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A Career of Conceptualizing and Quantifying in Social Science

Karl A. Fox

This is the 30th anniversary of the establishment of the Economic Research Service, the 40th of my first article in *Agricultural Economics Research* (the earlier name of this journal), and the 50th of my enrollment in the graduate seminar which led me into agricultural economics

Gene Wunderlich, the editor of this Journal, suggested that I address the role of agricultural economists in social science and the profession of knowledge building generally. I have been living this role with as much intensity as I could muster for almost 50 years. I will follow Gene's suggestion in terms of my own experience and leave most generalizations for my concluding remarks

During 1942-54, I was a full-time member of the agricultural economics community. In it I found warm friendships, superb role models, and colleagues keenly interested in processes and institutions, open to useful ideas from several disciplines. I am not sure that any agricultural economist is "typical." So, my path through the profession should not be expected to represent the profession as a whole. Nevertheless, a "case study" does put real flesh on the abstract body of ideas called economics and social science. It also shows that the needs and opportunities for particular contributions to knowledge depend on the data systems, methods, and bodies of theory available at a given time and on current and anticipated conditions in the economy and society. The contributions of an individual depend on knowledge acquired before entering the profession and knowledge gained during its practice.

I acquired a superficial knowledge of the social sciences as a teenager, mainly because my father was a charter subscriber to the old *Encyclopaedia of the Social Sciences* (1930-35). I would leaf through each volume when it reached our home, the 15 volumes appeared at the rate of one every 4 months. I assumed then, as today, that all social sciences were equally relevant to our understanding of people in society.

Schooled in the traditions of several social sciences at the University of Utah, I completed an M.A. in sociology in 1938, then moved into general economics and completed my Ph.D. coursework and prelims at the

University of California-Berkeley in that field. The most challenging job available when I was ready to take one was as an agricultural economist in the War Food Administration in San Francisco in 1942.

My career developed in the reverse sequence: 12 years in agricultural economics, 18 years largely in general economics, and 19 years working across the social sciences (including economics and agricultural economics). In all three phases, I emphasized visible, tangible, and measurable basic units and the integration of theory, methods, and data, traits I associate with agricultural economists.

A few special influences accompanied me to my first job. At Berkeley, in 1941, I had studied quantitative agricultural price analysis with George Kuznets and Harry Wellman and advanced economic statistics with A. H. Mowbray. The principal text in both courses was Mordecai Ezekiel's *Methods of Correlation Analysis* (1930), which was the pre-eminent book on applied regression analysis.¹

Mowbray's course led me to Sewall Wright's 1921 article on "Correlation and Causation," which used arrow diagrams to state hypotheses concerning the causal relations connecting the various members of a system of variables. I followed Wright's example in my diagrams of the demand-supply-price structures for farm products, which figured prominently in my later work on demand analysis.

In Robert A. Gordon's seminar on business cycle analysis, I had given reports on Jan Tinbergen's *Statistical Testing of Business Cycle Theories* (1939) and Henry Schultz's *The Theory and Measurement of Demand* (1938), works that established the frontiers of macroeconometrics and microeconometrics for the next 15 years.

These courses and works placed heavy emphasis on applications. Schultz, Tinbergen, and Wright were reporting their own original research. Ezekiel was one of the most productive research workers in agricultural economics in the 1920's, and his book reflected this. In effect, all four were teaching by example from the initial conceptualization through the interpretation of results.

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Ideas and opinions expressed in this essay are those of the author and do not imply endorsement by the U.S. Department of Agriculture.

¹Sources are cited in the Bibliography section at the end of this article.

A final influence was a good background in mathematics, according to the standards of the time, including advanced calculus and advanced differential equations as an undergraduate and mathematical theory of probability as a graduate student. With a little effort I could figure out whatever demonstrations in economic or statistical theory seemed relevant to my applied work.

That first job in San Francisco was an exercise in applied common sense. Asked for analyses relevant to support and ceiling prices for many fruit and vegetable crops, my supervisor, Cruz Venstrom, and I launched a fast-moving but comprehensive project that studied the apparent responses of acreages to prices for each crop in each county, examined research reports on farm enterprise costs and yields, checked with California State statisticians to be sure we were interpreting their county average price series correctly, and surveyed experienced growers who were temporarily working for the regional office.

