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Two Agricultural Economists Look at Rural Sociology

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At the invitation of the editor, we shall attempt to describe our perceptions as agricultural economists of what is known to us as rural sociology. We should say right off that we are complimented that this invitation has been extended to us. We think that a comparison of our two disciplines has been useful to us, if only to clarify our thinking about our own. We emphasize that our perceptions are based on limited contact, and we have made no systematic study of your discipline. Even to attempt the critique and analysis of the kind requested of us presumes an arrogance on our part that is somewhat discomfiting. There will no doubt be a considerable disparity between our view and what you see as reality. But rest assured that we realize that we see through a glass darkly. In fact, it is quite likely that this piece will have more entertainment value for you than any serious scientific worth.

Still, both of us probably have had relatively more contact with rural sociology than have most other agricultural economists. And these contacts serve to illustrate how far apart the two disciplines may be and how difficult communication between them is. For example, the first author has been a member of several Ph.D. sociology oral exam committees, generally for students who have minors in economics. These experiences have been frustrating to him and to some extent to the examined students as well. Both questions asked by sociologists and apparently answers expected from the students appeared to lack precision and rigor. They seemed very broad and lacking in focus. In short, they seemed to lack the context of a framework that narrowed the scope of the inquiry. On the other hand, his questions were taken to be unrealistically narrow and removed from the real world in which people actually live and work. Unfortunately, the student was often caught in the middle, but usually gave answers acceptable to the questioning party if not entirely satisfactory to other members of the committee. Also, the first author has reviewed perhaps a dozen articles submitted to rural-sociology journals, probably because he knew the editors and presumably had some knowledge about the topics being discussed in the papers. He has not kept a careful tally but remembers that he recommended rejection for a big majority and, in a case or two, recommended revisions that considered issues that were important to him that had not been addressed in the paper. He has been invariably disappointed in the methodology employed. As a rule, the papers have not identified a researchable "problem," used theory to deduce testable hypotheses, nor adequately performed empirical tests. The other side of the coin has been no more successful. He has submitted three papers to rural-sociology journals, believing that they would be of interest to readers of those journals. All were rejected with the editor's admonition that they were too narrow to be of interest to journal readers. All were later published in either economics journals or reviewed agricultural-experiment-station bulletins.

The second author has had similar experiences with two manuscripts submitted to rural-sociology journals. Reviewers of the first found the paper to be "well-written and insightful, but not directed toward a sociological audience." All three of the reviewers appealed for "more attention to the work of rural sociologists and other social scientists, rather than just that of agricultural economists." The second manuscript was also rejected with the comment that the work of rural sociologists in the area had been neglected. Since the second author was not familiar with literature outside agricultural economics, a rural sociologist at another university was asked to collaborate on a revision. A few sociological works were cited in the literature review section, the rural sociologist was made a co-author, and the manuscript was accepted!

What is going on here? Naturally, separate disciplines focus on different problems and employ different theories and empirical techniques. That is why they are separate disciplines. Specific languages have evolved to satisfy the scientific needs of these disciplines. Each field has its own literature that is read primarily by people in that field. So why should the experiences we've described not be expected? In fact, we would probably have been surprised if the outcome had been much different. If economists and sociologists could easily publish in each other's journals, one of them or both would probably be expendable. Still, we believe that we have seen some differences in approach that may be important for both groups to understand. At least we won't be harboring illusions about how easy it is to crash each other's academic parties.[1]

KUHN'S PARADIGM

Kuhn's (1970) analysis of the nature of science will serve as a convenient framework and point of departure for the discussion. Kuhn's view is that a science is mature when it settles on a single paradigm that transforms the researchers, associated with various schools within a discipline, into a profession. The synthesis achieved by reaching consensus on a paradigm (an "ah-ha" experience) defines the field and creates the mechanism by which scientific progress can be made. Participant researchers (believers?) perform what Kuhn calls "mop-up" work--articulating the paradigm and extending it to other areas of experience. The paradigm gives researchers the criteria for choosing solvable problems ("puzzles") and selecting from among formerly meaningless data only those pertaining to the problem.

It is not part of the research agenda of such a mature science for members to challenge or even investigate the established paradigm, for it is sacrosanct and accepted by the club. The paradigm determines the scope of the view of the world, identifies the crucial problems, and postulates the significant explanatory variables. It may even at times set the bounds for prescribing a better condition for the world. The upshot is that the accepted paradigm creates expectations; when there is a discrepancy between what subscribers expect to observe and what they actually see, a problem emerges that cries for investigation. If it is solved, another notch has been cut in the belt of scientific progress. In contrast, an immature science has competing paradigms. It tends to make progress slowly, if at all, since much of the intellectual energy of the scientists is dissipated in debating the merits and demerits of alternative paradigms.

