

William A. Owens

SIOF President 1969-1970

(I) William Abbott Owens (Jr.) was born in Duluth, Minnesota on June 13, 1914, the only child of William A. and Sarah Jane (Hines) Owens. My father was also a psychologist, educated at the University of Chicago; and my mother was a newspaper woman before her marriage. After brief terms at the University of Rochester and at Cornell College, Mount Vernon, Iowa, my father moved to Winona (Minnesota) State University where he first became chairman of the Department of Psychology and subsequently Vice President. This pleasant, small town on the Mississippi river became my parents' home in 1920, and they never left it. I entered first grade there, obtained my high school diploma in 1931, and received a B.E. (B.A. equivalent) degree from Winona State in 1935 with a major in mathematics and a minor in biological science.

While I was in high school, college and graduate school, our family regularly vacationed at Burntside Lake, Ely, Minnesota. Here I learned an abiding love for the beautiful Arrowhead Country and its innumerable, primitive canoe trails. My fascination with it didn't help me to slip more easily into the role of a professional psychologist, but it has helped me all my life in keeping both academic matters and myself in reasonable perspective.

It is hard to say where career shaping begins, but one strong influence on me was my parents' gentle support and deep-seated conviction that I would make a wise career choice and become a competent professional. An effort to justify their beliefs constituted some strong motivation. With this behind me, and a Service Fellowship in hand, I enrolled as a graduate student in psychology at the University of Chicago in the fall of '35. For nine months I went through the motions without finding myself and then transferred to the University of Minnesota. Here I spent another year as a sort of "advanced undergraduate," but at the end of it, something happened. D. G. Paterson, my major advisor, and a man who influenced all of his students profoundly, let me have it. He said, "I think your Dad is a fine psychologist, but I'm not at all convinced about you." The medicine was just right! I got my act together and gave Pat no reason to complain again. In 1940 I received my Ph.D. with Paterson and Palmer Johnson (a statistician) as co-advisors. Formally, I was a differential psychologist with some specialization in statistics and counseling.

In reality, I had only three job opportunities. The first was as an assistant to one of the deans at Minnesota. However, the conventional wisdom of 1940 was that one should not remain on the campus where he had received his degree, and I therefore declined to pursue the matter. The second was at the University of Connecticut, where the ultimate choice was between Fred Mote and me, and Fred got the job. The third was at Iowa State, to which I went as an Instructor under interesting circumstances. It seems that the University of Oregon also had a job, but they would take a woman and Iowa State preferred a man. Accordingly, my contemporary, Dr. Leona Tyler, was recommended to Oregon, and the late Dr. Ray Hackman and I to Iowa State. Leona went to Oregon, and her subsequent distinguished career surely rewarded their nondiscrimination. Ray and I were interviewed together at Iowa State, but he was the prime candidate. Feeling no pressure, and striving largely to help Ray, apparently cast me in so favorable a light that I was offered the position.

At Iowa State in 1940 the psychology program was essentially a service enterprise. The department had no undergraduate major, although it was permitted to confer an occasional master's degree. For two years I attempted to teach a broad spectrum of courses that ranged from Mental Hygiene to The Psychology of Advertising, and that involved loads up to 17 quarter hours. I didn't do much research, but the years were not uneventful. On July 26, 1941, I married Barbara Louise Ramsey who has given great pleasure, stability and purpose to my life through all the ensuing years. As fate would have it on December 7th, we had just returned from a Sunday afternoon movie about Sergeant York, a hero of World War I, when a radio newscast advised us that the Japanese had bombed Pearl Harbor. Two days later, I applied for a commission in the U.S. Navy. This ultimately led to my going on active duty almost a year later.

