Ross Stagner

SIOP President 1965-1966

MY LIFE AND GROWTH IN INDUSTRIAL-ORGANIZATIONAL PSYCHOLOGY

The timing of my receipt of a Ph.D. degree from the University of Wisconsin was an important factor determining my subsequent professional career. The date was August 1932, and those young psychologists who in 1978 were bemoaning their misfortune in graduating into a tight job market may rest assured that job opportunities are lush compared to 1932. While the economic crunch did not hit me personally in that first year, I was acutely aware of widespread human suffering in the USA, and motivated to do something, although I was quite vague as to what could be done.

My first year, however, was one of relative affluence. A post-doctoral fellowship from SSRC (total stipend, $3000) maintained us in comfort while I continued my dissertation research on personality measurement and family influences on personality traits. Gordon Allport gave me some good advice by correspondence, and I visited Leon Thurstone at Chicago to learn of his new techniques for attitude scaling. I had no awareness of any interesting work in industrial psychology and no thought of planning anything in that area.

I should note that in those days one became a psychologist, with little specialization, and I still adhere to this as a training ideal. I have tried to indoctrinate in my students a conception of psychology as a body of theory, research, and application, in which developments in the laboratory may affect field work (as industry or clinic), and new theories may influence either.

My selection of a career in psychology was in part accidental. Because I had in high school been praised for my writing skill, I entered Washington University in 1925 with a vague plan to become a teacher of literature and a creative writer. The English professors with whom I worked encouraged this orientation. I therefore had elected only a few courses in psychology which seemed possibly useful in a writing career: introductory, abnormal, social. I had completed these and expected to take no more psychology when, during registration in my junior year, Marion Bunch captured me in the corridor. It seemed that he wanted to teach a new course in psychology of learning; he had six students but the college would cancel the proposed course if the enrollment was less than seven. As I had a full schedule, I demurred, but he was persuasive, so I took the course as an overload. As we began to do research and actually observe human learning behavior, I found this activity more exciting than critiques of Victorian novelists. Thus, in my senior year I switched to a psychology major.

Bunch and a medical friend, Herbert Gasser, also helped me get an appointment at Wisconsin as an assistant to Clark Hull. Unfortunately, I reported in September 1929 and found that Hull had just left for the Institute of Human Relations at Yale. I assisted Richard Husband instead; we got along well, but I did not respond at that time to his interest in applied psychology. I found Norman Cameron and his aggressive behaviorism more exciting, and adopted a Pavlovian approach for a time. This shows in the first (1937) edition of my Psychology of Personality. (It was some years later that Norman was psychoanalyzed and repented of his behavioristic preachings.) I also took some medical school work (physiology and physiological chemistry) for a minor; it was interesting, and at times also amusing. I recall one occasion in which Abe Maslow and I were involved in a demonstration when our professor (the Dean of the Medical School) was stimulating the peripheral end of the cut vagus nerve. He predicted that the heart rate would slow down, but instead it speeded up. Both of us laughed and suggested that a course in statistics was indicated to cope with the probability issue.

Norman Cameron left Wisconsin, and my adviser became Dr. V.A.C. Henmon, who was essentially interested in educational psychology (co-developer of the Henmon-Nelson Test of Mental Ability). He was Chairman of the Psychology Department, although chronically ill. I proposed a dissertation on "The Measurement of Personality Traits" which he approved. However, because of his frequent absences, I received little advice from him, and relied more on Kimball Young. I really operated almost entirely on my own. I remember that when the deadline date for filing the dissertation arrived, I went in and placed a dissertation of 400+ pages on Henmon's desk and asked him if he wanted to read it and offer criticisms. He preferred to sign it and send it to the Graduate School.

Some friends have assumed that my doctoral dissertation was on the curare experiment with Harry Harlow. This is incorrect. My involvement with the curare study was almost accidental. One evening, when I was already deeply involved in my dissertation-in fact, I was actually typing part of it-Harry entered my office and began a discussion of the current debate over the question "Can learning occur without a response?" He proposed the use of curare to prevent a response. When I asked, "How do we keep the animals alive?" his characteristic response was, "That's your problem." And indeed it was, but eventually I solved it. The excitement over the curare research led many psychologists to assume that my specialty was physiological, but I clung to my idea that one should be accomplished in all areas of psychology.

