Stanley E. Seashore

SIOP President 1968-1969

My entry into psychology, into an applied sub-field, and specifically into I-O psychology is apparently a matter of predestination and implacable environmental forces. I have little recollection of making deliberate choices, but for the acceptance or rejection of opportunities as they were pro-offered. I had no career plan until later years when the "plan" revealed itself by simple forward extrapolation. Attempts to become a personnel manager, diplomat, cultural anthropologist, or journalist rather than a psychologist were consistently subverted.

I might as well mention the family connections right off. The psychological family patriarch was my uncle Carl E. Seashore, an early notable in the field. A member of the APA directorate in 1906 and President in 1911, he thought psychology ought to be useful. He devised a screening test to locate children of exceptional musical talent, wrote a popular book on the use of psychology in everyday life, and when I talked with him last was interested in the use of vocal sound wave analysis in medical diagnoses. His son, Robert H. (my cousin, of course) became chairman of the psychology department at Northwestern, member of the APA directorate in 1943, did basic and applied work on motor skills. My brother, Harold, applied psychologist and entrepreneur, brought the Test Division of the Psychological Corporation to a full flowering. My cousin-once-removed Charles, son of Robert, now practices the psychological trade together with his wife Edith as organizational and group process consultants, and his sister Marjorie, a social psychologist, specializes in complex organizations. My own daughter Karen Seashore Louis, conducts research on organizational change and program effectiveness. That makes eight in three generations who plausibly can be claimed as I-O psychologists.

I

Exposure to psychology came early. At age 19, in mid-depression both economically and psychologically, I was taken into the household of Uncle Carl in Iowa City, and stayed more than three years while a student at the University of Iowa. I was officially the paid houseman, with a full complement of kitchen, household, and yard duties, but was treated in other ways as a member of the family. My uncle at the time was Dean of the graduate school and also Chairman of the Department of Psychology. Table conversation was occasionally psychological; the stream of house guests included local students and faculty as well as itinerant psychologists from other universities.

I have just looked at my transcript from Iowa. It appears that I took a one-term introductory course in psychology. That's all, in psychology. I got a grade of "B," which raised my average that term. My major was political science, with minors in economics and history. Where choices allowed, I took courses of a foreign or international focus, having in mind, a bit vaguely, journalism or the U. S. Foreign Service.

Following graduation from Iowa in 1937, with jobs scarce and no marketable skills, I took an offered summer adventure with Dr. Als Hrdlicka, then head of physical anthropology for the Smithsonian Institution. Our small expedition took us throughout the Aleutian Islands chain and to the Komandorskyes, with the task of locating prehistoric cultural artifacts and physical specimens that might help unravel the early human subarctic migrations. The pay was poor -- nothing -- but the company was good, and all costs covered. Dr. Hrdlicka took seriously his offer to provide instruction in lieu of pay; the work schedule, when interrupted by weather or tides, allowed many a soggy hour of lecture or individual tutoring in anthropological matters. I got interested and, on return, borrowed some money (it didn't take much in those days) and signed up for graduate study with the Department of Anthropology, University of Minnesota.

II

I soon conceived the idea, hardly original, that social anthropology needed an infusion of psychometrics and sociometry as an antidote for random field observations interpreted without guiding theory. This naive and misguided view led me to take some courses in psychology. Donald Patterson taught the course in individual differences, with a heavy emphasis on job analysis and vocational guidance which were then his main preoccupations. Charles Bird taught what must have been, at the time, an innovative course in social psychology, although I began to appreciate it only as the unimpressive substantive content began to fade from my mind. There was a course in personality, which was my first effective exposure to the world of theoretical combat.

Instructed but disenchanted with anthropology, and introduced to psychology, I took the Master's degree and entered the doctoral program in psychology at the University of Iowa. It was a short stay, as I dropped out in mid-term.

