AUTOBIOGRAPHICAL STATEMENT

Otis Dudley Duncan

My parents were both members of large farm families in northeast Texas in the early years of this century. Although their levels of living were low and their upbringing arduous, their aspirations were high. They met at college during World War I, so that I was a member of the "baby boom" that flared up briefly after the War. My father became a public school teacher, principal, and superintendent to earn his livelihood while trying to pursue graduate studies. We moved around Texas a good deal as he sought better jobs and spent summers at college. Finally, he was able to enter upon Ph.D. study at the University of Minnesota, where he was strongly influenced by Pitirim A. Sorokin. But the sojourn came to an unhappy end, as my father was failed in his attempt at his preliminary examinations. There is a great deal more that could be told about his struggle for an education, but to cut it short, I mention that he received his Ph.D. at the same time I received my B.A., both at Louisiana State University in 1941.

In the meantime, in 1929, my father had joined the faculty of Oklahoma A. & M. College (Stillwater, Okla.) -- later called Oklahoma State University -- as a rural sociologist. In 1936, he was asked to form a department of sociology, and he remained its head until his retirement.

Until I was eight years old, therefore, I was "on the road" with my parents, the son of a school teacher, graduate student, and beginning professor. For the next eleven years, I grew up in a quiet college town, more or less out on the cultural frontier of America. (Oklahoma was the 46th State admitted to the Union, in 1907, a recent event as of the time we moved there.) I entered college at Oklahoma A. & M. in 1938, and shortly thereafter resolved to major in sociology. One never knows his own motives, And, in my case, I tend to have near-total amnesia for the events of childhood and youth, not to mention many occurring since. Thus, it need not be taken at face value if I state that occupational inheritance is not the direct explanation of my entry into sociology. Instead, I suspect the major factor was friendship with and admiration for William H. Sewell, who is now a distinguished Professor of Sociology at the University of Wisconsin. In 1937 he was a fledgling member of my father's department, a close friend of the family, the proud possessor of a beautiful and charming wife, Elizabeth, and soon to be the parent of three attractive children. I mowed the lawn and did heavy chores for Liz, baby-sat with the children, and from time to time got tidbits of wisdom from Bill. In the summer of 1939, I took my first two courses in sociology, one from Sewell. His teaching was orderly and informative but not, as such, inspiring to me. What was exceedingly stimulating was that he referred me to the current polemical literature in sociology, which revolved a good deal around the question of whether and how the discipline could be made into a science -- the answer of George A. Lundberg, in Foundations of Sociology, being that we must seek rigor and reliability of observation, perfect our instruments of measurement, and attack the testing of hypotheses with research designs that had proved robust and productive in the natural sciences. I must have discussed all these issues with Sewell since he claims to remember as much; my own memory is exceedingly vague.

Through the accident that my father finally found a way to earn his doctorate by spending a year at Louisiana, I transferred there my last year in college. (I completed the baccalaureate in three years by attending school every summer -- a real grind, I suppose, but it was better than finding a job!)

It was less expensive for me to accompany the family to Baton Rouge than it would have been to complete college some other way. By comparison with Stillwater, Baton Rouge was a liberating experience, and I found especially gratifying the participation in the musical life of the campus and contact with students whose literary and political interests were broader than mine had been. I remember with affection the teaching of Rudolf Heberle, a refugee from Nazi Germany, and the son-in-law of one of the founders of sociology, Ferdinand Toennies. Heberle's lectures stood me in good stead when later I had to write prelims in "social theory" at Chicago; but otherwise he did not particularly influence me in any way that I could recognize. I was still on the "science kick," stimulated by Sewell and Lundberg. Not much could be done on this score at L. S. U., although I did learn some rudiments of demography from T. Lynn Smith, which later proved useful. I also completed college courses in French and German, as my father (from experience) had warned me that languages could be a stumbling block for a graduate student. He had also advised me to take as much mathematics and statistics as my schedule would permit. This I did, even though it did not amount to much on any absolute scale. Still, I had more exposure in college to these subjects than, say, 95 per cent of sociologists in my birth cohort.