The fact that I reacted to the traumatic economic situation of the 1930's by becoming sympathetic to unions is in some ways surprising. I came from a very conservative background. I was born in a small city in central Texas, and my father was at various times a farmer, a skilled leather worker, and a small retail storekeeper in a rural village. The opinions I heard expressed at home and in the community ranged from conservative to reactionary. Racial and religious prejudices were standard fare. It should have been quite easy for me to have reacted to economic distress as did many lower middle-class Germans, nostalgically seeking for the "good old days" under a totalitarian banner.

An incident in my college career likewise might have predisposed me to oppose unions. In Texas I had learned the printer's trade, working on a small weekly paper at the age of 12, setting type by hand and linotype, making up forms, and feeding presses. I couldn't lift the form onto the press; it was too heavy. However, I did everything else. When I went to St. Louis in 1925, I found a job as a typesetter in a job printing firm, but was let go after a few weeks. The boss told me I was too slow, but he also intimated that the union objected to my working. When I inquired about joining the union, he said there was a large initiation fee, and the union might not accept me anyway, because they preferred young men who expected to make a career in the field, whereas it was clear that I was not likely to do so. I could have developed a grudge against the union on this basis, but again, strangely, did not. (My annoyance was eased by finding a nonunion shop where I worked successfully for a couple of years.)

As I search my memories for possible explanations of this tolerance for what might have been perceived as an injury by the union, I recall my freshman English instructor, Edgar Curtis Taylor. Taylor was not much older than his students, and he combined a considerable personal charm (and polished speech) with cynicism about our society, especially about the military. This, of course, was in 1925, and disillusionment with World War I was rampant among intellectuals. I doubt that Taylor ever expressed an opinion about unions, but his tendency to heckle the established truths of conservative society may have rubbed off on me. Certainly he influenced my personal tastes and ideas in ways I can identify. (He also helped me out financially; in my sophomore year I graded many freshman themes for him, thus developing some editorial skill which has plagued my own students and faculty colleagues over the years. My rabid insistence on accurate spelling, of course, goes back to my years as a printer.)

It was during my years at Washington University that I studied with Floyd Ruch. He was the only one of my teachers who ever achieved the presidency of Division 14. Floyd was teaching for a year before returning to Stanford to complete his Ph.D. I ran rats for him in the basement of an old wooden building. However, I found the research on human subjects with Marion Bunch more exciting, and spent most of my time on it. Floyd, if my memory is correct, was not particularly inclined toward industrial work at that time; certainly he did not point me in that direction.

To return to Wisconsin - after my year on the SSRC fellowship, when I felt more affluent than I have in the 40-odd succeeding years, I fell on hard times. There were no jobs. The Department obliged by giving me a half-time teaching assistantship (at $440 for the academic year!) This helped slightly with our financial problems; my wife was much more helpful, getting a job as a statistician with a federal project.

My interest in labor unions undoubtedly stems from this year of my life. I joined a group of unemployed workers known as the Workers' Alliance, which lobbied in Madison and Washington for more generous work programs. I also became active in Socialist Party politics and almost got elected to the Madison Board of Education. I read a great deal about fascism in Italy and nazism in Germany, and was stimulated-probably by the Sinclair Lewis novel, It Can't Happen Here - toward developing a scale of pro-fascist attitudes. This was the predecessor of the F-scale and The Authoritarian Personality, although the Adorno group chose not to mention my investigations in their book.

Our finances improved marginally in the fall of 1934. My friend, Maurice Krout, was teaching at a small institution called the People's Junior College in Chicago. Classes met in the Jewish People's Institute on Douglass Boulevard. Teaching was done by faculty who had been laid off when the city closed Crane Junior College in 1932; each student paid $50 per semester, and the faculty divided the income equally. When Krout got a better position, I took his post, earning about $900 for the year 1934-35. I taught several courses in sociology, an area in which I had no training at all, as well as the entire psychology curriculum. I managed to gather respondents for the preliminary fascist scale, exploit interested students to do the statistics, and prepare a revision of the scale. (Journal of Social Psychology, 1936.)

