a business/management school, whether at Berkeley or elsewhere, might be a better home, as it were, for my interests. Management as an area of study and object of research seemed more central to a business school than to a psychology department. But, if a change to a management school, why Irvine? (My good friend, Jack Miner, had said to me shortly after my move: I can't understand why you would go there, it's like buying a pig in a poke; several years later, after he had visited me at Irvine, he recanted: I see now why you moved to UCI.) There were three specific reasons: Richard Snyder, GSAs first dean, had done an effective job of recruiting me; second, helping to build a new campus of the University of California (the umbrella institution which I knew would insure high quality) was a challenge that I found hard to pass up; and, third, the southern California costal area around Newport Beach (which is contiguous with Irvine) had always impressed me as an especially attractive place to live and, I can now attest, it definitely is. Nevertheless, despite these reasons that I gave to myself (and to Meredith), the decision to leave Berkeley -- which I made around March of 67 during my year at Yale -- was absolutely wrenching. Perhaps because it was so wrenching, I have never made another permanent move.

Just after the six-day Arab-Israel war had finished in mid-June of 1967, we departed New Haven by car for the Irvine campus to drop off a large number of books and files I had taken with me to Yale the previous September. We then would proceed back up to Berkeley to sell our house before returning to the Irvine area to look for housing. I mention this small detail of dropping off a set of books at my new campus office on a few-days-visit to Irvine before returning for good because of one particular incident that occurred at that time. I had asked the dean's secretary if she could get someone to help me carry up a number of boxes of books from my car. She said Sure, here's one of our master's degree students, and he will be glad to help you. It was John Van Maanen. I don't think either John or I realized that early July day of 1967 that we would eventually be working together so closely during the next four or five years.

The transition from Berkeley to UCI went fairly smoothly that first year for me, though it was not very easy on my family (since we had no relatives or close friends in the region when we moved to the Newport Beach/Irvine area). I was kept busy in my new role as Associate Dean of the GSA (later

renamed the Graduate School of Management) as well as adjusting to my new surroundings and making all sorts of new acquaintances. By the end of the 1967-68 year I had my feet more-or-less on the ground and was able to start focusing more intensively on resuming my research activities which had been interrupted, for all intents and purposes, by the move. One problem, though, was that GSA did not yet have a Ph.D. program in place. Since I was used to working closely with doctoral students, this seemed like a fairly serious obstacle. Indeed, I was concerned enough to ask Jim March, the founding dean of the School of Social Sciences at Irvine, what he thought I should do. Jim, in his usual incisive manner, said: Well, just go out and create them. It was then that I decided to work with what seemed to be the most appropriate second year master's student available, Van Maanen, even though at that time he did not appear to be terribly interested in my particular field of interest relating to behavior in organizations. John turned out, of course, to be quite helpful, and we found we did have some common interests. The following year GSA started a Ph.D. program and John was one of the first two admitted. (As a footnote, and to be true to the record, I actually had argued in a faculty meeting against his admission, not because he wasn't intellectually qualified, which he obviously was, but because I was concerned that he didn't have sufficiently defined interests and was in danger of becoming too much of a dilettante. So much for my skills in selection of talent!)