In 1941, with war imminent, I went to the University of Minnesota, largely at Sewell's suggestion and, no doubt, in consequence of his recommendation. It was hardly at my father's bidding, since his memories of Minnesota were bitter indeed. I was luckier. I hit it off well with F. Stuart Chapin, who had been my father's nemesis 14 years earlier. Chapin was very much in the Lundberg camp and interested in what, for the time, were rather exotic topics in sociological methods. What I heard from him reinforced my adherence to quantitative sociology. Unfortunately, it was easier to acquire enthusiasm than to gain competence. I have trouble stating what, if anything, I really learned at Minnesota. The timing of the military mobilization after Pearl Harbor was such that I was barely able to finish an M.A. degree before being drafted, at the tender age of 20. My thesis was a routine demographic analysis. It showed, using a set of vital statistics compiled under my father's direction in Oklahoma, that rural women in that State began child-bearing at an earlier age than urban women. At my father's suggestion, I published an article based on the thesis in Rural Sociology in 1943, after I was in uniform. Apart from some poems in the L.S.U. student newspaper, this was my first publication. Again, I credit my father's good advice to the effect that it is wise for professional purposes to publish early and often. But the publication itself is no scientific landmark. Quite a few years later, some time in the early 1950's, I went to Northwestern University for interviews relating to a possible appointment on their faculty. The proceedings got off to an inauspicious start when the crusty old dean, Simeon Leland, looked at the first title on my bibliography and roared, "Now why in hell would anyone write on a subject like that?" My only answer was that I wanted to finish a degree before I was called to the army. I guess it didn't satisfy him, because I was not offered a job at Northwestern. Since then, I have always prepared my bibliography in reverse chronological order, so that you have to look at the end of the list of papers to find the one that set off Dean Leland.

I was lucky during the war. I travelled a lot in the United States, but was never out of the country or even involved in serious combat training. One nice interlude was six months at the University of Iowa, in the Army

Specialized Training Program in Personnel Psychology. Later, I had a little chance to ply my trade as a clinical psychologist in the Army, interviewing soldiers who had cracked up under combat. The last few months were spent in training and sitting around, waiting for an assignment that never came, in the Office of Strategic Services. But the main thing that three years in the Army did for me was to make me eligible for three years of study under the G.I. Bill at the institution of my choice.

For reasons that I can no longer recall -- more of that amnesia -- I decided on the University of Chicago. I think I was attracted because at Chicago there were people like Louis Wirth, who wrote on "sociology of knowledge," which sounded mysterious and intriguing, although I never learned much from him or anyone else about it. I was presumably drawn as well to W. F. Ogburn, on account of his reputation as a statistical or quantitative sociologist. In any event, he became my favorite teacher and, in many ways, a role model. I have written about Ogburn's work and, to a small extent, his life in the preface to William F. Ogburn on Culture and Social Change: Selected Papers (1964) and in "An Appreciation of William Fielding Ogburn," Technology and Culture, Vol. 1, Winter 1959. All the ideas of Ogburn that I summarize in these essays were influences on me, in one degree or another. But Ogburn was an elderly man when I first encountered him, and somewhat distant and formal, although cordial, in manner. Had I been closer to him in a personal sense, I might have become his disciple; as it was, I was his student and am no one's disciple.

At Chicago, we learned the "Chicago tradition" of sociology -- to which Ogburn, by the way, was in a measure an outsider. It includes a sensitivity to the empirical side of sociological inquiry, whether one works with documentary and historical materials, carries out quasi-ethnographic field work, or conducts statistical analyses of demographic data. We were never oppressed by the notion that our mission in life was to bring revealed truth to the ignorant, whether in the form of "theory" (as at Harvard, in those days) or "method" (as at Columbia, contemporaneously). Instead, we learned to root around for social facts, and to try to make sense of them in terms of a rather catholic collection of sociological concepts. Nor, in those happier days, was it considered mandatory that a sociologist continuously signify his dedication to some form of social reconstruction, political program, or ideology of secular salvation. It was enough to carry out one's inquiry in good faith, to the best of one's ability, and trust that the diffusion of such knowledge as one might win would be to the long-run good of the society and one's fellow man.