III

It was Joseph Tiffin, without any doubt, who with a single act made a psychologist out of me. While a student at Iowa I had various supplemental part-time jobs, as students do, one of which was a brief period as laboratory aide to Joe. At the time, he was a "brass instrument" experimental psychologist. I did minor lab chores, punched the calculator, and out of innocence and interest made various suggestions that seemed to help the work. Joe Tiffin became a renegade f rom classical sensory and psychophysical psychology by going to Purdue to work in industrial psychology. He sought industrial contacts, one of which was with the people in charge of training and of personnel at the Gary (Indiana) Sheet and Tin Mill. This plant of some 11,000 employees was then a part of Carnegie-Illinois Steel Corporation, and of the U. S. Steel empire. There was an opening for someone who could do odd chores to justify his pay while being available to help with Tiffin's work in the plant. I received a phone call from Joe, arrived a few days later in Gary, and got hired on as a personnel office assistant, duties unspecified. The pay was 62.5 cents per hour, the going minimum rate in the industry. I had never before been in a factory of any kind. The affinity was instant.

At the time, late 1939, the steel industry was already well out of the depression doldrums and preparing for heavy demands that followed the loss of production in parts of Europe and the likelihood of U. S. entry in World War II. The Gary Sheet and Tin Mill was a lively place, open to innovations in personnel management and training. My mentors there were E. B. Mapel and R. J. Greenly, as well as consultant Tiffin. I was not merely allowed to introduce some research and changes in operational practice, but challenged and pressed to do so.

At that time I did not think myself to be a psychologist, but merely a bright clerk with a smattering of irrelevant training in statistics and in various social and behavioral sciences. I had never administered a psychological test, but soon was occupied in test validation and special test development, guided only by books at night and Tiffin on his occasional visits. Some of my validated test batteries and manuals for their use soon were adopted throughout the firm. I was a partner in a study of the employee merit rating procedures which, to my astonishment, got into print (Journal of Applied Psychology, 1942) and justified attending the 1941 regional psychological convention. I got involved, in a minor role, in a massive experimental program to screen employees for visual deficiencies, set up the data management system for this work, and did some analyses on the relationships of visual skills to aging, job performance, and accidents. I taught myself the mysteries of punch-card equipment, and the tricks for setting up codes and cards for easy analyses. For this work I took the train to Chicago one or two nights a week where I had midnight access to a supersorter that could actually count the cards.

These were heady times for me. I was ready to learn, stimulated by the immediacy of attention and action following my work, and very self-directing within the range of Joe Tiffin's suggestions and his quality control over my work product. It was also a time of belated growth in self-esteem and confidence: I discovered that I could do useful things, deal with people, earn a living, and tackle new problems on my own.

Within two years, my mentors, Greenly and Mapel, were summoned to head the corporate-wide personnel and training programs, and I was brought along to Pittsburgh with them, stayed through the war period. The work was an extension and enlargement of the kinds of work started in Gary. Sample tasks: Develop job performance tests, for skilled tradesmen trainees in naval armor layout and machining, that would allow certification by skill level achieved rather than by calendar time in training (cut training time in half); Develop and validate selection standards for female steelyard laborers (can read safety signs, optimum weight 165 pounds, etc.); Field test a group process for job and role redefinition among overloaded plant production schedulers; Prepare case materials for supervisory and management training courses. The latter examples are significant: They are precursors of a drift of professional concerns away from individual psychology toward issues of interaction, social processes, and formal organization.

The significant events in my professional identification and development during this period derived from contacts outside of my main employment. J. Stanley Cray, then professor at Pitt, asked me to write two chapters for his book, Psychology Applied to Human Affairs. Gray just assumed that I was a qualified psychologist, and that I could write to professional standards; I was not prepared myself to make either assumption. The chapters, in fact, were singled out for derogatory comment by two reviewers. I was asked, and accepted, to teach evening courses in personnel psychology at the University of Pittsburgh, and given an appointment as Lecturer in Psychology. I was visited at work by a few established psychologists (they had proper degrees and titles) who seemed interested and respectful of what I was doing. I was included in local professional psychological activities with such others as Ross Stagner, Dora Capwell, Glen Cleeton, Richard Husband.

During my final year or so with the steel company I found myself decreasingly engaged directly in research or research-like investigations, and increasingly drafted into administrative work and in-house consultations. During one interregnum period of several months I was acting as head of the corporate office personnel function - a position rather at odds with my experience, and with my slight seniority in a seniority-conscious firm. There was a clear choice to be made; I could go on in personnel administration, or resume and pursue work as a professional specialist in industrial psychology. As usual, someone else made the choice for me. Harold Wright of A. T. Kearney & Company, Chicago, made an offer I could not refuse.

