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FUTURE DIRECTION OF POLICY RESEARCH IN AGRICULTURE

ROBERT G. CHAMBERS*

Since I am the last speaker in this workshop, I will keep my comments brief and, hopefully, to the point. First, I want to say that I really have little idea as to which direction policy research is going to take in the future. However, I do have some strong feelings as to the direction policy research should take. During much of this workshop, I have sensed an undercurrent of feeling among many of those engaged in policy analysis that much of academic research is irrelevant for policy purposes.

More than once I have heard someone refer to a particular policy problem that was posed as a subject of study by a panel of academic researchers only to have the academics arrive at an answer well after a decision had to be made. To my mind, this is putting the cart somewhat ahead of the horse. By its very nature, good research cannot be scheduled. To expect a researcher to deliver quality research within a short time constraint is to doom oneself to constant frustration. Policy makers should realize that academic and other research forms the basic corpus of knowledge that every "real world" or "seat of the pants" policy analyst uses.

If the problem were restricted to the frustrations of a small group of policy analysts then I think little more needs to be said on this issue. However, I feel the problem is much more pervasive and thus more threatening. This tendency for some policy analysts to view academic research as irrelevant ties in closely with a mood that is prevalent in much of the agricultural economics profession at present. Roughly speaking, this mood or feeling is that theoretical research should be left to the general economists while agricultural economists should only deal with "real world" problems.

This attitude is particularly unfortunate since I do not believe that agricultural economists can afford to allow general economists to shape entirely the direction of future theoretical research. My reason is simple. General economists rarely if ever ask questions that are directly pertinent to agriculture. The theoretical constructs and concepts which they develop often times have to be quite significantly modified to be applicable to the agricultural arena. Thus, it is inescapable that a certain contingent of agricultural economists should be actively engaged in theoretical research on agricultural problems if agricultural economists are going to do their job.

I feel even more strongly that something must be done about the tendency for those of us in agricultural economics who are not actively engaged in theoretical analysis to classify analysis of this type as esoteric, irrelevant, or meaningless. While the immediate implications for the real world of much theoretical research may not be apparent, few would argue that the theoretical work done by early economists such as Marshall is not indispensable for modern day practitioners of agricultural economists. Just because the payoff from theoretical research is not immediate does not mean it is non-existent.

*Assistant Professor, Dept. of Agricultural and Resource Economics, Univ. of Maryland.

This tendency is all the more disturbing since in many instances agricultural economics has gone from an innovator in the area of theory and methods to a follower of the rest of the general economics profession in these areas. Schultz set the tenor for much of modern empirical research in the area of demand and applied analysis while studying agricultural problems. Similar statements are true about the Working brothers, Waugh, and a multitude of other economists who were actively engaged in the solution of agricultural problems.

These economists realized what many of us seem to be missing today. Agriculture poses many unique and interesting problems that cannot be solved without an equally good grasp of economic theory and the agricultural perspective. The very complexity of the agricultural situation in the U.S. and other countries guarantees that there will be many instances where relatively simple theoretical constructs and tools will just not be applicable and if agricultural economists are going to attempt to realistically model the problem they are going to have to use very sophisticated theoretical, mathematical, and econometric techniques. The scientific law of Occam's razor encourages the researcher to choose the simplest alternative which accurately explains the phenomenon under study; it does not tell us just to choose the simplest alternative.

A more recent case in point is the rapidly burgeoning interest in the area of commodity price stabilization. This area has long been the subject of keen interest for a small but intrepid group of agricultural economists. It is only with recent developments in international commodity markets, however, that general economists have turned their interests to these issues. If a few agricultural economists had not been willing to spend a good deal of time theorizing on this problem, the general economics profession would have a much weaker grasp of the issues involved. It is here and in a host of similar type problems that I think there are areas that agricultural economists cannot afford to eschew any and all theoretical approaches to problems involving agricultural commodities. As I constantly remind my graduate students, it is harder to be a good agricultural economist than a good economist. To be a good agricultural economist, one has to have more than just a sound grasp of economic theory and methods; one also needs an understanding of agricultural issues and problems.

In closing, let me just say that I hope no one takes these remarks as implying that all of research in agricultural economics should be highly theoretical or mathematical. Rather, they should be seen as a plea for a bit of tolerance on the part of the profession for those instances when the truly complex nature of agricultural problems forces an agricultural economist to utilize relatively sophisticated theoretical and mathematical implements in their reasoning process. Thank you for your attention.