Toward the end of my doctoral study at Chicago, a dynamic young professor, Philip M. Hauser, came on the scene. He had been a brilliant graduate student at Chicago himself, and had then had a career in the Federal statistical agencies, winding up as Deputy Director of the Bureau of the Census. He was full of big news about sampling of human populations (introduced for the first time in the 1940 Census; I am referring to the year 1947) and about the expansion of topics and tabulations in the census. (Again, for the first time in 1940, there were questions on educational attainment, income, and short-period migration.) I studied demography with Hauser, more to experience his style than because of any firm intention to specialize in the field. But my dissertation, an extension of Ogburn's earlier work on variation in characteristics of cities according to size, used a good deal of demographic data and technique. Hence when listing potential teaching specialties in applying for a job, I included population as a possibility.

By chance, at Pennsylvania State College, where I started my academic career in the fall of 1948, there was much interest in having work done in this field. Hence, it became my main preoccupation for the next few years. A fortunate aspect of the appointment at Penn State was that it carried some explicit responsibility for the conduct of research and provided some modest resources for this purpose. This was quite a rare thing in those days. (I attribute my good fortune in locating so desirable a post to a recommendation made by my first mentor, Sewell.) I carried out one rather pedestrian demographic study, improving a bit on techniques used earlier by T. Lynn Smith to infer levels of fertility in the village population. A real "break" came shortly thereafter. A study of social stratification had been planned by the Department of Rural Sociology (where my research appointment was located); but at the last minute, the person designated to plan and direct it had to remove himself for reasons of health. I was asked to take charge. With the aid of my first really capable graduate student, Jay Artis, I planned the study and got it under way in a matter of days. It was essentially flying by the seat of my pants, for I did not know that much about the subject or have that many ideas about what should be done on it. I did take advantage of my acquaintance with Sewell's work in devising a scale for measuring socioeconomic status. Negatively, I was inspired by the example of W. Lloyd Warner, whose work I despised and from whom I had had two terrible courses at Chicago, to try to show what nonsense his ideas about "social classes" were. In the event, there were a few constructive things in the study, and a good deal of waste motion.

It has been suggested that in this statement I mention my "first contribution to science" and also that I list "discoveries which you regard as the most important and the circumstances under which they were made." This is a difficult requirement for me. I do not use the word "science" lightly, and sociology as a whole and my work in particular have relatively little to offer that I would call "science" in the presence of heirs to the work of Kepler, Galileo, Mendel, Mendeleev, or Darwin. Hitherto, I would not have had the nerve to lay claim to any "discoveries." Of course, I have published voluminously, and have amassed piles of what sociologist call "findings." But many of these are merely redundant with or extensions of common sense or the discipline's conventional wisdom. If one would reserve the term "discovery" to a finding that makes a difference in how people thereafter understand some pheonomenon of nature or man, then I don't know if any "discoveries" should be credited to me. (I know that election to NAS is on the basis of "discoveries," not prolific authorship and the like, so that I have some trouble understanding how I find myself in the position of recording these experiences and thoughts for the benefit of NAS files.)

But perhaps there was one real discovery in the Pennsylvania stratification study; its importance seems greater in perspective than it did at the time. We had asked local people to rate the social standing or prestige of their acquaintances in a little rural community in central Pennsylvania. We had also ascertained certain objective information, such as the occupations of the members of the community. We classified people by occupation, using standard census categories, and computed mean prestige status for each occupation group. Then, it occurred to me to compare these means with the means of occupational prestige as ascertained in a famous 1947 national survey, in which respondents rated not persons but merely the titles of occupations. The two arrays of means agreed almost perfectly. This showed, I argued, that the "local" stratification system was not much more than a microcosm of the national system. Thus, I considered that I had produced evidence in favor of the "mass society" argument that was attracting a good deal of attention at the time. What now seems much more important is that I had stumbled upon an invariance in occupational ranking -- in this case an invariance across populations and also across methods of eliciting the rankings. In later work I have shown similar invariances, when rankings are derived from (a) data on residential segregation patterns in large American cities, (b) patterns of intergenerational occupational mobility, (c) frequency of intermarriage between occupation groups, defined in terms of the occupations pursued by fathers of bride and groom, (d) education and income levels of the incumbents of occupations. Other researchers have shown that the survey rankings of occupational prestige are very close to invariant over time (up to 50 years), space (both developed and less developed countries), and social location of respondents supplying ratings. Some of the analysis leading to this last conclusion was carried out at my suggestion in a work of which I was co-author. The proof of the temporal stability of prestige ratings was provided by one of my finest students, who had earlier worked with me on occupational mobility and socioeconomic characteristics of occupations.