A. T. Kearney & Company is a management consulting firm, then of some 30 professional staff members, a product of the fission of the prominent McKinsey-Kearney firm. Hal Wright should be better known among industrial psychologists than is the case. Most of his career prior to becoming a partner in the consulting firm was spent in personnel management and research with the Western Electric Company of the Hawthorne experiments fame. His name shares the title page of the 1939 report (Management and the Worker) along with Roethlisberger and Dickson. He had spent a long time deeply involved in innovative practices of psychological and social psychological themes. Wright was a man who could transmit both the philosophy and the practice. I worked with Wright for five years, the only psychologist with the firm. As it came about, Wright had his clients and I had mine, and we did not often work jointly. There were frequent occasions when I could display for him a strategic or diagnostic problem; his help was always supportive, insightful, and (it seemed to me) remarkably often correct in estimating organizational forces, interpersonal dynamics, and forthcoming events. My work became decreasingly concerned with individual selection and training, increasingly concerned with organizational policies, structures, and change.

More publications were then beginning to appear about the social psychology of organizations and about organizational change; I found them troubling and stimulating. I came to two ideas. First, I was working beyond my confident and comfortable competence. Second, I preferred to inquire rather than to dispense oracular solutions. I decided to go to some place where significant questions were being asked. The possibilities were three in number: Harvard Graduate School of Business Administration (Mayo, Roethlisberger, and others), MIT (McGregor, Myers, and company), and Michigan (Likert, Katz, Newcomb, et al.). There would have been a fourth possibility - Carroll Shartle's group at Ohio State - but I had not yet heard of their existence. Visits proved that Harvard was cold, MIT receptive, and Michigan warmly promotive. At last, at age 34, I made a proactive career decision. I became a student in a doctoral program operated jointly by the Michigan psychology and sociology departments, and took a part-time assignment as student research assistant in the Survey Research Center.

V

The transition back to being a student was quite easy, except for the economics of it, and seemed very much a continuation of what I had been doing but with a different mix and setting for the activities. I was plunged immediately into full time work on a new project - a study of communication in a Federal agency. We undertook to apply sociometric methods to understand the structure of interpersonal relations of the whole of a large organization. The approach paid off in this case, but proved to be technologically premature; the approach is currently (1978) in strong revival, now that improved statistical methods and data processing capabilities have removed the forbidding drudgery of manual computation. My colleagues in this study included Seymour Lieberman, Robert Weiss, and Eugene Jacobson. Jacobson, being senior in research experience and SRC tenure, was the group leader, and a good one, too.

In 1950 we were experimenting with forms of research project and program management. The staff of the Human Relations Program (later to become the Organizational Behavior Program) was small enough to allow us to try to do everything together. There were weekly meetings, all gathered around a large table, to review design options, measurement methods, field and analysis progress, reassignment of staff, and the like. Around the table there might be Daniel Katz, Floyd Mann, Robert L. Kahn, Nancy Morse (Samuelson), Gerald Gurin, Donald Pelz, Arnold Tannenbaum, and a half-dozen others. All but

Katz were graduate students. Occasionally Rensis Likert, Director of ISR, or Angus Campbell, Director of SRC, joined the group. The mood was one of innovation and discovery, with an overlay of ideology and mission. We thought that with a suitable theory, enough observation, and quantified measurement, complex organizations might be understood sufficiently to help make them more humane and more effective. At the same time, our immediate preoccupations were with specific theoretical and methodological issues. The main precedents upon which we were building came from Likert's early studies of organizational properties in the insurance industry, from Katz's wartime studies of work organizations, and from Newcomb's then recent work on socialization and role theory.

The doctoral curriculum in the social psychology program was undoubtedly, at the time, the best available, and the learning itself very much a group process: Daniel Katz on schools of social psychological theory; Theodore Newcomb on socialization and social role phenomena; Ronald Lippitt on small group observation and intervention; the Cartwright-Festinger-French-Zander seminar on group dynamics was in full swing (and continues to this day); Clyde Coombs was unfolding his theory of data; John Atkinson was extending his theories of motivation and action. The courses offered by the sociological faculty were less stimulating, perhaps because less pertinent to my concerns. At that time there was no one who could deal effectively with the structural and developmental issues of formal and complex organizations.