Since I've already mentioned John Van Maanen, this is as good a place as any to talk about the remarkable string of doctoral students that I have been privileged to be associated with as major advisor during my years in GSA (GSM) at Irvine. In recounting these names, the reader will again see evidence of my recurring good luck. First, as noted, was Van Maanen. John was unlike any of the other students I have had before or since, but he was someone who would put his own unique stamp on the field. His doctoral dissertation study of the Seattle Police Force training program was an especially insightful piece of work, in my opinion, and my main contributions were simply to encourage him to do it and to try to be as supportive as possible. Following Van Maanen (who graduated in 1972), my next doctoral student at Irvine, Rick Steers, was as different (from John) as two people can be. Whereas John tended toward the impulsive with a devil-may-care attitude toward life, Rick was careful, planful and serious, but no less talented. Just a different style. Rick and I have collaborated on several articles during and since his doctoral years and, as I write, the 5th edition of our edited text on motivation, Motivation and Work Behavior, has just been published by McGraw-Hill. Concurrently with Ricks period in the Ph.D. program there was another Rick, Rick Mowday, who became my third Ph.D. product. The two Ricks became friends and, of course, still are as colleagues on the faculty at the University of Oregon. Rick Mowday early on showed a great deal of promise, and my chief task, as I saw it, was simply to try to help develop his potential. Rick M, as well as Rick Steers, was at Irvine at the time that intense development was going on with respect to a research program I had started several years earlier dealing with organizational commitment. The three of us worked on several sets of data we had jointly collected (along with several other students, including Bill Crampon and Paul Boulian, and my great good friend and professional colleague [then] at Sears Roebuck & Co., Frank J. Smith). Ultimately, Rick M. and Rick S. and I published a book on our research on commitment, Employee-Organization Linkages: The Psychology of Commitment, Absenteeism and Turnover (Academic Press, 1982). The book was preceded in 1979, by an article on The Measurement of Organizational Commitment in the Journal of Vocational Behavior, an article that apparently has been heavily cited over the years.

Van MaanenSteersMowday. Not a bad way to start an OB Ph.D. program from scratch at a university less than ten years old. Just luck, however; sheer luck. But other good OB doctoral students also came to

Irvine in the 1970s. There is not enough space to list and talk about each one, but among others Eugene Stone certainly deserves mention. Gene, as would not surprise those who know him well, went through our program rapidly. He was always steadily focused, always applying himself to the task at hand, and always thinking about how to collect data to attack an interesting research problem. Together, and with great initiative on Gene's part, we published several articles dealing with job characteristics and their relationships to job attitudes. Gene Stone was (and still is) one of the best scientists I have had the pleasure to work with. (My long mentor at Yale, Neal Miller, would have been thrilled to have a student with Genes drive and understanding of the scientific process.)

In the late 1970s and early 1980s the continuing succession of Ph.D. students with whom I worked included Bob Allen, Hal Angle, Dan Dalton and David Krackhardt. The former two and I wrote what is still one of my own favorite pieces, a chapter on The Politics of Upward Influence in Organizations in the 1981 volume of Research in Organizational Behavior. Bob and I, along with my then-colleague Patty Renwick and two other graduate students, Dan Madison and Tom Mayes, had previously published articles utilizing an interesting set of data that we had collected dealing with perceptions of organizational politics in the managerial ranks of a sample of electronic companies. This empirical research in turn stimulated the Porter-Allen-Angle ROB conceptual chapter that put forth a number of testable propositions regarding upward (political-type) influence in organizations. The second author of that chapter, Bob Allen, has always been an astute observer of management both before and since he was a doctoral student, and the third author, Hal Angle, was a simply outstanding doctoral student and a person who is one of the three best psychologists qua psychologists that I have encountered as students over the years. (The other two are Gene Stone and Mordechai Eran [a former Ph.D. student at Berkeley who was then on leave as a high ranking officer in the personnel section of the Israeli Defense Forces]). Dan Dalton, though not my advisee, and I worked together on several articles dealing with turnover, and it was plain to see even then that Dan was headed for a productive scholarly career. David Krackhardt was a principal advisee of mine, and the two of us worked intensively on issues relating to organizational commitment and turnover. For his dissertation, David employed his (even then) expert knowledge of network analysis in studying what happens to those who remain when colleagues leave intact work groups. At the risk of overusing the word brilliant, David certainly fit that label. Finally, in the late 1980s at Irvine I had the good fortune to work with someone who is sure to make his mark on issues dealing with international aspects of behavior in work organizations: Stewart Black. Stewart was like Gene Stone in that he was highly focused and was able to move through the doctoral program at rapid speed, and he is like Ed Lawler in that he knows how to get things done. His student colleague, Hal Gregersen, who graduated a year after Stewart at the end of the 1980s, is also someone who has the potential to be a major contributor to the OB field.