Finally, in the spring of 1935, I obtained my first regular faculty appointment, at the University of Akron, then municipal, now a part of the Ohio system. My enthusiasm soared when, on my first day in that city, I saw a huge banner welcoming the "Rubber Workers Organizing Committee," an activity of the then developing CIO. Before long I had gotten involved in educational and organizational efforts of this group. Interestingly enough, although some faculty colleagues suggested that I was imprudent, no administrator ever reprimanded me for this extracurricular activity. (I was told at one point that a member of the Board of Trustees had hired a private detective to observe my non-university actions, but if this was true, nothing ever came of it.)

I learned a great deal about industrial working conditions from the rubber workers over the next four years. I also had an extensive indoctrination in factionalism. My year at Chicago had seen me elected vice-president of a teacher's union local-A.J. Carlson was the president. In Akron I found myself as one of a small group of socialists entirely surrounded by Stalinists and Trotskyites. Since these two groups hated each other more than they disliked capitalists, I often found myself presiding over meetings because each group would vote for a socialist in preference to one of their detested opponents.

One memory of this period lingers especially clearly. Some of the material I had written for the United Rubberworkers was picked up by Ruth McKenney in her book, Industrial Valley, and printed as proof of how the Communist Party had helped the union! I was furious but had no way to counter this canard.

My teaching load was heavy: the standard was 15 hours weekly, and because students often petitioned me for advanced courses, I taught 18 hours several times, and 21 in one disastrous semester. I never made that mistake again. Obviously I taught everything in the curriculum, from statistics to abnormal psychology, and it was here that I first worked up a nodding acquaintance with the industrial area, which was just developing. on a couple of occasions I taught an introductory industrial-personnel course. As was traditional and may still be, the textbooks never mentioned labor unions, and at the time, I limited my comments to pointing out that unions were beginning to impose some restrictions on time-and-motion study, supervisory practices, and the like. Later I discovered the Hawthorne studies and incorporated some references to social factors in the work situation into my teaching. It is worth noting that I never had a course in industrial psychology, as an undergraduate or graduate student. This freedom from preconceptions may have been useful in my later Career, and it confirms my long-standing prejudice in favor of general training for all would-be psychologists. I believe that premature specialization imposes an undesirable rigidity on the cognitive patterns of the budding professional.

During the stay in Akron I published the first edition of my Psychology of Personality (1937), the fascist attitude studies, and some work on social stereotypes. Fortunately, these attracted the attention of Ted Karwoski and he obtained a faculty appointment for me at Dartmouth College. This was a welcome change from Akron's grime, and the teaching load at Dartmouth was far lighter. I, like the other junior members of the faculty, assisted Charles Leonard Stone in the introductory course. Stone gave the lectures, and we met quiz sessions once a week. I was also invited to prepare two advanced courses, and I elected to offer a course in "the psychology of world politics" and one in "the psychology of industrial conflict." I am reasonably sure that the latter was a complete innovation in psychology anywhere. Some courses had been taught in the psychology of politics (e.g., I believe Harold Lasswell gave one at Chicago) but the industrial course was a real innovation.

Many Dartmouth students came from wealthy families, and teaching them a balanced view of union-management problems was not easy. Some even alleged that what I offered was an unbalanced approach. But many of those young men developed a much better understanding of industrial problems than they had had earlier. Two episodes were good for teaching purposes. One young man, whose father was president of a large public utility, decided to do a study of AT&T labor relations policy, including a survey of stockholder attitudes. I told him he could never get a mailing list, but be assured me that, as a holder of a large block of shares, he could do this easily. When he came back from Christmas recess, defeated by the AT&T bureaucracy, he had less enthusiasm for the way American industry was managed.

Just before America's entry into World War II, some of my students decided to do job satisfaction surveys. Two of them went down the Connecticut River valley to a small city where some small but important machine tool factories were located, collecting interviews at workers' homes. Word of this reached the owners, one of whom contacted the president of Dartmouth, and I was instructed to stop the research (note that at Akron no one had been so restrictive!) When the students heard of this, they disappeared and returned a few days later (with their full complement of data) and I was able to assure the president that the project had been terminated.