The doctoral degree came in due time, on schedule, and was totally painless. For a dissertation, I chose to work on a problem theoretically formulated from laboratory experiments on group cohesiveness but to be examined in the "real life" setting of a large manufacturing organization. Daniel Katz was my committee chairman. As I recall, I had just two meetings with him on the dissertation; one was to discuss and ask approval of the general plan of work, the other to get some help on an interpretation problem about which I was uncertain. His imprint, nevertheless, is upon that work, and I was glad to have a chance to try a bit of independent scholarship and to produce a product that satisfied me.

I had assumed all along that when my sabbatical-like interlude at Michigan was over I would return to management consulting or find some industrial firm willing to harbor an investigative psychologist. As soon as my dissertation project was started, I began in a leisurely way to explore possibilities. The first thing I learned was that the attractions of consulting, aside from the money and the chance to work in many different organizations, were small. The second thing learned was that few firms were instantly ecstatic at the prospect of getting the services of a psychologist who wanted to work on problems of leadership, group processes, communication and related social-psychological matters. Minnesota Mining and Manufacturing and Minneapolis-Honeywell, then growing firms, were bruskly impenetrable; IBM and GE, among others, were receptive to me but not to what I had in mind to do. This was the first and only time in my life I had actually looked for a job. Job offers were easy to get, but it was also easy to say "No".

As usual, someone else made up my mind and directed my career. Rensis Likert asked if I would stay on at the Institute for Social Research. The idea was that ISR needed someone with reassuring social science credentials to help, part time, with administrative and public relations chores and to conduct some personal research on the side. It looked like a good fit to me, and I accepted, bought a house to fit my expanding family, and stayed on for (so far) a quarter of a century.

The Institute for Social Research, of which the Survey Research Center is a part, is a curious hybrid form of organization. It is a part of a university, yet very autonomous in its fiscal base and internal operations. It is an academic institution with strong norms derived from and supporting the academic purposes and values, yet also a fragile economic enterprise that survives only by selling its unsubsidized services in a competitive market. It is not a teaching organization offering courses and degrees, yet most of its professional staff are engaged in formal teaching, and the institution takes pride in the large number of distinguished social scientists who are alumni of the Institute and who wrote their dissertations under ISR auspices. It is dedicated to inefficiency, in the sense that the pressures, self-imposed, are always to do more that the project time and money budgets allow. How can such an organization be "administered?"

I have never fancied myself to be a very effective administrator, although it has helped to have a certain orderly compulsiveness in doing necessary chores, and a readiness to assume that others will do their respective jobs well. The former attribute proved to be useful in an organization in which administrative intrusion is not particularlv welcomed, but where the buffering of staff from administrative nuisance is highly valued. The latter was suited to the tradition, and deliberate intent, that ISR should run itself out of the autonomous good sense and responsibility of the project and program professional staff. Actually, ISR has always been well supplied with spontaneous initiative and leadership, dispersed throughout its staff. I recall one period of more than six months when five of the top seven officers were absent, without visible loss of momentum and direction within ISR. The administrative tasks that came to me were of interesting kinds for the most part. I got out the annual budgets, prepared institutional reports, salvaged misplaced employees, coordinated the traffic of visitors, drafted an endless stream of policy papers to deal with emerging new problems, coordinated innumerable committees.

After a few years, I asked to be relieved of this work on grounds that with the growth of ISR it could no longer be handled on a half-time basis and I was unwilling to forgo direct engagement in research. I became head of the group engaged in organizational research, then composed of five professional staff, a half dozen student research assistants and a supporting staff of eight or ten. Aside from a later full-time spell of administrative service as Assistant Director of the Institute, I was, thereafter, primarily doing my own research.