For most of the ensuing three years I was attached to The Bureau of Naval Personnel (Bu Pers) in Arlington, Virginia where my time was devoted to test construction and to both selection and classification research. It was almost an internship in personnel psychology, and the experience could hardly have been better for a young man only two years beyond his doctorate. There were stimulating colleagues to share problems with, both in uniform and out. An assuredly incomplete list of the former would have to include such names as those of Alvin C. Eurich, Dewey B. Stuit, Guy L. Bond, Daniel D. Feder, James W. Maucker, Royal F. Bloom, Everett C. Brundage, David G. Ryan, Rutherford B. Porter, Harold P. Bechtoldt, Ray N. Faulkner, Walter F. Johnson, Howard T. Batchelder, Joseph B. Cooper, A. Eason Monroe, Gerald V. Lannholm, C. Robert Pace, James F. Curtis, R. B. Embree and Kenneth E. Clark all within the Tests and Research Section of Bu Pers. In addition, a number of established professionals in the field were attached to the National Defense Research Committee Project N-106 or the College Entrance Examination Board and were in and out of Bu Pers as the situation dictated. Such names as the following come readily to mind: Herbert S. Conrad, Norman Frederiksen, John M. Stalnaker, Henry Chauncey, Harold O. Gulliksen, Nicholas A. Fattu, Joseph Miller, George Satter and Ledyard R. Tucker. Of course it was an exciting time. In retrospect, I learned many things, but perhaps most importantly I learned how to implement some of my ideas, and which of my professional skills were really useful and which were not-in at least this context.

As the war wound down I found myself considering several academic jobs and one in full-time consulting. However, in the end, (then) Dean Harold V. Gaskill made me an offer, which was fabulous by pre-war standards, and I returned to Iowa State as an Associate Professor of Psychology in January 1946. In what was clearly a "seller's market," I then became a Professor in April of that same year; and ultimately, in 1947 at the ripe old age of thirty-three, I added the title of Head, Department of Psychology. In the language of the gag writers, if I had it to do again, I wouldn't. I mean only that it seems to me that I should have better established myself as a professional before venturing into administration.

In any case, the succeeding thirteen years at Iowa State were eventful and enjoyable. The department added some new staff members, received permission to grant an undergraduate major, and implemented a well-rounded M.S. program through which degrees were conferred on such notables as Jay Uhlener, Bob Boldt, Duane Thompson, Daryl Nichols, Bob Morrison, Dave Campbell, Jack Larsen, John Campbell, Paul Wernimont, and many others. To the best of my knowledge, none of our M.S. recipients who undertook the Ph.D. failed to receive it, and a number have distinguished themselves. It is a record of which all members of the department were and are proud.

Last, but not least, three years into my tenure as head our son, Scott Ramsey Owens, was born. He is now in the practice of corporate law in the city of Atlanta, but his area of specialization is labor relations and EEOC compliance. This is close enough to some areas of industrial psychology to keep his father a bit more current and alert than might otherwise be the case.

In retrospect, I view my research as having been in three areas: first, the measurement of high-level or professional aptitudes; second, the appraisal of age-related changes in mental abilities and their correlates; and third, explorations with scored autobiographical data, which have resulted in the development and evaluation of a conceptual model for such work. All of these were begun at Iowa State. Thus, construction of the Mechanical Comprehension Test, Form CC, was initiated while I was in the Navy, but concluded in the context of the selection of engineering students at I.S.U. Similarly, the Veterinary Aptitude Test came into existence basically because only about 20% of the applicants could be admitted to the College of Veterinary Medicine during the early post war years. An even more definite local stimulus existed for the development of the Tests for Creativity in Machine Design. (Then) Dean of Engineering J. F. Downie Smith was well aware of some disconcerting facts known also to the Department of Defense; namely, that well over 90% of the mechanical innovations useful to our just concluded war effort came from heads not educated in this country. The Dean, therefore, obtained a research grant from the Office of Naval Research directed at finding more appropriate ways of identifying and educating students with creative potential in the area of machine design. I became involved with the Dean in the first phase of this effort.

My work on age and mental abilities was initiated through a happy circumstance. In 1947 I was teaching a course in individual differences using Leona Tyler's book on the subject as a text. She clearly pointed out the absence of longitudinal evidence regarding the effects of aging on mental abilities. At about the same time I was cleaning up an attic used by our department and discovered that my predecessor, Dr. J. E. Evans, had carefully filed away some of the scores of students who had taken Army Alpha as a freshman entrance examination in 1919. In the context, the notion of retesting them was unavoidable. I began by writing several notables in the area for reactions and guidance. Dr. Sidney Pressey at Ohio State wrote a most helpful letter and suggested the collection of some personal data hypothetically explanatory of certain of the anticipated changes in test scores. His suggestion was incorporated in a research proposal later funded by the Office of Naval Research. Ultimately, I had the pleasure of reporting our work at the Gerontological meetings of 1953 in San Francisco on the same program with Dr. Nancy Bailey who reported comparable outcomes for subjects from Terman's gifted group. I believe these two were the first substantial reports based on longitudinal evidence and indicating that mental abilities might very well improve thru age 40 to 50 rather than decline as suggested by cross-sectional studies.