Let me pick up another thread in my career development. In Chicago in 1934 I met I. Krechevsky, later known as David Krech. We were both bitter about the employment situation and together we circulated petitions to psychologists, collecting several hundred signatures, urging the APA Board of Directors to take a more active role in psychologist placement, advocating more public jobs for psychologists, etc. While the APA turned us down, as was usual in those days, Krech and I followed through by calling a meeting at the 1936 APA Convention (on the Dartmouth campus, coincidentally, although I was still at Akron) at which we organized the Society for the Psychological Study of Social Issues (now known as SPSSI or Division 9 of APA). I was chairman of the organizing session and he was the secretary.

The Council of the new society decided that our first contribution should be a Yearbook series. The first, on industrial conflict, was coedited by George Hartmann and Ted Newcomb; the second was to deal with war and peace. I was invited to chair the editorial committee for this book; the other members were J. F. Brown, Ralph Gundlach, and Ralph K. White. The 4-person format was unfortunate. White and I took a socialist line, while Brown and Gundlach could plausibly be called Stalinists. This caused no trouble while we stuck to psychological theory, but when we ventured into applications to national violence, the committee often split evenly. I finally resolved this problem by ruling that in a 2-2 tie, I as chairman could decide. Curiously, neither Brown nor Gundlach criticized this version of democratic centralism.

The book on war and peace was making good progress when I moved to Dartmouth in 1939, and this was fortunate, because I found Charles Osgood waiting to work with me. He had acquired his B.A., and chose to stay on for a year (without stipend) before applying to graduate school. He and I immediately embarked on a series of studies on vocational stereotypes and nationalistic attitudes which led him eventually to develop his semantic differential technique. In the meantime, his wife, Mrs. Osgood, helped me tremendously by typing stencils for a mimeographed version of the peace MS. to be used in my "world politics" class.

This book had a curious fate. In a spat with McGraw-Hill over the industrial conflict yearbook, we cancelled contracts both for that book and for the one on war and peace. However, other publishers expressed an interest in it, and I was just doing some final editing when the Japanese unkindly messed everything up by their discourteous behavior at Pearl Harbor. Because some of the remarks about our State Department might have been misinterpreted, we unanimously agreed to drop the matter for the moment. Then, during 1944, I received permission from my co-authors and the SPSSI Council to use the material in a new book, to be called "Psychology and Peace Planning." Harpers accepted this one, but then cancelled when their paper quota was slashed. The material sat around until 1967, when I again rewrote it as Psychological Aspects of International Conflict (Brooks/ Cole, 1967).

The Japanese delinquency also affected my career drastically. Because we expected Dartmouth to close (as all young men were going into service), the Dean advised us to seek useful jobs for the duration. I went to Koppers Company in Pittsburgh and found that I had to teach myself personnel routines. Being disposed to be truthful, especially when I was sure to be found out if I exaggerated my knowledge, I pointed out to the company executives who interviewed me that my knowledge of personnel procedures was strictly academic, as I had no practical experience. To which the then Treasurer of the corporation said, "That's all right, Ross, neither do we."

As things turned out, he was right. There were no records to speak of, no selection system, no training, no communications program. I found people classed as "clerk" who earned from $2,000 to $18,000 per year. Since wage-control legislation required "established procedures" for giving increases, e.g., when a person was promoted, I had to devise records and accumulate data proving that procedures did exist. This meant conducting a very large job-classification program for white-collar employees, and establishing formal rules for merit and promotional increases. I also supervised some aspects of conformity to wage controls for several thousand blue-collar employees. Since most of these were unionized, the problems of established rates and merit increases did not cause trouble.

When I had gotten the salary administration under control, I urged - and was allowed to implement - development of a better personnel program in general. One of my innovations was to suggest a simplified profit-and-loss statement for employees. The Executive Vice-President asked, "What business is it of theirs, how much money we make?" I asked if he had any unions to deal with, and whether they collected data on the company's profitability. He got the point and approved a version which could be mailed to all employees, showing that by far the largest proportion of company expenditures went for wages and salaries. (As I learned more about accounting, I found that one cannot take profit-and-loss statements at face value, but at the time I felt pleased at making a contribution to communication.)