To continue with the chronological account, I completed the writing of the Pennsylvania stratification research only after moving to the University of Wisconsin, drawn there by Sewell, who was by this time the most prestigious sociologist on their faculty. At Wisconsin I learned some more statistics and demography, by virtue of having to teach these subjects, and also studied a little bit of human genetics, to be able to cope with the literature on "population quality." (Throughout my career, I have had to work hard to make good my undergraduate deficiencies in basic science; I had very little of it, and that little not adequately presented.) I did not accomplish much else there, but in any event within nine months I was on my way to Chicago, having been called back to my alma mater by my old professor Ogburn, to work primarily with his younger colleague, Hauser.

Hauser had just succeeded in getting one of the first "big" grants for social research of the post-war period. It had to do with urban morphology. I dusted off the ideas that had accompanied the work on the doctoral dissertation, re-read my notes on "human ecology," and began playing with Lorenz curves. It shortly seemed that I had hit upon a general strategy for discerning order and establishing relationships among series of data for small areas in a big city. The work on "cost-utility" curves (our term, for reasons too lengthy to mention, for Lorenz curves of concentration) and "segregation indexes" was shared, almost from the beginning, with Beverly Davis. I had met her at Penn State, where she was the smartest student of the lot, although at that time not professionally motivated as was Jay Artis. During my year at Wisconsin, she studied at Chicago, where she was well appreciated by Ogburn and Hauser. Most of the reports in the Urban Analysis Series issuing from Hauser's project are by Duncan and Davis, for we were not married until 1954. Probably our biggest single thrill of discovery was that of the "Beverly-beta matrix," as we call it only in jest, in private conversation. It is the triangular matrix of indexes of dissimilarity between occupation groups that has a beautiful simplex form. She discovered this regularity by shifting cards around on the carpet of an evening trying to tease out a pattern. Once she had that one licked, I figured out how we could show the isomorphisms with similar matrices generated from occupational mobility data and with the prestige and socio-economic rankings of occupations. Duncan & Duncan, "Residential Distribution and Occupational Stratification," where we finally recorded these results, has been "reprinted almost everywhere," as one of my young colleagues said recently.

The work on indexes of segregation led us, after the Urban Analysis Project was concluded, into a substantial study of Negro residential patterns in Chicago. At that time, for several years, a major source of research support was the City of Chicago, through its agencies concerned with planning and housing. But the applied aspect of our work was not uppermost in our minds. We were interested in discerning patterns, in -- as I once put it -- generating some cold statistics on the hot topic of residential segregation and the spatial expansion of the Black Belt. In Chicago, we found a rather inexorable process of succession at work. But this was no new discovery. Indeed, my main interest throughout this period was not in discovering new principles of urban form and process, but in testing and verifying the rather loosely formulated inquiries and insights of the pioneer human ecologists who had used Chicago as their "social laboratory." Our work on occupational segregation, thus, was seen as a confirmation of R. E. Park's insight that "spatial distances reflect social distances," a proposition that he could make plausible, but did not know how to confirm. I suppose much of my reputation in sociology is as a "methodologist" (a term that I dislike), because I have so frequently been in the position of offering a more rigorous and more quantitative approach to research on topics where the significant insights have already been attained by sociologists using less formal techniques.

After the "Negro book" (as we call it in private) was done, we were asked by Harvey Perloff -- an economist and city planner we had known at Chicago, who had moved to Resources for the Future -- to work in collaboration with a group of young economists he had put together on the general topic of regional economic growth and development. This was the first of several occasions on which I have been at rather close quarters with economists; these contacts have always been problematic for me. On the one hand, economics is vastly more highly developed than sociology as an abstract theoretical scheme and in terms of quantification. But on the other hand, economists sometimes seem to be just downright ignorant and insensitive about the most rudimentary aspects of human social existence that a sociologist knows about by the very nature of his discipline. But I have learned a great deal from economists and, on occasion, have been able to repay some of the debt. The first project with Perloff was not such an occasion. We did a rather uninspired study of geographic variation in certain economic indices, making use of the system of State Economic Areas devised by our friend and colleague D. J. Bogue. A by-product of this work was the little book, Statistical Geography, the best part of which is the title. We had actually scooped the geographers, who were just beginning to move on the statistical and mathematical front. But their subsequent accomplishment has been much greater than anything we could have imagined at that time.