Finally, in the matter of the biodata research which still engages me, it was in 1955 that my good friend the late Dr. Edwin R. Henry invited me to the so-called Educators' Conference on Employee Relations Research held by the Standard Oil Co. of New Jersey. The format called for a day or two of orientation to The Company, a week in the field, and a pro or con report back to The Company. I asked to stay in New York and review the home office's work in personnel research. My request was granted and I had the great privilege of learning, among other things, the history of biodata research from one of the men who made it. The topic was very current because of the inclusion of a biodata form in a battery of tests being constructed and assembled for a large, multi-company study devoted to the early Identification of

Management Potential. I returned from the conference and began some biodata research, not yet sensing where it would lead me.

As the years wore on at Iowa State, I became increasingly aware of a need to be affiliated with a doctoral program and with student candidates for that degree. Thus, when an offer came from Purdue University to join the industrial psychology program there, it seemed that the time and the opportunity had come. Accordingly, in 1959 my family and I left Ames, Iowa and moved to Lafayette, Indiana. Probably all changes are stimulating, and this one was no exception. The industrial program, among the best in the country, was big, well-established and moving like a broad river. With that which I had chiefly sought I was almost over-blessed; the graduate students were able, goal-oriented and numerous. I enjoyed them tremendously and recognized them as a virtually unmixed blessing. Consulting opportunities were often present, and many of them required research which could be performed by a graduate student at regular stipend rates and under only nominal supervision. I have always believed that business and industry cannot buy such competent help for so little money in any other way, and that graduate students can't find a better quasi-internship experience.

Time passed rapidly at Purdue. I arranged for a second retesting of the subjects of the age and mental abilities study, and the results were coherent with those obtained ten years earlier. I also arranged for a fairly comprehensive study of the predictive validity of the tests for creativity in machine design with generally satisfactory results. More and more, however, I found myself intrigued with and working with biodata. By 1965 Dr. Michael Driver and I had put together a substantial research proposal which was funded in 1966 by the National Institute for Child Health and Human Development and which continues as this is written. Unhappily for me Dr. Driver moved to the University of Southern California a year later. The research endeavor itself prospered in spite of this and carried me more and more in an interdisciplinary direction. When the University of Georgia approached me, I agreed to visit with a half-closed mind. What I found was a university in transition which offered great flexibility, strong support for research, and a fine opportunity to shape a place for oneself and to "make a difference." Once more we reached a family decision to move, and did so during the late summer of 1968.

Again, the change was dramatic. Psychology was well established at the University of Georgia, but there had thus far been no program in (applied) measurement and human differences, and it was this program that I was to initiate. Within a year we had added to our faculty Dr. Lyle Schoenfeldt from Purdue University, via A.I.R., and Dr. William Love from the University of Illinois. By the time a second year had passed we were beginning to become acquainted with the first members of a continuing stream of the fine graduate students with which our program has been blessed. We shortly added Dr. Robert Lissitz to our group, Dr. Jorge Mendoz replaced Dr. Love, and, some time later, Dr. Ed O'Connor joined us briefly. It was with Lyle, however, that I chiefly interacted. He had taken his degree with me at Purdue and the relationship was a comfortable one. I shared his interest in the Measurement and Human Differences (M & HD) program, which grew to some 17 or 18 students, and he shared my interest in biodata research. Indeed, he became a co-investigator on my N.I.H. grant and an invaluable asset to the entire undertaking.

The University of Georgia had for some time had a so-called Social Science Research Institute. During early 1970 I was asked whether I would like to be considered as a candidate for its directorship. When I discovered that I could retain a working relationship with the M&HD program in Psychology, I said that I would. Thus, Lyle headed that program and, in the Fall of 1970, I became the Director of a rechristened

Institute for Behavioral Research (I.B.R.). Formally, I was one-third time in research within this institute, one-third time in teaching, and one third time in administration. Ignoring all of the formalities of structure and functioning, it has been a broadening, humbling and fascinating experience to watch able people from such fields as Sociology, Political Science, Psychology, Management, Educational Psychology, Economics, Education and Geography attack a common problem. All the fields share some methods, convictions and practices; there is, nevertheless, enough uniqueness so that each can learn from the others and all can develop greater breadth, sensitivity to problems, and analytical skills. Thus, for example, Dr. Lyle Schoenfeldt (Psychology) and Dr. James Ledvinka (Management) have both won the Cattell Award while affiliated with us, so the level of stimulation has not been low.