My interest in the economic aspects of psychological problems paid off in an unexpected way at this time. There was a rather widespread expectation that World War II would be followed by a sharp depression such as the USA experienced in 1920. Consequently, the Pabst Brewing Co. sponsored an essay contest for postwar employment planning. There was a distinguished panel of judges. First and second prizes went to two professional economists, each of whom was later on the President's Council of Economic Advisers. I was one of those who tied for third place and received a $1000 award (of 12 winners, I was one of the two individuals not possessing a doctorate in economics). Of course, as things worked out, we did not have a real downturn in 1946, and nobody made any attempt to implement the plans we devised-all of which sounded rather similar-but it was a gratifying experience.

About this time one of our new plants, near Pittsburgh, was organized by a union, and I found myself drawn into some grievance handling. I had no difficulty communicating with the new union officers, and believe I was relatively successful. The fact that there were no established precedents, and the plant had no history of conflict, must have been helpful. I learned a lot, and produced no catastrophes.

The importance of organizational membership in determining perceptions, attitudes, and even acceptable goals, was brought home to me during this period. After the war I began to expound the importance of union and management organizations as influences on the behavior and perceptions of participants (cf. Personnel Psychology, 1948). While this did not involve the emphasis on specific roles which now characterizes the social psychology of organizations, it constituted a preliminary step into this area which has become so important for industrial-organizational psychology.

My relations with the top executives at Koppers were good, and I was offered a very substantial salary increase to remain after V-J day. Two factors decided for me that I should return to teaching. One was a serious bout of infectious arthritis in the summer of 1945, which made me and my family dubious about the allegedly more stressful life in industry. The second was an assignment to do a job proving that certain employees were not entitled to time-and-one-half pay for overtime hours worked during the war; after a couple of weeks, I concluded that they were entitled to such pay. I had no open controversy with top management about it, but just felt that I might be happier back in academia. Thus I returned to Dartmouth in September 1945 at a 50% cut in pay.

The students returning to Dartmouth had an obsessively practical orientation; they demanded courses in industrial psychology and personnel methods. I offered two new courses in these areas and was swamped with students. I became more deeply involved in industrial relations when my friend Herman Feldman (of the Tuck School faculty) died suddenly and I was asked to teach his courses for a semester. To tell the truth, I was not really excited about all of this practical stuff; I still cherished the dream that I was going to make important theoretical contributions to psychology. I craved graduate students, who could do sophisticated research under my supervision. I felt that I could teach my undergraduate courses in my sleep, and occasionally feared that I was doing so.

The opportunity for a change, however, actually developed out of the company-oriented work I was doing. I was pleased to be invited to summer personnel conferences, two at General Motors, one at Standard Oil. The Standard Oil visit provided me with a delightful anecdote; I was interviewing a professional chemist, and he said, "At Standard you produce-or you stay!" He also gave me a marvelous definition of his job; he described it as a furlined rat-trap. The benefits were so good that he could not make up his mind to leave, although he felt that his professional growth was being stifled. We are seeing more and more of this kind of problem in our affluent society today.

On my second visit to GM I met Phillips Bradley, then Director of the Institute of Labor and Industrial Relations at Illinois. Bradley decided I was just what he needed for his Illini City research team, but we dickered for a year or more because his financial offer seemed inadequate to the expenses of moving. Finally he raised his bid to a level which would take care of this problem, and I moved my family to Urbana in the winter of 1949. Because there were few industrial doctoral students, I found myself supervising a number of researches on personality and enjoyed this tremendously, but I was also greatly intrigued by the complications of trying to communicate with economists, sociologists and political scientists in a research team. Louis McQuitty was already in the group, and the two of us labored to confirm both the methodological utility and the conceptual validity of a psychological approach.