Kuhn goes on to explain the nature of scientific revolution--conceptual blockbusting on a grand scale, as when Copernicus switched the idea of what goes around what. Against the expectations/background provided by the established paradigm, anomalies are noticed by some mavericks in the profession or outside it that seem incapable of resolution with the established paradigm. The crowd, however, either ignores them or tries to rationalize them away. It is very much threatened by the possibility that its investment in human capital will be rendered obsolete by any new paradigm, mastery of which would require a large commitment of scarce financial and time resources. The expected loss in wealth and status that would ensue if the new replaced the old causes those who understand the old to hold on tenaciously and resist the new with great vigor. But those who have not invested so much and have most of their professional lives ahead have much less to lose, and they persist and eventually create a crisis. When the new sun-centered paradigm is simply irresistible, a scientific revolution takes place. When the dust settles, those with too much invested in the old simply become irrelevant or retire. Normal science begins afresh under the replacement paradigm.

We believe Kuhn's paradigm model to be strongly relevant to both economics and sociology in their present states. In fact, we would argue that nearly all of the past frustrations resulting from our contacts with sociology can be explained by our compulsion to put the problems encountered into our dominant paradigm. We treat problems this way almost automatically because our discipline is trying to do normal science in the Kuhnian sense. The understandable reaction of sociologists to our paradigmatic posturing has been antagonistic since they have not yet settled on a single framework; rather, different sociologists have their favorite paradigms, none of which is akin to ours. Since this hypothesis--that economics is a relatively mature science working at articulating its paradigm while sociology is a multi-paradigm discipline not yet doing normal science--is central to the purpose of this paper, we shall develop it at some length.

PARADIGMS AND SOCIOLOGY

If economics is immature--say, in its late teens--then, in our opinion, sociology is still in infancy as a science. It is at the stage of haggling about paradigms, not yet having settled on one, and thus not yet ready to do normal science. We consulted three books in sociology--THE IDEOLOGY OF SOCIAL PROBLEMS (Reasons and Perdue, 1981), SOCIOLOGY: A MULTIPLE PARADIGM SCIENCE (Ritzer, 1975), and SOCIOLOGY (Robertson, 1977). We must leave it to readers in sociology to judge how representative these books are of the field. The first two books mentioned explicitly utilize Kuhn's paradigm framework for their analyses. We gather the third is an elementary textbook for introductory sociology--just right for us. Robertson discusses sociology's paradigms without mentioning Kuhn.

Paradigms are defined by Reasons and Perdue (1981:4) as "models of society..." that "shape not only scholarly explanations and definitions, but also the general views most of us hold about social problems." A paradigm is a "mind set" specifying the issues and questions to be addressed, enabling the researcher to be selective. We have absolutely no quarrel with this definition. Reasons and Perdue describe two major paradigms for sociology--order and conflict. Robertson adds one in addition to these two--interactionism. Ritzer puts order and conflict under one of his and then uses a different thought-classification system entirely which he calls "paradigmatic."

Following Robertson, we will briefly describe three major sociological perspectives--paradigms. This will either be old-hat to our readers or heresy if our interpretation is not valid! The first, the functionalist perspective, tracing back to Spencer and Durkheim, focuses on the basic order in society and sees the specialized elements in society as analogous to organs in a living body. The second, the conflict perspective, stemming from Marx, emphasizes that the elements in society are always in tension, hostility, competition, and sometimes outright violence for scarce ends--power, wealth, prestige. The third, the interactionist perspective, influenced by Weber, does not focus on social structures but on individual-level relations and interactional behavior.

Ritzer, calling sociology a multiple paradigm science, refers to these traditional categories as theories and places them (and other theories) under three other "paradigms:" social facts, social definition, and social behavior. "Social factists" look at social structure (e.g., a group, a bureaucracy) or an institution (e.g., a family, a religion) as "real" things, focusing on their nature and interrelationships. Functionalism (order) and conflict are theories about social facts. "Social definitionists" study intra- and inter-subjectivity and the action that results. The interest is in the way individuals define their social structures. Weber's interactionist perspective is included here. The social-behavior paradigm is really psychology applied to sociological problems. The interest is in understanding, predicting, and changing the behavior of man. Ritzer (1975:26) states that the adherents of each of these paradigms "attempt to deny the validity of the other paradigms." Thus, it would seem that, if he is right, sociology fully qualifies as an immature science, since haggling over paradigms is the chief Kuhnian characteristic of an immature science.