In each case, we selected a set of variables which we thought had accounted for most of the changes in acreage planted to a particular crop in a particular county. If two crops competed for land in a given county, their acreages were jointly determined. Our methods were informal, but we gradually built up a system of prices for many crops that we thought would have maintained their acreages at nearly constant levels during 1935-39. They provided the starting points for approximate adjustments to the cost levels projected for 1943, enabling us to handle requests for advice on different crops in a consistent manner.

In August 1944, I transferred to the regional office of USDA's Bureau of Agricultural Economics (BAE) in Berkeley to work 6 months with Marion Clawson, one of the best rough-and-ready economic analysts I ever met and a forthright and vigorous leader. Clawson had directed studies of the probable economic impacts of the Columbia Basin hydroelectric power and irrigation project in the Pacific Northwest and was coordinating similar studies of the Central Valley Project in California.

My first task was to draft a report on the prospective impacts of the Central Valley Project on industrial locations in California. Fortunately, the National Resources Planning Board had just published an outstanding report, *Industrial Location and National Resources* (1942), a good introduction to a field later known as regional economics. Mostly, I applied P. Sargent Florence's concepts of resource-oriented, consumer-oriented, and footloose industries to California data. Concepts of the economic base (including agriculture, which would expand with increased supplies of irrigation water) and an employment multiplier were at least implicit in Florence's approach and my application. Abundant supplies of electric power at

projected low prices were also considered as a factor in attracting new industries.

Next stop was the Washington headquarters of BAE and work with its Chief, the brilliant and incisive O. V. Wells. Not long ago I heard Harold Breimyer refer to him as "the man with the trip-hammer mind." Breimyer had known him since the 1930's and regarded him as the principal (though anonymous) author of the Soil Conservation and Domestic Allotment Act, which salvaged and redirected the farm price support program after the original Agricultural Adjustment Act had been overturned by the Supreme Court.

During 1945-50, Wells used me on a series of urgent special assignments, each of which required me to collate and interpret forecasts of demands and supplies for farm products comprising most of the agricultural sector. The specific forecasts were usually made by commodity specialists in BAE's Division of Statistical and Historical Research (S and HR), but as time went on I examined most of the formal and informal analyses on which their forecasts were based. The analyses were of varying quality, but there were no resources for improving the weaker ones.

The special assignments included 3 months (April-June 1945) as economist for the House Special Committee to Investigate Food Shortages, which held hearings and issued reports on the demand, supply, price control, and rationing situations for sugar, food fats and oils, and meat, poultry, and fish. Black markets were widespread and marketing channels distorted. It was a rare opportunity to observe demand and supply systems under pathological stress.

When the Marshall Plan for European economic recovery and development was proposed in June 1947, I assembled USDA estimates of the U. S. capacity to export specified agricultural products in support of the plan. As secretary of the Food Resources Subcommittee of the President's Committee on Foreign Aid, I worked with other secretaries to appraise the capacities of all relevant sectors of the U. S. economy and to prepare materials for the committee's consideration.

Also during 1947-48, I coordinated and edited a USDA report to the House Agriculture Committee on long-range prospects for American agriculture. During 1949-50, I helped coordinate an internal USDA study of the probable costs and consequences of alternative price support programs over a period of years under three different economic scenarios; the study was left incomplete when USDA's concerns shifted to shortages and inflation rather than surpluses and price supports in June 1950 with the outbreak of hostilities in Korea.

In the fall of 1950, I took stock of my professional situation. For several years I had been busy and happy with the special assignments which brought me approval and promotions from Wells and prestige among my colleagues in BAE. But I had published only one journal article and I had not submitted a dissertation to Berkeley.

I thought I could make an important contribution to demand analysis for farm and food products. I had internalized an enormous amount of information about the food and agricultural sector, read a large proportion of the published literature on demand analysis, and acquired up-to-date training in economic theory and econometrics.

