THE RESTRUCTURING OF SOUTHERN AGRICULTURE: DATA NEEDS FOR ECONOMIC AND POLICY RESEARCH: DISCUSSION

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The general goal of the Skees and Reed paper is to emphasize the need for data which permit anticipatory, problem-oriented research by the agricultural economics discipline. More specifically, they seek more funding and support for the concept of state-generated data and, very specifically, support for panel design surveys by each state.

The issue is timely and appropriate for consideration by members of the discipline as well as interested public outside the discipline (i.e., producers, university and government decision makers, and food and fiber consumers). The authors effectively dissect the issue and suggest a specific alternative for resolution of the problem.

Researchers generally distinguish themselves by inherently searching for more and "better" data. They seem to have insatiable appetites for new facts and figures. Ironically, their most recent meal usually leaves the preparers somewhat apologetic over the indigestion that results from "vital" facts and figures that always seem to be lacking.

While real-life drama marches on, a datum drawn from it exists as a point waiting for vitalization and explication by creative and enterprising researchers. If a datum is to have life in research, it must be credible, replicable, generalizable, testable, and on the net, beneficial. In fact, if the benefits from a datum are greater than the costs, all the other necessary conditions are likely being met. The first four are indeed necessary but not sufficient. The fifth and final criterion brings sufficiency to the evaluation and efficacy for a particular datum. Although beneficence is often in the eye of the beholder, Jordan emphasizes this point in terms of relevance. He suggests that researchable questions must be questions that society considers worth answering. The exposition of Freebairn et al. suggests an approach based on economic surplus which, if adapted, could lend some sophistication to the Skees-Reed presentation of the benefits of research gains. However, that point will not be elaborated upon in this discussion.

The authors here are suggesting that current data are limited in utility and therefore fail in being generalizable for many applications. This forces methodological constraints which result in a discipline that is slowly reactive to crisis applications. With a change in data, the authors claim that the discipline can become more anticipatory in response or, at the very least, quickly reactive to crisis applications, thus revealing their applied orientation in research.

The authors cannot be faulted for recognition of the problem. Even proponents of current data sources such as the Farm Costs and Returns Survey (FCRS) of Economic Research Service and National Agricultural Statistical Service note the need for additional "complementary" data to monitor microeconomic indicators (Baum and Johnson). Neither can they be chastened for seeking support of a specific type of data collection such as the panel design survey. If there is a flaw in their argument, it is in stressing that the panel survey ought to be "the" method of choice. It seems that some consideration could have been given to the richness of alternatives, including production data from alternate sources, other types of surveys, and creative uses of existing data. Specifically, such alternatives could involve cooperative information from farm management associations, other agricultural college departments with time series production data, and cohort and sampling testing of current

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Invited discussion presented at the annual meeting of the Southern Agricultural Economics Association, New Orleans, Louisiana, February 1-3, 1988. Invited discussions are routinely published in the July *SJAE* without editorial council review but with review of the copy editor (as per Executive Committee action June 25, 1982).

Discussion comments are based on a preliminary draft which may vary somewhat from the final article. Comments by Francis Epplin and Daryll Ray are much appreciated.

survey data such as FCRS to verify data and/or sample groups for consistency and representativeness.

The obvious question of whether available resources may be the issue rather than the data and data collection methods remains unanswered. Also, many hypothesize that the current crisis was the result primarily of external factors beyond the control of farmers. Panel survey data may indicate impacts. However, such impacts could be masked with a lagged effect. The direct causal factor could also go undetected in the complex interaction with the environment.

If one were to push economists to the extreme, the ideal research methodology would perhaps provide a way to get inside the very psyches of economic actors (undetected and without impact, of course) in such a way as to know the process each uses to make economic decisions and to identify the factors which affect the process. Too, it would ideally enable them to recognize activities/situations where the process (1) recurs with certainty; (2) is one of a variety of processes for various activities, but the use of each can be identified and predicted with certainty; or (3) has identifiable factors which alter it in measurable ways. In other words, the desire is to model the market and its economic agents with certainty, build change into the model as a recognizable pattern, allow for tests and analyses that go undetected by the agents, and have confidence that the results are generalizable.

That is one description of the ideal. If the researcher perceives it to be unattainable, what then is second best? And how can second best be made better? That is the focal point of the Skees-Reed paper. Their suggested tool to make second best better is the panel design survey, especially at the state level. How does it stand up under the earlier criteria? As Skees and Reed note, survey design has become more scientific (many of their sources support this; however, Dillman is particularly enlightening). The panel survey can be applied to achieve data that are credible. In some cases it may also be replicable, testable, and generalizable.

The question is one of assumptions. For example, the panel participants must continue to be representative of the population from which they were drawn. It should also be assumed that a particular participant will respond in similar fashion to similar events should the events arise again. It is not clear, however, how the process of "learning" is to be factored into or out of the survey model as participants respond to enviromental shocks. Too, the impact of research intervention on participants must be assumed to be insignificant or controllable in practice and separable in analysis, especially if research management changes fundamental aspects of the program over time.

Skees and Reed rely on the animal husbandry analogy of response to an equine virus crisis to establish a hypothetical goal for economic research. Ironically, Georgescu-Roegen focused on a seemingly similar issue in stating the reason "economics cannot follow the example of husbandry":

> The reason is that the evolutionary pace of economic "species"—that is, of means, ends, and relations—is far more rapid than that of the biological species. The economic "species" are too short-lived for an economic husbandry to offer a relevant picture of the economic reality (p. 320).

His point is germane to the issue at hand. The economic process is evolutionary. While there is a mechanical analog, its evolutionary dynamism requires periodic review and reformulation. He goes on to note that while the economic principles are universally valid in "form," their "content" is necessarily determined by the institutions within which they operate. Institutional relationships matter in the economic decision-making process. Any methodology that ignores this fact is likely doomed to failure of both predictive and descriptive results.

What sound survey research can do is provide respectable analysis of case studies to improve understanding of the actor's decision-making process over time. If the actor's pattern of response can be identified, that information can be of specific benefit to that actor. It can also provide a rationale for modeling the general population or anticipating sector trends if it is generalizable. Other tests will be required to have confidence in that assumption.

When seen from this perspective, then, the merit of such data must be weighed against the cost of collection. The authors are on target when they suggest that such decisions should be made at the state and regional level, although industry-wide impacts lend support for at least federal cooperation and perhaps funds. The "content" of economic principles could vary because the institutional environment can vary by state. States may have a

better feel for research resource availability and applicability. In many cases, such research will have a relatively low opportunity cost and significant benefit on a local/state level. In other cases, other types of data collection/methodologies such as simulation based on periodic field samples may be more beneficial. The process of cost-benefit analysis of research methodology itself could help in clarifying the preferred method. It is also noteworthy here that the funding source is not necessarily at issue. In fact, Johnston has studied the issue and found that the funding source does not make a difference.

In summary, the contribution of Skees and Reed is this: they have appropriately identified a timely issue for discussion by the discipline; they have suggested an alternative solution; and they have focused attention on the need for support of state/regional data-based research.

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