What does all this add up to for the purpose of this paper? We are in no position to judge the validity of the competing claims for paradigmatic superiority in sociology. Whether Kuhn is even right about competing paradigms as the hallmark of an immature science, perhaps only philosophers and historians of science can fully assess. It occurs to us, however, that sociologists might argue that the scientific scope of sociology is very broad, and that various paradigms are needed and justified if social phenomena of such great diversity are to be satisfactorily explained. Could be! We would observe simply that your field is so broad that many of your "problems" are also of interest to us. But our perception is that you are guided to a different set of explanatory variables by your paradigms than we are by ours. The upshot is that when we review your work, it appears that you never formulate the questions adequately, never establish an adequate theoretical base, and seldom perform a satisfactory empirical test.

PARADIGMS AND ECONOMICS

Discussing the tremendous variety of positions on economic policy by economists is a favorite passion of the press these days. As an example, in its January 31st issue of 1983, THE U.S. NEWS AND WORLD REPORT (Anonymous, 1983:66-71) interviewed six Nobel-Prize economists on "How to Get the Country Moving Again." The results must have made the public wonder why a prize is given in the so-called science of economics.

* Milton Friedman: "The most important single action we can take now is for the Federal Reserve to lower the rate of monetary growth.... The key is for the Fed to stick to the targets it has set rather than erratically allowing an explosion in growth and then cutting way back."

* Kenneth Arrow: "Continued monetary ease to bring about a further decline in interest rates is the most important single step we can take to get a recovery going.... The Federal Reserve was correct last year when it decided to allow money-supply growth to exceed the narrowest monetary target."

* Paul Samuelson: "President Reagan and the Federal Reserve should continue allowing more growth in the money supply until we are definitely in a healthy recovery with an annual growth rate of at least 4 percent after adjustment for inflation."

* George Stigler: "One thing that could be done to help achieve stability would be to eliminate the large swings in the money supply such as we had during the first and second halves of 1982."

Lawrence Klein: "The first thing to be done to get the economy moving again, apart from letting natural forces work, is to continue the course on monetary policy that was initiated a few months ago, making credit conditions easier."

James Tobin: "We need, above all, an expansionary monetary policy--one that will bring real interest rates down to reasonable levels...."

All agreed that monetary policy was of central importance, but what does it mean when scholars recognized as the best by the Nobel Committee are so completely polarized about what ought to be done? Recommendations were almost as disparate concerning the deficit, unemployment, taxes, and whether the government should have an industrial policy. For one looking at economics from the outside, including, certainly, policy makers earnestly seeking guidance, economic science must seem a shambles.

These major macroeconomic disagreements, however, may represent a full-blown Kuhnian revolution (a mature science in crisis turning over its paradigmatic structure), haggling over paradigms as in a Kuhnian immature science, or simply a contest among theories subsumed under a more general paradigm. It appears that Friedman (1953:25) would subscribe to the third view; he noted that differences in economic policy:

> derive predominantly from different predictions about the economic consequences of taking action--differences that in principle can be eliminated by the progress of positive economics--rather than from fundamental differences in basic values, differences about which men can ultimately only fight.

We believe that Friedman is essentially correct. The question is whether positive economic science can yield the answers needed to such important policy questions within a reasonable time frame. The fact that most economists seem to be working within a given paradigm at least gives us some hope. That paradigm is economic efficiency, an allocation of scarce resources such that the value of output is a maximum. Nearly all main-stream economists work in a framework that assumes rational utility-maximizing consumers and profit-maximizing firms, all constrained in their options by limits on income and time that make economizing relevant. The market where voluntary choices enhance the welfare of both buyer and seller is the primary institutional device utilized for this economizing. This dominant paradigm goes all the way back to Adam Smith (1776). In his Nobel lecture, Stigler (1983:532) said:

> It was Smith who provided so broad and authoritative an account of the known economic doctrine that, henceforth, it was no longer permissible for any subsequent writer on economics to advance his own ideas while ignoring the state of general knowledge.

In Kuhnian terms, some of the advances in economic theory such as "the addition of the principle of variable factor proportions, or the notion of the consumer with relatively stable transitive preferences" (Gordon, 1965:124) can be seen as a "further articulation" of Smith's basic paradigm. So can Baumol's (1982) "perfectly contestable markets" and Stigler and Becker's (1977) treatment of the consumer as having a production function for utility, using not only market goods as inputs but also human time. These are examples of paradigm articulation and extension--of doing "normal" science. Thus, we see economics as growing up, but with a struggle. It obviously is an immature discipline as sciences go. It is fractionated by schools, and if you want to predict how an economist really feels in her/his heart of hearts, especially about economic policy, just find out where he/she was trained. This is hardly a convincing testimonial for scientific objectivity and maturity.