Even though much of my energy and efforts at UC Irvine has been invested with an abundance of pleasurable returns in working with doctoral students, I have also found some time to work on behalf of the institution itself. For the first five years I was at Irvine I served as Associate Dean of GSM, and then for the following eleven years I was Dean (from 1972-83). This stint as Dean was a very rewarding period for me in a personal sense, even though it was an extremely difficult time for the campus, for our school of management, and for me because of constantly tight budget and resource constraints (the Reagan and Brown years).

One direct benefit of my deaning experience that I had not anticipated was the opportunity to become involved in the American Assembly of Collegiate Schools of Business (AACSB) by serving several terms on

its Board of Directors. This experience provided me with an overview of university-level management education on the national scene that I never would have been able to obtain as the dean of a single school. The experience also at least indirectly, to the opportunity of a lifetime: to be able to have a major role in the first comprehensive, nationwide study of management education that had been carried out in the U.S. in 25 years. This project, which was co-directed with my good friend, Larry McKibbin of the University of Oklahoma, culminated with the publication of our report by McGraw-Hill in 1988: Management Education and Development. The reason that the chance to take part in this study was such an involving experience for me was because I had spent a good portion of the preceding 15 or so years thinking about management education and how to improve it, and here was an opportunity to visit over sixty different universities and more than fifty companies and discuss with faculty members, deans, and business executives what was right and what was wrong about the way we were carrying out management education in our country. No project I had ever done before, nor am ever likely to do in the future, was so intensely involving for such a continuous period (two and a half years) .

My time deaning, working with doctoral students and serving on AACSB committees were not my only sets of activities during the years since arriving at Irvine in 1967. As mentioned earlier, I had joined Division 14 of APA as far back as the beginning of the 60s and had become deeply involved in that organization. In 1975, after serving on its Executive Committee for several years, I was elected President, a proud moment for me. An equally proud moment was 14 years later when the (now) Society of Industrial-Organizational Psychology presented me with the Distinguished Scientific contributions Award. (The fact that the presentation at the Annual Meeting held in New Orleans in 1989 was made by my former student, Gene Stone, added greatly to the occasion).

The other professional organization with which I have been heavily involved over the years has been the Academy of Management. I had joined the national Academy in 1966 and had chosen to become as active as possible, since it was my belief this was going to be the primary scholarly organization for those of us in the broad fields of management and organizational behavior, and I wanted to be where the intellectual and scholarly action was. (Little did I realize back in the mid-60s what a relatively mammoth organization the Academy would become.) In 1971 I was appointed the first Chairman of the Organizational Behavior Division of the Academy and subsequently elected Academy President in 1973. For someone who has been long associated with this organization, it was therefore a distinct and unforgettable honor to be selected with Herbert Simon as the first recipients of the Academys Scholarly Contributions to Management Award (now called the Richard D. Irwin Award) in 1983. (I can still remember as clearly as if it were yesterday when Art Bedeian called to tell me that I was to receive the Award. To say I was shocked would be to put it mildly; I wasn't even aware that the Academy had recently instituted the award.)

There have been still other activities that have engaged my attention and efforts over the years in relation to professional and educational interests. There is not enough space to go into all of them, but I will (again for the record) briefly mention the following: service on several committees of the American Psychological Association (including a task force involved in an unsuccessful attempt to reorganize that organization); service as an accreditor for the regional university accrediting association (the Western Association of Schools and Colleges); member of the Graduate Management Admission Councils (recent) Commission on Graduate Management Education; current (as I write) member of AACSBs Accreditation Task Force charged with re-writing AACSBs total set of accreditation standards; consultant

to several publishing companies (starting with Goodyear which merged into Scott, Foresman which merged into, now, HarperCollins); and, capitalizing on my fantasized, but totally unrealized, athletic talents, current service as UC Irvine's Faculty Athletic Representative to the NCAA.

As these last few pages probably demonstrate, and as I emphasized at the beginning as one of the themes characterizing my life and career to date, I seem to have a strong need to maximize variety in my life. Regardless of what has, or, more likely, hasn't been accomplished, this melange of activities has made for a great trip -- an unmanaged trip that has largely been devoted to trying to understand what organized activity and its management is all about. For one who likes to travel, this trip has been a good one so far, and it certainly has been exhilarating.