A second project was supported by Resources for the Future, at Perloff's instance, and this resulted in a work about which I once wrote to my Dean: "I feel about this as Berlioz is said to have felt about his Requiem; if all my other work were lost, I would want this to be the one piece that was saved." (I don't know if I would write the same today; that was about 1960 or 1961.) Here we put together what we had learned from geographers, economists, and human ecologists about the way in which cities relate to the hinterland. This led to some rather ingenious statistical analysis and to a quantitative depiction of the "system of cities" as a "metropolitan hierarchy." The terms used in quotes were in wide circulation at that time. But no one before us had been able to exhibit a systematic quantitative scheme which made their meaning and implications clear. This book was well received. I remember with special affection a review by the plant ecologist, Edgar Anderson. (I knew a little about his work and thought of him as an eminent scientist.) He made various complimentary remarks but then complained that the book was "written in a dull, polysyllabic, professional mumble." He was right about that, but I took no offense, because he found Metropolis and Region meritorious on scientific grounds. Perhaps he was taken with it because we used a somewhat unorthodox method of graphic presentation for some of our results that had a certain kinship with graphic methods used by Anderson himself.

The years 1951-60, therefore, were primarily devoted to urban studies and were in some measure an outgrowth of my doctoral dissertation as well as my exposure to human ecology in graduate school at Chicago. I simultaneously kept up a strong interest in demography, especially from the teaching side. It was a privilege to work with the eminent economic demographer, Joseph J. Spengler, on two books of readings for students of population, and with the last of my three mentors (Sewell, Ogburn, and Hauser) on a symposium volume, put together at the request of the National Science Foundation. We were charged to assess the status of demography as a science, for, at this time, NSF was in the very early stage of moving into the social sciences and wanted to identify those parts of that sprawling domain that were strategic in terms of their mission to support basic science. Whether we helped on that is something I do not know. But "Hauser and Duncan" has remained the authoritative statement of the nature and scope of the discipline for these past 15 years.

Toward the end of my stay at Chicago, my efforts shifted back to the topic of social stratification. This came about because of initiatives others took that affected the course of my work. Indeed, upon reflection, it seems to me that my entire scientific career has consisted of taking up problems that someone else set for me. The masters thesis was based on data my father suggested I work on. The doctoral dissertation was on a problem defined by Ogburn. The Pennsylvania stratification study fell into my lap uninvited. The urban research at Chicago was initiated by Hauser. What happened next was that some statisticians at the National Office of Vital Statistics asked me to work on an occupational classification that could be used for a socioeconomic indicator in mortality studies, since occupation of the decedent, but no other piece of socioeconomic information, appeared on the death certificate. Somewhat reluctantly I took up this task. But I soon found that my work converged with that of my old friend (and collaborator on a census monograph about urban and rural communities), Al Reiss, who was reworking North and Hatt's study of occupational prestige. I found that I could predict the North-Hatt prestige scores quite closely with census data on the income and education levels of men in the several occupations. This allowed me to estimate prestige scores for the many occupations for which direct evidence on prestige was not available. The "Duncan SEI" (socio-economic index for all occupations) has been used quite widely in subsequent sociological research, although not by the vital statisticians who originally instigated my work on it. They were disappointed when I showed that the occupational index is only modestly related to <u>individual</u> measures of socioeconomic status like education and income. I had told them in advance that this was so, but had been unable to make the point clear enough to be compelling. It is one of a thousand illustrations I could quote to the effect that most social scientists, even those who work primarily with social statistics, do not have a good "quantitative sense." They don't really understand the meaning of the numbers they work with every day. In this instance, they just could not assimilate the quite elementary distinction between variances and covariances within occupations and variances and covariances between occupations. The "Duncan SEI" works with the latter, and quite successfully, but in the very nature of the case it cannot cope with the former.