To me, personally, the I.B.R. context has been an invaluable aid in my research. Thus, we have been assigning college students to subsets based upon the patterns of their pre-college experiences via biodata. It has, then, been our amply reinforced hypothesis that persons who have behaved comparably in the past should continue to do so in the future, and in a wide variety of extra-biographical domains. So far, under I.B.R. aegis, investigators from Political Science, Sociology, Education, Counselling, Psychology, et. al., have put us to the test. That is, they have selected subjects for their own studies by sampling our subsets. Naturally, certain advantages accrue to them, but the feedback indicating whether or not there is differential behavior by subset has been vital to us - and it does exist in some 80% of cases.

Since 1970 only one event has marred the tranquility of life at the University of Georgia. In 1976-77, I was asked by President Fred C. Davison to serve for one year as acting Provost (and subsequently as Senior Faculty Advisor to the President) for the express purpose of reorganizing the upper administrative structure of the University and of finding an individual to become our most senior vice president. Our proposed reorganization was unanimously approved by the Board of Regents, and we subsequently were fortunate enough to attract to the University of Georgia the talents of Dr. Virginia Trotter. Dr. Trotter had been a highly successful Vice President for Academic Affairs at the University of Nebraska prior to serving for three years as Assistant Secretary for Education of the Department of Health, Education and Welfare. Ostensibly then, the year was a successful one, but the experience told me again what I thought I already knew, namely, that I would never choose to devote my efforts to full-time administration at that level.

Given this chronology, largely turning about research, it now seems appropriate to comment briefly regarding my involvement in some other phases of academic life. Teaching has, of course, been close to the core of my activities for nearly forty years. By and large I have greatly enjoyed it and believe I have been reasonably successful at it. I am sure I am more alert to problems and do better research when I also teach. In addition, as in the age and mental abilities study, the actual research problem may be posed in material read for a course. On the always controversial subject of teaching versus research, then, I am clearly an interactionist who believes that each benefits the other.

A very special kind of teaching is that involved in the direction of graduate students undertaking a thesis or dissertation. I have now had the privilege of working with over 100 of such students, and I have almost invariably found the relationship both pleasant and stimulating. It is hard for the individual himself to guess how effective he may have been. For better or worse, I have tended to try to find a real mutual interest with each student, and to indicate my full confidence in that person's ability to solve a given problem and to make a worthwhile contribution to our discipline. The end product has been

theirs-not mine! Indeed, for every problem I have identified and turned over to a graduate student I have gotten back not only an answer but an expanded list of problems. Of all the activities involved in academic work it seems to me that the direction of research is most unique. If I did not enjoy it, I would be elsewhere. Looking back at it from the far end of a career it is both sobering and satisfying to contemplate one's possible impact on the next generation of professionals-hopefully for good!

In the realm of service, it strikes me that the best thing I have done is to develop some personal consulting contacts. My time commitment to them has been modest, and I have tried to avoid doing anything which a graduate student on a standard stipend could do as well. Thus, these contacts have tended to provide not only invaluable experience and financial assistance to graduate students, but a high quality of responsible service to business and industry as well. In addition, they have in effect extended academic budgets by providing a non-appropriated source of student stipends. Although these appointments were not systematically established as internships, they have served much the same function. The tangible product was commonly a thesis or dissertation, but whether or not it led to one of these, the student almost always valued the "real world" contact, and not uncommonly found that it provided a valuable credential when he entered the job market.

If I were to try to identify the influences which have made me an industrial psychologist, per se, I would first have to recognize my great indebtedness to Professor D. G. Paterson, who clearly believed that individual differences and measurement form the basic cornerstones of the entire structure upon which all of the applied sub-disciplines rest. Next, I would acknowledge my predecessor at Iowa State, Dr. J. E. Evans, who provided many particulars, and who had a deep and abiding faith in the ultimate value and utility of our procedures. Finally, I would, of course, want to recognize my friends and colleagues at Purdue where all of this came together in a program operated under the long and faithful stewardship of the Drs. Joseph Tiffin and E. J. McCormick. It was here that I first became involved in the professional affairs of Division 14, an involvement which ultimately and ironically led to my becoming its President in 1969-70-after I had left Purdue.