ADDITIONAL DIFFERENCES IN METHODOLOGY

In addition to the questions of appropriate paradigms that divide us are the various roles of theory and data, an area we know as methodology. In outlining his new paradigmatic classification scheme, Ritzer (1975) briefly described the methodology used by each paradigm. Under the social-facts paradigm, he mentioned that questionnaires or interviews are the preferred method but questioned whether this individual-level method can get at social facts. He suggested instead using the comparative and historical method to study social facts. The social-definition group employs a method called simply "observation." The social behaviorist turns to the experiment--either in a laboratory or in a real-world setting. But with respect to each paradigm we would ask questions? What is to be asked in a questionnaire? What is to be observed? What experiment? How can we know what to look for without clear guidance from a theory that postulates the crucial explanatory relationships?

It seems to us that the first step after the identification of a "problem" or a "puzzle" in any scientific study is the building of a theoretical model that sets forth causal relationships that are hypothesized to hold. Then, to test hypotheses, the model is given an empirical specification that clarifies what data are relevant. In our field, this usually consists of some econometric or optimization specification. Using data specifically identified and collected, parameters of the model are estimated and tested for statistical significance as hypothesized. Our limited exposure to manuscripts from rural sociologists has revealed a propensity to proceed with empirical observation without the necessary theorizing for establishing scientific causation, and this seems to be consistent with our reading of Ritzer.

AN ILLUSTRATION

To illustrate how different our approach is from the way we perceive yours, consider an important social problem in which both sociologists and economists are very interested--divorce. First let us present our notions about how we think sociologists would approach the study of the issue. Having mastered statistical techniques, sociologists would select a sample from the population of interest and collect data about it, most likely by interview. Information would be categorized and crosstabulated, appropriate statistical tests would be conducted, and conclusions (generalizations) drawn (or perhaps Ritzer's social behaviorists would probe deeper into the psychology of each couple).

Using our economic paradigm, Becker (1981) provides a useful theory of divorce.[2] He puts this individually traumatic experience into terms that can be analyzed and explained as a social phenomenon. The theory is actually quite complex and only the barest outline will be presented. Participants in marriage are assumed to be rational individuals who have limited information about the utility they can expect with potential mates because of search and courting costs. Resources are invested in acquiring information about desirable traits in a mate, but information is too costly to be complete. Many marriages fail early primarily because of imperfect information in marriage markets and the accumulation of better information during marriage. A husband and wife would both consent to a divorce if, and only if, they both expected to be better off divorced. The financial settlement is the inducement for a recalcitrant spouse to consent to divorce. Women with higher earnings gain less from marriage than other women do because higher earnings reduce the advantages of the sexual division of labor in marriage. Thus, women with higher earnings should be more prone to divorce. Becker goes on to explore several implications of his theory and then tests these implications with relevant data from the real world. The explanation is useful because it helps us to understand the societal phenomenon of divorce, perhaps to predict its direction of growth or subsidence, to evaluate its impacts on other aspects of society, and to plan at the policy level--for example, the state's compensation laws for the injured party.

CONCLUSION

In sum, the major difference between us is that economists have chosen to be more precise in a limited sphere of life, while sociologists have elected to remain inexact but broad. The economist's choice is well illustrated by reference to a well-known citation from Machlup (1967). In discussing agricultural prices, he (1967:8) argued that economists can only address the third of the following questions with any reasonable expectation of success:

> (1) What will be the prices of cotton textiles? (2) What prices will the X Corporation charge? (3) How will the prices of cotton textiles be affected by an increase in wage rates? (4) How will the X Corporation change its prices when wage rates are increased?

Only the third? What a sterile science economics has become! But we believe it is significant that we can say something quite definitive about the third question.

It is this very issue of the narrowness of scope and methods that has caused some economists to be very critical of modern economics (Ward, 1972). In this they are joined by other social scientists, and we would think most sociologists. You might enthusiastically agree with Ward when he (on the jacket of his book) says: "Yet, this sophisticated 'analytical machinery,' so effective in solving minor puzzles, has proven unable to cope with many of the most important problems of our time." Meanwhile, sociologists--without an established paradigm and without much of a quantitative tool bag--have tackled social problems such as alienation, crime, drugs, racism, sexism, the aged (to simply repeat a few of the subjects from the chapter titles from Reasons and Perdue). Sounds fascinating! Perhaps we are in the wrong field! But, no, we have too much human capital invested now to change disciplines. So we will probably go right on criticizing you for fuzzy thinking, too little theory, and bad methodology.

NOTES

[1]For purposes of the discussion, we choose to drop both "agricultural" and "rural" since our basic differences stem from those of our respective parent disciplines--economics and sociology.

[2]A short aside might be useful. Machlup's (1967) well known (to us) analogy of the theoretical automobile driver may soften the blow accompanying your reading of an economic explanation of divorce. Theory (and paradigms) only serve to explain and predict effects of mass behavior. When roads become wet and slippery and fog reduces visibility, theory allows us to predict that traffic will slow and accidents increase. Theory does not allow us to predict the behavior of any particular driver, only the "theoretical reactions of a hypothetical driver" (Machlup, 1967:6).

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