The second external stimulus to renewed work on stratification was Peter Blau's invitation to join him in a study of social mobility. At Hauser's suggestion, we approached the Bureau of the Census with the proposal that they collect the data for us, making use of their Current Population Survey. In March 1962 they carried out "OCG" ("Occupational Changes in a Generation") as a supplement to that month's survey. In the meanwhile, my tremendously gifted student, R. W. Hodge, and I had been working with another set of data, exploring the possibility of a new means of analyzing occupational mobility, using the Duncan SEI as a quantitative variate in regression models. Virtually all analysis hitherto had looked at occupational mobility in terms of contingency tables, usually with occupations grouped into quite broad categories. We gained both precision in the measurement of occupational status and power in the ability to summarize the salient relationship, which we identified as the extent to which the occupation of a son depends on that of his father.

I now quote the bulk of a letter I wrote to Professor Hanan Selvin on 26 April 1971. He had asked me to recount how I became acquainted with path analysis.

You ask for some autobiographical notes. There is a path diagram on p. 282 of Snedecor's 1938 statistics text, which I used as an undergraduate in 1939. It always looked like a weird and wonderful thing to me, since I did not understand it. But it had a kind of esthetic appeal, as Lorenz curves later came to have for me. In his course in "Partial Correlation," which I took upon first going to Chicago in 1946, Ogburn made very passing reference to Wright. It is just barely possible I saw the reference to Wright in Dodd's Dimensions of Society, but if so I had forgotten about it until Karl Schuessler reminded me of it some months ago. I certainly did not first learn about Wright from Tukey.

So for a long time I had this thing about path coefficients in mind. I no longer remember when I first tried actually to read a Wright paper. It might have been as early as about 1952, when I acquired some reprints that Ogburn was disposing of, among them Wright's 1931 "Statistical Methods in Biology." But I certainly wasn't serious that early. In 1960 or 1961 I did a paper (never published), which decomposed a zero-order regression coefficient into a direct and indirect effect, using one version of the normal equations for partial regression coefficients. I must have verified the formula in some statistics book at the time, but I don't think I referred to Wright then. Hodge and I published a primitive version of a path diagram in AJS, May 1963, but as of the time I left Chicago in 1962 I still had looked only superficially at Wright's work. I remember giving Hodge the advice that he should study it carefully -- advice, as it turned out, that I was to follow on my own.

It was reading Blalock's 1964 book that really stimulated me to do the hard work necessary to understand some part of what Wright had done. It occurred to me that the specification of the Simon-Blalock approach was the same as the one Wright used, and I suggested as much to Blalock in 1964. In any case, by 1964 I was doing preliminary versions of the Blau-Duncan stratification model and had begun (without knowing of Boudon or his work) thinking about an expository paper, which came to be published in 1966 after going through several revisions, the last of which incorporated suggestions which Professor Wright kindly offered after I asked him to read the paper. I am proud that the most recent edition of Snedecor (with Cochran) now contains a reference to this paper.

By the time my paper appeared, a lot of other people were onto the scent, and I am not sure exactly how you'd explain that. If you want a real puzzle, you can try to figure out why the economists ignored Wright's solution in 1934 to the identification problem that the Cowles Commission people were to struggle with in the 1940's, even though Henry Schultz was on the same campus. Professor Wright told me that he communicated his work to Schultz, having borrowed Schultz's data, but did not arouse any response. It is also something of a mystery to me why Ogburn did not see that Wright's work was the way to do what Ogburn was trying to do with partial correlation. I'm sure they were acquainted, and we know that Ogburn knew of path coefficients.

It comes as no surprise to me to learn that it was "literally impossible to deduce the causal structure of the data" in your simulation. Anyone who has studied Wright seriously will know it is <u>always</u> impossible to "deduce the causal structure from the data." Of course, with a contrived example, someone might by luck hit upon the simulation routine used. But with data from nature, there will always be more than one causal structure that could have produced the data and the data will not allow one to rule out all the erroneous alternatives. Thus, while I can appreciate the didactic value of analyzing simulated data, I consider that it has nothing to contribute to the problem of how to construct better sociological models and estimate their parameters. Read the quotation from Wright on p. 15 of my 1966 paper.