The last coordinated set of demand studies by BAE economists had been published in the 1920's and early 1930's. Since then, BAE's data systems on food consumption and farm-to-retail price spreads had been greatly improved, as had the Commerce Department's data on disposable personal income. Some problems associated with multiple regression analysis of time series (multicollinearity, autocorrelation, the interpretation of significance tests) had been substantially clarified. And, there were now 20 years of data (1922-41) undisturbed by the special circumstances of World Wars I and II and their immediate aftermaths.

A consistent set of demand analyses for many farm and food products would be of value to S and HR commodity specialists in their economic outlook work and in their responses to requests for sectorwide projections and price-support program analyses, which were bound to recur. It might also be of interest to the profession as a whole.

During November-March 1950-51, I worked directly with S and HR's excellent commodity clerks and outstanding Computing Pool. The first result was an article, "Factors Affecting Farm Income, Farm Prices and Food Consumption" (1951a), which included about 70 statistically estimated equations. (Other Karl Fox publications are listed in "A Career in Print" in this essay.)

The second result was a dissertation submitted to Berkeley in 1952, "The Demand for Farm Products," a condensed version of which was published by USDA as *The Analysis of Demand for Farm Products* (1953a). Both included arrow diagrams of the demand and price structure for major livestock products and for five categories of crops. The identification problem posed by Haavelmo in 1943-44 was carefully considered. I demonstrated that the demand functions for many foods and farm products could be estimated without bias by ordinary least squares. I also showed that the demand functions for other commodities had to be estimated by simultaneous equation methods, for which I had neither time nor computing resources.

These demonstrations formed the basis for my article on "Structural Analysis and the Measurement of Demand for Farm Products" (1954b).

From 1951 on, I carried in my mind a sort of econometric map of the agricultural economy, based on my arrow diagrams, empirical estimates of many of their components, and estimates of other analysts. I could easily visualize elaborations of this national map to include demand and supply curves for specific commodities in each member of a set of regions. The existence of interregional trade in a given commodity would imply that prices in shipping and receiving regions would differ because of transportation costs. Quantitative models of interregional trade might permit S and HR specialists to regionalize their outlook statements for certain commodities.

I explored this idea in "A Spatial Equilibrium Model of the Livestock-Feed Economy in the United States" (1953b). The model involved a demand function for feed and a predetermined supply of feed in each of 10 regions, plus a matrix of transport costs between regions. It proved easy to solve and explain without using the formal methods and jargon of quadratic programming, and the model revealed important insights into a system accounting for more than half of all cash receipts from marketings of farm products. "The Use of Economic Models in Appraising Foreign Trade Policies" (1954a) adapted the 10-region model to explore the effects of general and discriminatory tariffs, subsidies, and changes in transport costs on prices, consumption, and trade in a hypothetical 10-country world. In this context, the spatial equilibrium approach also clarified some important issues.

In 1951, I decided to put on record in "The Measurement of Price Support Costs" (1951b), the logical and mathematical framework of the 1949-50 study of price support programs previously mentioned. A May 1954 conference on Policies to Combat Depression led to my semi-empirical paper on "The Contribution of Farm Price Support Programs to General Economic Stability" (1956a), which followed the outlines of the 1949-50 study and also provided for agricultural feedback to the nonfarm economy. In effect, I combined a detailed model of the agricultural sector with a highly aggregated model of the nonfarm economy.

In 1955, Klein and Goldberger published a 20-equation model of the United States. In reviewing it in "Econometric Models of the United States" (1956b), I pointed out that the model's chief limitation was its high level of aggregation, much too high to accommodate significant information about particular sectors of the economy, such as agriculture, and to tap the knowledge and judgments of sector specialists. I observed that "our knowledge of the agricultural sector is adequate to support an econometric model of considerable complexity," including demand and sup-

A Career in Print

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ply functions for 20 to 25 major commodities or groups, and that such a model would be highly useful for policy purposes. A truly large-scale dynamic model of the U S economy would permit the advance appraisal of any set of economic policies and programs in relation to any initial positions and trends of the various sectors of the economy.

