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RISK ANALYSIS FOR AGRICULTURAL
PRODUCTION FIRMS: CONCEPTS,
INFORMATION REQUIREMENTS AND POLICY ISSUES

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THE ROLE OF MICRO-RISK BEHAVIOR IN
AGGREGATE RELATIONSHIPS: A DISCUSSION

Lindon J. Robison

Just and Zilberman (JZ) have introduced us in their paper to the difficult topic of aggregate relationships under risk. For merely broaching the subject they deserve applause. Their challenge was to model choice behavior for a collection of individuals obeying expected utility maxims with differing attitudes towards risk facing realistic choices of distribution. To obtain such a goal readers should be willing to concede them most of their simplifying assumptions. Two parameter choice distributions (mean and variance), single parameter utility functions, and fixed proportion production functions fall in the class of acceptable assumptions. Less acceptable is to assume each decision maker uses completely a fixed quantity of land. This assumption ignores quite an active land rental market. And finally an unacceptable assumption is that credit availability is related to assets controlled rather than equity or cash flow. Indeed, the role of credit as a reserve to avoid liquidation charges or take advantage of new opportunities is ignored.

On the positive side--a lot of relevant features are included in their model. In this class of useful features are distributions of risk attitudes and endowments, imperfect capital markets, fixed adoption costs, diversification opportunities, and credit constraints.

One can hardly complain that JZ have not been generous in their inclusion of real world features in their model. But their effort to introduce some real world features while ignoring others illustrates the fine tension between realism and tractability; one does come at the expense of the other. Still I find myself in agreement with their perception that "the workings of empirical models with great detail can hardly be understood without theory (p. 4). Moreover I also believe, like they do that useful empirical modeling in the aggregate must begin with the proper micro-macro linkage which they attempt to build.

On the other hand, whether or not JZ's model is a useful abstraction of the real world depends on the outcome of empirical tests. In this regard, Shultz writes:

"Sooner or later every economist learns that when he appeals to theory for guidance he is told, "You can't miss it." When he turns to data, he is swamped with ambiguities, and when he thinks he has found something, it looks beautiful but like a rainbow it won't stay put" (p. 1).

Thus it is quite likely that the most important and most difficult part of the JZ paper has not been written; namely, the empirical test.

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Model Results

Perhaps the most important contribution of JZ paper is that it finally provides a theoretical framework for analyzing distributional impacts of farm policy under risk. With their relatively simple constraint sets, 4 adoption/farm-size relationships develop: nonadoption, partial adoption due to the credit limit, full adoption, and partial adoption due to risk. The segmented class of decision makers arises because of the discrete nature of the adoption variable, and the credit and land constraints. Thus interior and corner solutions become relevant.

The paper then explores a number of interesting policy questions in the context of their model. Equity questions are addressed along the way. The final result is as JZ point out: "The results show that a number of standard neoclassical results can fail and that some common thinking about the distributional effect can be incorrect" (p. 44). This result is not surprising given the uniqueness of the approach and the richness of their model.

My concern for their model is the manner in which farm policy affects the firm. In the JZ model, various policy variables impact the firm by altering the firm's variance and mean, in essence by causing a symmetric-mean preserving spread as a result of changing the variance or by shifting the distribution as a result of changing the mean. This simply does not describe well the impacts of policy variables. Price supports programs, for example, truncate left hand tails of distribution. Insurance programs in which farmers pay, shift the distribution to the left, truncate and pile up probabilities in the left hand tail and increase the expected value of the final action choice.

Unfortunately, there are no quick and easy solutions to model these effects on probability distributions. To add more complexity to the model may discourage potential readers and make the resulting more difficult to interpret. And maybe the essential features of the agricultural sector have been sufficiently captured to be a suitable predictor. Only an empirical test of this paper's results can indicate whether or not the model is adequate.

Finally, the constant land price assumption limits the model's usefulness as a policy tool. Since land is roughly 75% of the farm sectors inputs, to ignore the effect of farm policy instruments on land values renders the model a not very useful policy tool.

Still these concerns should not obviate the important contributions of the JZ paper. They have produced a reasonably tractible and in many respects believable model with micro-macro linkages. I do not have much hope it could be supported empirically but that is not likely to be its major contribution. Its major contribution, perhaps, is to point out the importance of distributional questions and suggest a methodology for building models which then can be tested and interpreted. If indeed this effort follows, we will have all benefitted from the JZ paper.