Acquiring the ability to work with path diagrams after the fashion of Sewall Wright made all the difference in the way Blau and I accomplished the analysis of our OCG data. We produced a "basic model," which, primitive as it was by the standards of, say, econometrics, summed up a great deal of what sociologists had been trying to get hold of in analyses of social mobility. For example, there had been several ineffectual treatments of how education figures in the process of status transmission between generations. In our model, it came through in a very elegant way, and put a quite different light on the issue than most sociologists had anticipated. I hesitate to call the Blau-Duncan model a "discovery." But it has been called, by some sociologists who have a superficial acquaintance with Kuhn's thesis about the pattern of scientific development, a "paradigm." If that only means that a lot of people proceed to use the model and to modify it for their own purposes, then I suppose it is indeed a "paradigm." I long ago lost count of the number of books and papers that take the Blau-Duncan model as a starting point.

I contributed quite a few of them myself, during the six years or so at Michigan while completing the work with Blau and extending it with various other collaborators. Among the more significant results of this further work, which involved rather substantial elaborations of the original model, were the clarification of the roles of intelligence and of race in the process of occupational achievement. I do not consider that my work on these two vexed topics has yet been superseded, although there is a very great need for improved data and better structured models.

One of the unanticipated rewards of venturing into the area of formal models was the initiation of correspondence, and later friendship and collaboration, with a very fine econometrician, Arthur Goldberger. He noted the similarity between our models, developed with the aid of path diagrams, and models used in econometric research; and he raised questions about some of the strange things we did with these models. After a great deal of discussion, we established that there is no real difference in principle between Wright's approach and that of the econometricians. But the latter have the advantage of using the more sophisticated tools of mathematical statistics. (Such tools were nonexistent when Wright did his seminal work; I was merely ignorant of them.) Moreover, I convinced Goldberger that, in our blundering way, we sociologists were really posing problems that fell in the interstices between the well developed procedures of econometrics and psychometrics. Goldberger has repaid my debt to psychometrics in a fine paper in which the communalities of that discipline and his are pointed out. Even more gratifying is the fact that he repaid my debt to Sewall Wright by writing a paper in which he points out that Wright single-handedly solved the "identification problem" that was only attacked by econometricians some years later, despite the fact that Wright himself used an econometric problem as the vehicle for his presentation of the matter.

It is a matter of great pride that my notification of election to membership in NAS was in the form of a telegram from Sewall Wright.

From boyhood, when I avidly read Paul de Kruif's biographical essays on Microbe Hunters, Men against Death, Hunger Fighters, and the like I have had a tendency to idolize fine scientists as "great men," though I hope not in an uncritical way. It is just that there are orders of achievement that I feel I can never hope to comprehend, but at best to appreciate. (I also have this feeling about composers of great music.) I have not been privileged to have significant contact with many "great men," although I regard Ogburn as something of an approximation thereto. (He would not have appreciated that comment, because he wrote a paper of which he was quite proud demolishing the "great man theory of history.") But I do consider Sewall Wright a "great man," and consider myself very fortunate to have had a genuine engagement with some small part of his work.

I more or less wound up my empirical work on stratification in 1968, but continued to take an interest in "structural equation models" (which seems to be the best comprehensive term for causal models of the type Wright worked with, the confirmatory factor analysis models of the psychometrician, and the simultaneous equation models of the econometricians). These models have been taken up quite enthusiastically by sociologists in the last five or six years. For some of them, my 1966 paper, "Path Analysis: Sociological Examples," is the point of departure, but I am quite sure that it would have happened, one way or another, without that paper. What we realize now is that a great many sociological issues that used to be approached in an undisciplined and intuitive way can be formalized for purposes of parameter estimation and hypothesis testing. This allows us economically and quickly to dispose of lots of false leads and bad guesses that formerly would have polluted the literature for years on end. We have really achieved a major advance in regard to the amount of detail and level of sophistication that we can handle rigorously. Like all significant developments in the discipline, this one has many of the aspects of a fad. Thus, we have to anticipate that a good deal of superficial and shoddy work will be defended on the ground that its results are expressed in the form of a structural equation model. But it is at least a merit of the approach via such models that questionable assumptions and procedures are fairly transparent to the disinterested critic.