I never left the BAE, the BAE left me. Pieces of the former BAE were absorbed into new agencies in 1953-54. My position, absorbed into the Agricultural Marketing Service, involved a promotion and more administration than I cared for, so I joined the staff of the Council of Economic Advisers in 1954 and then moved to Iowa State University in 1955.

At Iowa State, research on commercial agriculture had been going well without my help. However, I found

that a good many people in Iowa were concerned about the decline of small towns and the weakening of rural institutions. Some agricultural economists recognized that the seemingly inexorable process of farm enlargement was reducing the farm population and hurting small towns, but they were not paid to think about such things. State extension economists and sociologists handled individual requests originating in one or another of Iowa's 99 counties. The larger picture was ignored. Who would be foolish enough to produce an answer for which there was no question?

During 1955-60, pressure from concerned Iowans on university administrators brought more focused attention to problems of "agricultural adjustment." In 1961, the Kennedy administration responded to similar pressure at the national level and provided funds to the land-grant universities for programs of rural develop-

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ment which could involve areas larger than counties Extension Service administrators at Iowa State asked me to suggest criteria for delineating such areas

Studies by rural sociologists before World War I indicated that the residents of a small town and its trade area formed a relatively self-contained community The boundary of the trade area was about five road-miles from the town or an hour's travel time with horse-drawn vehicles The automobile had changed the face of rural America When workers in Iowa began daily commutes in 1961, most of them headed for central business and industrial districts of Iowa's larger cities

My research showed that a relatively self-contained community could be delineated around 11 or 12 Iowa cities, each of which was a wholesale trade center with

a population of 25,000 or more Each such city had a large number of jobs in a wide range of occupations and was the center of a labor market area, a home-to-work commuting area with a radius of about 50 road-miles and extending over several counties I later called such areas FEA's, functional economic areas

The trade and attendance areas for retail stores, schools, churches, and other voluntary associations were usually much smaller than an FEA Most of the wages and salaries paid out by employers in an FEA would be spent at establishments within its boundaries, making it relatively easy to create FEA income and product accounts compatible with national economic accounts The resident population of an FEA would also spend nearly all of its time within its boundaries during a given year

The FEA delineations were used for several purposes in Iowa and the concept was rapidly disseminated across the United States. The Census Bureau commissioned a study by Brian J. L. Berry at the University of Chicago which delineated 358 FEA's, containing 96 percent of the U.S. population.

The FEA concept was endorsed by a committee of the Social Science Research Council on (Geographic) Areas for Social and Economic Statistics in 1967. A national system of FEA's comprised of clusters of contiguous whole counties could accommodate the county data bases of several agencies: the Standard Metropolitan Statistical Areas (SMSA's), which contained the central cities of the more populous FEA's, the additional counties within commuting distance of each SMSA, and the many nonmetropolitan FEA's centered on cities of less than 50,000 people. The Commerce Department's Bureau of Economic Analysis used the FEA's as a starting point for its system of BEA Economic Areas, which was used for regional economic projections by several U.S. agencies and by the National Planning Association.

During my tenure as a department head at Iowa State, I was fortunate to recruit some outstanding young economists. Two of them, Erik Thorbecke and Jati Sengupta, shared my admiration for Jan Tinbergen and were particularly enthusiastic about his 1952 book, *On the Theory of Economic Policy*. According to Tinbergen, a policymaker should classify the variables in a national econometric model as (1) targets, (2) instruments, (3) noncontrollable, and (4) irrelevant in relation to the array of policies under consideration at a particular time.

Desired values of the target variables in the coming year (for example, employment, income, and balance of payments) might be chosen intuitively, but in principle they (and some or all of the instruments) could be included in an objective function reflecting the policymaker's value system. Tinbergen emphasized the implications of a specified set of target values however chosen. Henri Theil, in his *Economic Forecasts and Policy* (1958), worked out the formal implications of an objective function. Theil's version of the "steering problem" had strong affinities with stochastic control theory.

Sengupta was well versed in quantitative economics and operations research, including stochastic control theory. Thorbecke was specializing in economic development. I joined them in coauthoring a book, *The Theory of Quantitative Economic Policy* (1966), which made major extensions and applications of the Tinbergen and Theil approaches to economic growth and stabilization models and regional and sectoral analyses.