VII

While at ISR I have carried out solo, or been a substantial partner, in about 30 research projects, all having some evident connection with organizational psychology. These studies have been diverse on several dimensions. A few were narrowly directed to an applied problem (e.g., improving the success rate in Peace Corps recruitment) while others were on topics of evident impracticality (do natural, uncontrived types exist among human groups?). The earlier studies were focussed upon individuals in their workplace relationships - i.e., in groups, in superior-subordinate relationships, in entry and exit processes, and in job performance. The later studies, in contrast, have spanned the range of levels of analysis from physiological phenomena related to working conditions, to the development of a conceptual basis for cross-cultural comparisons of work environments. My first three publications while with ISR dealt with theoretical topics identified by the term "group dynamics"; my latest ones have concerned the professional role of organizational psychologists (i.e., ethics; cross-disciplinary methods) and with a reconceptualization of the meaning of my favorite criterion -organizational effectiveness. There are several aspects of the things I have done, or helped to do, while at Michigan that have warranted note by my colleagues in I-O psychology, or that have given especial personal satisfaction to me.

One of the transitions in I-O psychology during these years has been the transition from independent, encapsulated studies, to the conduct of "programs" involving many parties, many theoretical perspectives, and some considerable span of time. My dissertation, 1953, had some flavor of this change as it was based upon a massive field study (done by others) designed in a way to produce data usable for a variety of analytic purposes; I mined that data bank to make a comparative study of natural work groups, N=228 groups, 5,900 individuals. In the ensuing years, impressed by the analytic power of large data sets and the representation of populations of organizations rather than of individuals, and impressed by the long-run effectiveness and economy of such strategies, and being in an institutional setting with resources to allow such work, I directed four large scale surveys of populations of organizational units.

This feature of my work is not very visible to others. There does not exist, for example, an overview report of the United Parcel Service Study, or the Northwestern Mutual Study. To find the products, one would have to search in libraries under several rubrics other than "I-O psychology." The products from these four ventures, so far, include 9 doctoral dissertations, about 25 technical reports (working documents or sponsor reports) and about 35 journal articles and book chapters. The latter is probably an underestimate, as the data from these studies have been used by people at other institutions and the publications are not always brought to my attention. No individual has read all of these products; I have not. They appear in books and journals of management, sociology, political science, psychology, and in several languages.

My sense of accomplishment and pleasure comes from having helped to design the topical content, the measurement operations, and the field strategies, in a way that could sustain such an amount and diversity of product. A number of people have gone rather far out of their way to explain to me the hazards and limitations of secondary analysis, and their views are valid. Nevertheless, in the span of two decades, secondary analysis has become respectable, and the archiving of social science data for shared use has become a small industry. I am presently engaged, with Edward Lawler, Cortlandt Cammann, and Barry Macy, in an effort to obtain and archive longitudinal data for a population of organizations that are undergoing purposeful, semi-experimental change processes. I don't know yet how that will work out as it strains the available conceptual and methodological capacities of our trade. The field work will require about eight years of time.

We are asked to mention in these autobiographies something of the aspects of our work that have had the most reaction from or impact upon our colleagues. Aside from the foregoing matter, I would mention three things. First, I have had a persistent interest in the meaning of "organizational effectiveness" and the means for assessing effectiveness. There are four frequently-cited published papers on this theme and a fifth in preparation. This work has been noticed, and embodied in the research of other people. Second, I have had a belief that we need to employ semiexperimental methods in organizational studies, and have contributed to the design and conduct of field experiments with complex organizational units; this theme is represented by two books reporting such experiments, two articles that have been of ten reprinted in collections of such papers, and is an integral part of the current work of a number of people. Third, I have become involved in the quality of working life aspect of the contemporary "social indicators movement" and contributed somewhat to both the assessment of trends within the United States, and to the conceptual and methodological problems of cross-national assessments. Recent publications of UNESCO, OECD, and of the Canadian Journal of Social Indicators Research represent this line of my work which has been carried out in response to some institutional or public demand.

VIII

There are three additional matters to be mentioned to round out this account of one person's way of being an I-O psychologist. These are related to the tasks of (1) preparing successors to carry on the work, (2) aiding current professional and scientific integration, and (3) building bridges to connect warring factions among I-O psychologists.