The Illini City studies involved an intensive study of eight firms in a nearby city. In addition to a lot of data on wages and working conditions, we collected long interviews with key management and union officers, as well as foremen and rank-and-file employees. All of us agreed that we wanted to do something innovative, something beyond the case histories which institutional economics produced in abundance. The final framework, as it was published, included three major dimensions: economic status of the establishment, degree of union influence, and attitudinal climate. It is at least partly correct to interpret these as the contributions, respectively, of economists, political scientists, and psychologists. Since McQuitty left the team before this framework was evolved, I think I can claim credit for the introduction of the "climate" concept in organizational research. Of course it has been used very widely and in many different senses since that time.

If the Illini City researches convinced me of anything, it was that psychological variables must always be interpreted in a larger socio-economic context. I had long since adopted the general theoretical framework of Kurt Lewin (cf. the 1948 edition of my personality text), hence I found it convenient to formalize my ideas in terms of life space, valences, boundaries, barriers, and tensions. We completed the Illini City write-up in 1953 and I set out almost at once to prepare a complete theoretical analysis of unionmanagement relations from a psychological viewpoint. This was published by Wiley in 1956 as The

Psychology of Industrial Conflict. While it would be an exaggeration to say that my colleagues in the industrial field were exuberant over the book, it received a fair amount of praise and I was interested, in later years, to note how often I saw it in the libraries of corporate labor relations directors. (There are no records as to its value in moderating conflicts.)

The Illini City studies led, accidentally, to a small research study which got an undue amount of publicity. While we were visiting the eight firms in Illini City, one personnel manager told me of a salesman who had tried to sell him an employee selection system which seemed to have no supporting data, and the idea occurred to me of a demonstration which might drive home to personnel and industrial relations people the importance of demanding evidence such as reliability and validity statistics.

Our extension service was arranging a large conference for Illinois personnel executives on the Urbana campus. I arranged with those in charge to administer a very brief personality scale on the first day of the conference, with a promise that scores would be returned on the following day. In the meantime we had mimeographed the list of "glittering generalizations" about personality collected by Forer (1949), circled the numbers of certain items with red pencil, and folded them. The name of each personnel official was then copied in red on the outside, to support the illusion that the items had been marked specifically for him. (A real report form, showing percentiles and trait definitions based on the bona fide scale, was also prepared.)

On the second day, the fake analyses were distributed with some comments about confidentiality to discourage comparisons. All were asked to vote on how accurate the description was; about 80% rated it highly accurate. They were then allowed to compare forms, and discovered that all were identical. After they quieted down, I handed out the legitimate forms and discussed the importance of quantification, statistical analysis, and validation. The lesson seemed to be effective. However, it has not permeated the field. In January 1978 two of my graduate students repeated the process with the Detroit Personnel Managers Association and got substantially identical degrees of "gullibility."

Personnel Psychology reprinted the 1958 report in a mini-reprint format and mailed out thousands of copies with an appeal for subscriptions. This probably accounts for the fact that so many I-O psychologists recognize my name.

The Illini City years involved some disappointments as well as feelings of achievement. After two or three years in the research team, I began to evolve some theories about union and management leadership, trying to apply my extensive reading in personality. I develop some "structured projective" devices focused on these hypotheses. One was an adaptation of Saul Rosenzweig's P-F (picture frustration) Test; I set up a series of cartoons depicting common industrial relations events, leaving a spot for the respondent to supply the reply of one participant, manager or unionist. While field testing showed that it had face validity (managers and unionists differed sharply in their projected answers), none of my hypotheses about extrapunitive and intropunitive aggression checked out, and nothing of this was ever published.

Similarly I collected a lengthy series of TAT-type photographs, with responses to be chosen rather than given freely by interviewees. My research assistant, Ed Lawson, interviewed at least 100 industrial relations and union officials, but an exhaustive analysis of the data revealed about the number of significant differences to have been expected by chance. This one did lead to a better-designed study, executed in Detroit, demonstrating that personality traits of a shop steward and of his managerial opposite number played a significant role in the quality of union-management relations in the tool-and-die industry.