I extended my earlier conceptualizations in "The Study of Interactions Between Agriculture and the

Nonfarm Economy—Local, Regional and National" (1962) and "Spatial Price Equilibrium and Process Analysis in the Food and Agricultural Sector" (1963). My most detailed conceptualization of the food and agricultural sectors of the United States and other large countries or multicountry regions was realized in 1969 in "Toward a Policy Model of World Economic Development with Special Attention to the Agricultural Sector." Regions classified according to soil and climate intersect with functional economic areas. Spatial equilibrium models are suggested both within and among 20 large world regions. In addition, concepts from several social sciences are introduced as relevant to the description and analysis of systems of villages, towns, and cities.

My work on FEA's had put me in touch with some outstanding urban and regional economists and quantitative geographers, and my membership on the Board of Directors of the Social Science Research Council (1963-67) involved semi-annual meetings with leading social scientists from several disciplines. These contacts stimulated me to read some of the best books published by social scientists in the 1950's and 1960's—the best disciplinary research, in Glenn Johnson's terminology.

The social indicators movement was launched in 1966 with impressive essays by outstanding scholars. Their immediate objective was to create a Council of Social Advisers (parallel to the Council of Economic Advisers), which would spearhead rapid development of data systems adequate for the guidance and evaluation of the many social programs initiated by the Johnson administration. A yearly *Social Report of the President* would appraise social conditions and recommend policy adjustments; a statistical appendix would present the relevant data.

This objective was not realized, but a demand for social indicators had been created and some highly eclectic work was published by people with little or no training in the social sciences. There was an enormous gap between social indicators and social theory, and I thought I could make an important contribution toward closing, or at least narrowing, it.

During 1972-73, a National Science Foundation (NSF) grant enabled me to spend 14 months on full-time research. By March 1973, I was ready to describe a framework which I thought would accommodate observable units and measurements from several social sciences, including economics. The framework would accommodate commercial agriculture and rural communities along with all other sectors and elements of an economy and society. The result, *Social Indicators and Social Theory: Elements of an Operational System* (1974), which advocated the supplementation, and eventual partial replacement, of social indicators by social accounts.

Elements of an operational system were designated at three levels (1) individuals, families, and organizations in a small community, (2) cities and regions, and (3) national and world models and data. Subjectively, I felt I had written the book in the tradition of agricultural economics and dedicated it to Mordecai Ezekiel and Frederick V. Waugh, pioneers in combining measurement with theory.

While writing *Social Indicators and Social Theory*, I decided that "behavior settings," as defined by the psychologist Roger Barker of the University of Kansas, were promising basic units for a system of social accounts which would accommodate variables of interest to all of the social sciences. To implement such a system, we would need a criterion of comprehensiveness (what range of human activities should be included?), an objective method of classification that applied equally to market and nonmarket activities, and an objective unit for sampling and recording the contributions (inputs) people made to the social system and the rewards (outputs) they received from it.

I thought we could meet these requirements by viewing human societies from the perspective of "eco-behavioral science," a term introduced by Barker in 1969 after 22 years of pathbreaking research within a somewhat narrower framework which he called "ecological psychology"—the study of individual behavior in the settings of everyday life.

While observing children in a small Kansas town, Barker noted that their behaviors changed abruptly when they moved from one setting to another, these settings were units of the children's environment. Third-grade academic subjects called for one pattern of behavior, hallways for another, and lunchrooms for a third. Adults changed their behaviors abruptly as they moved from offices onto the town's streets and sidewalks and into restaurants, barbershops, or grocery stores. He concluded that the town, as an environment for human behavior, was *de facto* partitioned into hundreds of distinct observable units which he called behavior settings.

In various contexts, Barker and his colleagues asserted that "a school is its behavior settings" or "a community is its behavior settings." One colleague, Paul Gump, stated "People live out their lives in a series of environmental units (behavior settings), experience in these settings is life. If experience is good, life expands, if it is bad, life diminishes." The study of behavior settings, and of organizations and communities viewed as systems of behavior settings, is called eco-behavioral science.