Since the year 1969-70 was an eventful one for Division 14, a comment or two regarding those events which made it so may be in order. First, it was during this year that the membership voted to change the division's name from The Division of Industrial Psychology to The Division of Industrial and Organizational Psychology. Second, under the aegis of Paul Thayer, a practice was established of awarding a clear lucite desk piece, appropriately inscribed, to all past presidents in commemoration of their contributions to the division. I drew the most pleasant assignment of presenting one of these to each of thirteen past presidents in attendance at the annual meeting. Included in this number was Dr. Bruce K. Moore, the first President of our Division. Third, indicating our desire to be responsive to the concerns of the times, we created a committee designated as the Committee on Public Policy and Social Issues which persists and appears to function well.

Ultimately, perhaps each of us who searches the forgotten corners of his mind for some of the details of his own career development should be known at least in part, for his "position on the issues." But what, then, are the issues? There may be very nearly as many lists as list makers. Let me mention only three items which stand high on mine.

First, there seems to be more than a modicum of truth in the sometimes sardonically voiced opinion that sociology deals with an empty organism in a structural field and psychology with a structured organism in a bland environment. Mischel () for example, has clearly recognized that trait concepts

do not permit the effective prediction of behavior across differing environmental contexts-an awareness shared by Hartshorne and May () some years ago. Unquestionably, for the I-O psychologist this means a more difficult undertaking, but perhaps a more rewarding one. Personally, I see no reason why we cannot identify the essential dimensions of contexts, and cluster environments in terms of their profiles across these dimensions. Conceptually, then, one can envisage the prediction problem in terms of kinds or levels of persons on one side of a matrix, kinds or levels of environments on the other, and criterion variables within cells. Thus, outcomes derive from the combined influences of both sets of variables. The particular paradigm is unimportant, but the fact that we need to contemplate both person and content variables seem undeniable.

Second, I believe there are many reasons why we need to deal more effectively than we have with the problem of classifying persons. Not only is it one of our obligations as a science; but, in the absence of a reasonable rationale and procedure for so doing, we cannot even respond convincingly to the question of the man in the street who asks, "what kind of person is John Jones." Closer to practice, it seems that our social convictions, supported by legislation and regulatory guideline, are pushing us further and further from personnel selection and ever closer to classification. Specific solutions are demanding and tend to lack the generalizations which build a science. There would thus seem to be much to recommend a more general solution in terms of a tentative and flexible definition of "kinds of persons." Of course the diminution in predictive accuracy as compared to a given specific solution must not be too great, and we must be able to assume that scores on a wide variety of post hoc criteria will reveal strong differential affinities for the kinds or families of persons identified. It is my view that such families can be identified from their differing mean profiles across the factors of a biodata form systematically designed to cover the salient dimensions of the typical subjects prior experience domain. As this is written, I have submitted for publication a research monograph entitled, "Toward a classification of persons" in which it is suggested that appropriate methods are available and that results to date look promising.

Third, as Dr. Seashore has suggested in his biographical sketch, I too believe we are in danger of splitting industrial psychology into that which is individual vs. that which is group in orientation. This would be an unfortunate and unnecessary schism which seems to be in no one's interest. If it is true that we are near a fork in the road, then it is perhaps in part because it is difficult to offer even a partial explanation of group phenomena at the individual level. If this is so, it may in turn be because we have had no comprehensive, general set of labels to apply to the various subsets of individuals concerned. Thus, if the efficiency of a given size of group assigned to a given task is to some extent conditioned by the mix of individuals involved, we may be slow to recognize this fact in the absence of a comprehensive and meaningful way of identifying those individuals. I, of course, see this issue as related to the previous one of classification, and would hope that an appropriate way of identifying kinds of persons might constitute a bridge between our individual-level and group-level explanations of behavior.

In any case, I suspect that these issues will not be clearly dispatched during the period of my active identification with I-O Psychology. I, nevertheless, look forward to their solution and wish great success to those who will attack them with brighter eyes and sharper tools.

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