My success in achieving a modest reputation in union-management circles probably helped me to obtain my first Fulbright appointment, for the year (1955-56) in Rome. My first discovery in Italy was that my language lessons had not prepared me for conversational Italian; I had to work very hard for three months to manage even a slow interview with corporate executives. Union leaders were even more difficult, since many of them had regional accents which confused my unpracticed ear. However, I learned a great deal about the functioning of the unions, and was impressed by their concern with political rather than economic goals. In one plant I was somewhat horrified by safety conditions, and I asked a steward what the union was doing about these. His reply was "We don't worry about these; we are working for the revolution." My appreciation of American trade unions improved perceptibly as a result of my Italian visit.

In 1957 I received a very persuasive offer from Wayne State University to head the psychology program there; the offer included both monetary inducements and promises of support for expansion. Truth to tell, I hoped the administrators at Illinois would offer me a handsome inducement to stay, but mostly they congratulated me on such a flattering offer and wished me good luck. I am now inclined to think they did me a favor, and at worst I can say that I entered upon the new position without any serious regret at leaving the prestigious Illinois department for a smaller school. And I also believe that in my 15 years as chairman, I accomplished a good deal by way of narrowing the gap between the two universities, at least as regards psychology.

When I settled in at Wayne State, I found that Arthur Kornhauser had compiled an impressive amount of data on the mental health of the industrial worker. I like to think that the assistance I provided him, through administrative devices, in preparing his book for publication, is one of my significant contributions to the field. After Arthur's retirement I induced Hjalmar Rosen to leave Illinois and join the Wayne faculty, and we jointly prepared a paperback entitled Psychology of Union-Management Relations (1965). During this period I also managed to produce a third edition of my personality text, and, with some graduate student assistance, a small study on leadership as a variable affecting union-management relations in Detroit tool and die shops.

In 1960 my colleagues showered me with recognition: I was elected president of the Midwestern Psychological Association, and also president of the Division of Personality and Social Psychology. The next year led to election to the APA Board of Directors, where I spent three exciting years. My most vivid memory of that term relates to the erection of the Headquarters building at 17th and Rhode Island in Washington. When I was asked to serve on the Building Committee, I thought maybe my knowledge of industry would be of some value, but I soon discovered that Eddie Newman, fresh from a larger building project at Harvard, knew far more than I was likely to learn in a year, so I relaxed and let him advise us regarding materials, installation, architectural problems, and the like. I doubt that many members of APA know how much credit is due to Newman for his thoughtful and industrious work on the building project.

The borderline between economic theory and psychological theory, which George Katona was already exploring, also tempted me, and in 1962 I was fortunate to get an NSF Fellowship which enabled me to spend a semester in Ann Arbor in economics seminars. One result was to convince me that economists tolerate vastly greater error variances than we accept in psychological research. Another was to raise the suspicion that at least some of modern economic theory is an updated version of the medieval debate: how many angels can dance on the head of a pin? I posed the issue of decision-making at the top executive level of a corporation to a friend, a very prestigious economist. He spent about 30 minutes at his blackboard writing equations and elaborating on the marginal utilities involved. Finally I interrupted and asked, "But what does this have to do with actual corporate behavior?" To which his reply, without blushing, was "Nothing."

I probably have a large Zeigarnik index. At any rate, I decided that I wanted to study this decision-making process among top executives. The Ford Foundation kindly cooperated, and I spent 1962-63 interviewing corporate vice-presidents, as well as reading the voluminous literature, most of which turned out to be closer to reality than the theorist, but almost as useless because it was about 75% subjective opinion, with very little hard data.

It would have been nice to be able to conclude this phase of my life history with the conclusion that I found ways of integrating economic with psychological theory. Regrettably, I did not. In fact, my conclusion from the decision-making foray was that corporations behave like coalitions (an idea I borrowed from March & Simon), with the relative power of the competitors determining which side wins in an executive suite battle. This conclusion probably gives more comfort to the political scientists than to economists or psychologists.

The decision-making study provided me with a suitable presidential address when I was elected to the presidency of Division 14 in 1965. I astutely managed to avoid spending any time on bureaucratic tasks of the Division by taking my second Fulbright year, this one at London, where I learned more about problems of union-management relations as influenced by national culture. The British unions, while less politicized than those of Italy, were permeated by class consciousness, and so was British management. I again decided that there was much to be said for the highly mobile American culture, in which accent, style of dress, type of education, and similar variables played a smaller role in determining the fate of a specific individual. I also was reminded of the great advantage, from all viewpoints, of having one industrial union as opposed to sixteen craft unions in a given medium-sized factory. The competition between the unions created problems and production bottlenecks, and confused managers.