Barker and his associates made several comprehensive surveys of the behavior settings of a Kansas town. My 1990 book, *The Eco-Behavioral Approach to Surveys and Social Accounts for Rural Communities*, pro-

vides a detailed introduction to the method of behavior setting surveys and the microdata of Barker's last and most advanced survey. The book sets forth proposed applications and needs for multidisciplinary cooperation, highlighting the advantages of rural social scientists, including agricultural economists, in eco-behavioral research.

My 1985 book, *Social System Accounts: Linking Social and Economic Indicators through Tangible Behavior Settings*, and my 1989 article, "Behavior Settings and Social Systems Accounting," summarize my ideas for relating behavior settings to official data systems on establishments, industries, occupations, employment, and earnings, to the Standard Industrial and Standard Occupational Classifications, to Commerce Department data on stocks of various types of physical capital and consumer durable goods, to studies of time use, and to the objective social indicators published since 1982 by the Organization for Economic Cooperation and Development (OECD).

Agricultural economists are engaged in a remarkably wide range of activities. Many of us have cooperated with applied scientists and engineers in problem solving and subject matter research. Neither we nor they have felt obligated to uphold disciplinary purity at all costs. I have seen cooperation in the same spirit between agricultural economists and rural sociologists.

I don't know how other agricultural economists feel about our relationship to "general" economics. If we are strictly in an area of application of economic theory, then perhaps we should not make room in our graduate programs for other kinds of theory. However, in our preface to *Systems Economics* (1987), Don Miles and I asserted that "the most promising frameworks for multidisciplinary cooperation can be expressed in the language of general systems theory, broadly conceived" (p. ix). We suggest that this book of essays by nine economists might be used as a guide for new and experimental courses in "systems economics" or "systems approaches to multidisciplinary research." Students may choose additional readings from references cited in the various essays.

I have emphasized social accounts for particular cross-sections of time since 1973. However, social accounts for successive years or quarters over a period of time would provide bases for dynamic models of macrosocial systems at national and regional levels. Some important conceptual work has been done toward dynamic models of microsocial systems based on behavior settings; these are annotated in pages 355-73 of *The Eco-Behavioral Approach to Surveys and Social Accounts for Rural Communities* (1990). Notable contributions include James R. Prescott's chapter "A Behavior Setting Approach to Microanalytical Simulation Models at the Community Level," in *Social System Accounts* (1985) and a related chapter by Prescott in *Systems Economics* (1987), Allan W. Wicker's article,

"Behavior Settings Reconsidered Temporal Stages, Resources, Internal Dynamics, Context" in *Handbook of Environmental Psychology* (1987), and Jati K Sengupta's article, "Modeling Eco-Behavioral Systems," in *Mathematical Social Sciences*, Vol 11 (1986)

My path through agricultural economics, economics, and social science has been a long one. In agricultural economics, my role models were Mordecai Ezekiel, O V Wells, and Fred Waugh, in economics, Jan Tinbergen, in quantitative methods, Sewall Wright and Herman Wold, in social science, Herbert Simon and Kenneth Boulding, in social accounting and model-building, Richard Stone, in eco-behavioral science, Roger Barker

I felt a strong sense of community in agricultural economics during 1942-54, and I enjoyed warm relations among general economists during the 1960's. After 1971, my preoccupation with social system accounts and eco-behavioral science left me pretty much isolated from both communities, though not from particular colleagues who shared my new interests

If I had remained continuously involved with agricultural economists, I might have realized that their concerns had broadened tremendously. My participation in a 1988 workshop sponsored by the Social Science Agricultural Agenda Project woke me up, but like Rip Van Winkle, I was not able to contribute much to the brave new world

I have been greatly impressed by the accomplishments of my near-contemporaries—Glenn Johnson, James Bonnen, Harold Breimyer, and Vernon Ruttan, among others. The performance of some younger members of the profession marks them as worthy successors to Ezekiel, Wells, and Waugh. I believe the profession is well-equipped to generate new knowledge relevant to the enormously complicated economic, social, political, and environmental problems in which we are now immersed

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