The Michigan Doctoral Program in Organizational Psychology evolved very naturally from the conditions of the mid-Fifties. At Michigan, we had a small cluster of graduate students who wanted to specialize in the social psychology of formal organizations; we had a resident cluster of qualified faculty doing research in the area; the existing Doctoral Program in Social Psychology (an inter-departmental unit), already large, was not eager to expand to accommodate such a special group and the Sociology component of that program was flatly opposed to the idea; The Department of Psychology consented to sponsor a special program, with arrangements for the development of a curriculum and the apparatus for program management, provided they did not have to pay for it. To this day, the Departmental budget provides less than 20% of program costs, although the social integration of the OP faculty into Departmental affairs is virtually complete. I have served as chairman of the OP Program during about half of the 20 years of its existence. Floyd Mann, Daniel Katz, and Robert L. Kahn were among the prime movers in starting the Program. The faculty, with the exceptions of Norman Maier, and (for a brief period) L. Richard Hoffman, have been drawn wholly from the ISR research staff. The Program has kept its original character of specializing in the social-psychological aspects of I-O psychology. The alumni roster now numbers 74 and there are currently 26 resident students.

I became involved in the affairs of the American Psychological Association by being drafted for minor committee service in Division 14, Industrial and Organizational Psychology. There followed in due course election to various offices of the Division including the presidency, and appointments to committees of the central APA organization. The latter included periods of service as member and as chairman of the Policy and Planning Board, and, later, of the Committee on Scientific and Professional Ethics and Conduct. A collateral activity was service for a number of years, including the chairmanship, of the Commission for the Certification of Psychologists, an agency of the State of Michigan. I do not recall any occasion when I rejected an invitation or appeal to take on such tasks. I found them worthy, personally rewarding in diversified collegial exchange, and a means for maintaining a small voice for I-O psychology on the larger scene of psychological developments.

IX

What next? One might think that being asked to write an autobiographical note signals the end of one's career, and that may be the case more than I am willing to concede. However, there is one matter that I find especially threatening, challenging, and significant for our field of I-O psychology, and I hope yet to help do something about it.

While there are several areas of productive dispute among us, one relating to the study of organizational systems stands out in my mind. The matter rests upon basic differences in the models or paradigms with which we work - our image of what it is we try to understand, explain, and influence. From this basis, the matter unfolds into specific, contemporary differences in teaching, in research strategies and priorities, and in the professional services that we provide. Readers of this page will need only a few code words and stereotypic phrases to understand the thought.

We have a history of significant accomplishments in describing the structures of organizations, and some of the microprocesses that are associated with desired and undesirable states and outcomes. We have only a short history, but some significant accomplishments, in attempting to treat groups and organizations as to their emergent, evolutionary, and hence complexly differentiated characteristics. We risk the emergence and institutionalization of opposed camps, with powerful ideas (e.g., those of Trist, Herbst, Thorsrud, Emery, to name a few contributors) arrayed against equally powerful ideas of others (e.g., Thompson, Katz & Kahn, Dunnette, J. G. Miller, to name a few). Some might think the differences are merely a matter of esthetic preference or transient tactics, but Golembiewski has recently proven otherwise in showing that as an organization changes it not only becomes different from its former state but becomes in some degree incomparable (JABS, 12 (2), 1976). To risk an analogy, it is as though the sciences of biological maturation and disease etiology were to attempt separation from anatomy and biochemistry, when their complementary natures need to be exploited.

My own intellectual origins and predilections are clearly one-sided in this matter, although not by deliberate choice. In the mid 1950's I served apprenticeship with the NTL network, with the aid of Bradford, Bennis, Shepard, and Blake. I spent a year, 1965-66, with Trist and Emery at the Tavistock Institute to try to gain some understanding in their methods of diagnosis and intervention strategy. In the late 1960's I worked on systemic intervention projects with Floyd Mann, who is a master of that art. All of my efforts came to little, and I concluded that direct interventions in the form of action research had better be left to others.

Nevertheless, I think the merger of approaches, both in practice and in theoretical formulations, will be needed, and will be accomplished in part by those who can alternate their style of work and in part by others, myself included, who can work in teams of mixed approach.

Autobiography of Stanley E. Seashore
Program Director, Institute for Social Research
Professor, Department of Psychology
The University of Michigan

May 1978

edited by Ross Stagner