In 1965 I was also honored by the Wayne State faculty, which chose me as the Franklin Lecturer for the year. (This was an endowed lectureship and generally was awarded to a scholar in the field of human relations.) I invited speakers to campus who discussed, respectively, marital conflict, racial conflict, industrial conflict, and international conflict. My contribution was a theoretical essay on the dimensionality of human conflicts. The lectures were collected and published by the Wayne State Press under the title, Dimensions of Human Conflict.

With advancing age, I found administrative work more distasteful, even though some people seemed to think I was a successful chairman. I did expand the Wayne State department from eleven full-time faculty members to 38, fostered an approved clinical psychology training program, and obtained training grants in social and experimental psychology. The department also garnered substantial non-governmental funding. Nevertheless, I began a yearly ritual of submitting my resignation, and the Dean evolved excuses for declining it. As he ran out of excuses (or as I approached the age of 65, at which time the policy was to terminate administrative assignments), he finally accepted my resignation in 1972, and a search committee was set up to look for a successor.

I wanted to expand my knowledge of brain and behavior relationships, so I used my sabbatical year 1972-73 to study with Karl Pribram at Stanford. Lest this be viewed as a regression to my infantile (i.e., graduate student) interest in physiological psychology, let me say that I had kept in touch with at least some of the literature in this field for many years. I had especially sought for support for my theoretical bias toward homeostatic theory, and for new insights into physiological bases for differences in personality.

Pribram, is of course a warm, exciting, and helpful person. I was duly impressed with the fascinating work being done on brain functions, at Stanford and elsewhere. Upon my return to Wayne State as a professor, I obtained a laboratory and started exploring some of the hypotheses I had generated. One was a cooperative project with Philip Rennick (of the Lafayette Clinic and the Wayne faculty) to study electrocortical patterns in self-described "impulsive" and "deliberate" persons. We used the auditory evoked potential and the contingent negative variation as dependent variables, and obtained some significant differences pointing to a higher excitation level of "impulsives." Due to Rennick's untimely death, this work has not been followed up with other investigations which might point to hereditary or learning explanations for these differences.

In the last few years, graduate students in the industrial area have sought my advice as dissertation sponsor, and so I am back in that field. One of the most interesting projects to be completed is one by Boaz Eflal in which we obtained pre-strike measures of attitudes of UAW members toward their local and national leaders, then got parallel samples during the 1976 strike, and after the strike had been settled. The results confirmed some deductions from more general theories as to the unifying effect of a strike and the over-valuation of benefits by those workers involved in the strike, as compared with other union locals not on strike.

Elderly psychologists are expected to acquire an interest in gerontological research, and I have conformed to that pattern. I have finished a study on "the propensity to work" of UAW retirees, which I consider to be important because of the social desirability of drawing some of these individuals back into the productive process. There are both economic reasons (for the welfare of society) and psychological reasons (for the welfare of the individual) why second careers, or part-time jobs, might be desirable. I plan to follow this with investigations of psychological factors disposing employees to seek early retirement, and using these to optimize pre-retirement planning and counseling activities.

This means that I cannot end this autobiographical statement with closure of my professional activities. I can, however, look back and feel that, in the industrial area, I contributed something to the push toward organizational research by my early publications on the importance of unions and the impact of unionization on the worker. I also did some pioneer writing on the role of perception and the significance of individual differences in perceived reality as determinants of the behavior of managers, workers, and union officials. Finally, I have tried, with less success, to interest I-O psychologists in the phenomena of industrial conflict. It has become increasingly clear that the economists have had relatively little success in clarifying the behavior of managers and unionists in collective bargaining. There is a real need for social/industrial psychologists to apply our conceptual and methodological tools in this area; if my writings have helped to move some younger psychologists in this direction, I shall consider this my most important contribution.