My current work is classifiable as an effort to contribute something in the area of social indicators, or measurements of social change. This brings me full circle back to Ogburn and will hopefully allow me to discharge the last of my debts to him (as I consider the stratification work to have discharged my debt to Sewell and the urban and demographic work my debt to Hauser). A Chicago classmate of mine, Dr. Eleanor Bernert Sheldon, started about 1966 a program at Russell Sage Foundation to direct the energies of some social scientists toward the monitoring of social trends, with much the same end in view as Ogburn had in directing the monumental Recent Social Trends (1933) effort carried out by the committee appointed by President Hoover. Like any other "idea whose time has come," this one has popped up from a number of directions, and a great many people have expressed interest in the production and interpretation of social indicators. It, too, bears the characteristic of a social movement, if not a fad. But there are some solid achievements on the record, as well as a lot of superficial discussion, in the work of the last few years, and it has been quite stimulating to be involved in some of the enterprises. Here, more than anywhere else among the topics on which I have worked, one might hope to discharge some of the debt to a society that treats people like me so well.

I see that I have completed my narrative (for which the accompanying formal curriculum vitae and bibliography provide a chronological framework) without commenting on some of the personal facts that are requested. I append a few miscellaneous notes to make good that deficiency.

Of avocations, I put foremost music. From junior high school through college, my greatest pleasure in life was playing in orchestra, band, and ensembles. No doubt a sociological interpretation would be that in these aggregations I found the response and approbation from significant others that everyone craves. Toward the end of my high school days my closest boyhood friend and I took an interest in composing. (He, like several of my friends of this period, has since become a professional music educator.) In the next few years, I "composed" a number of little pieces. They lay dormant all these years until quite recently, when, having acquired a tape recorder, I recorded them for my own amusement. Since 1969 I have also been doing a good deal of musique concrete with the tape recorder; this is invaluable as a way to relax of a weekend. The other side of the passion for music is a substantial record library and a good hi-fi set. My taste has evolved over the years. As a youth I enjoyed the great romantics (Brahms, Tchaikovsky, etc.) but began early to inquire into 20th century music, developing a taste for Prokofiev, for example, but also for most of the major composers of the first half of the century. I have gamely tried to get into the post-Webern stuff, but somehow it fails to grab me. What has happened now is that I recently got attracted by the modern approach to Bach via fidelity to his instrumentation and performance conventions. From there I have started to work through the whole Baroque, the Renaissance, and early polyphonic music. There is much more still to experience in this vein.

In college I used to read a good deal of fiction and was attracted by such writers as Anatole France, Aldous Huxley, and some of the great Russians. I seldom read fiction any more, have never learned to read poetry (though like every young man I wrote some awful poetry), and have little time for belles lettres. I do have a passion for H. L. Mencken, which I no doubt acquired from my father, but by and large I leave aside essays and criticism as well as fiction. I feel strongly the inadequacy of my education in the sciences and some of the social sciences and humanities. From time to time I give myself a self-study course on some subject which I missed out on. But mostly I read what I have to read in order to "keep up with the literature" and respond to requests for criticism of work by others.

I used to be a great fan of the movies, but attendance has dropped off as the years go by. I play ping-pong occasionally, but engage in no other sports. I was never attracted to athletics. However, for most of the last 20 years I have walked to work, typically a mile or more each way. At the moment I do not own a car, nor did I from 1953 to 1962 while living in Chicago.

I have many good friends, some from the years as a graduate student at Chicago, others among the extraordinary number of good students who have come my way, and still others among colleagues and former colleagues at some six or seven different universities. I have been a co-author or co-editor with over 30 different individuals, as senior, junior, or peer. All of these collaborations have been cordial; none has ever degenerated into a quarrel over who gets credit for what piece of work. For whatever success I have enjoyed in the intellectual or scientific quest, I have to credit an uninterrupted run of good fortune in respect to the persons who guided my intellectual development: parents, teachers, classmates, co-workers (above all, Beverly Duncan), colleagues, and students. Up to age 52 my adult life has been spent largely in the ivory tower, and I like it here. That's where you run into the finest people.

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