A SURVEY OF AGRICULTURAL ECONOMICS LITERATURE

VOLUME 2

Quantitative Methods in Agricultural Economics, 1940s to 1970s

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Published by the University of Minnesota Press, Minneapolis, for the American Agricultural Economics Association

Discussion of George G. Judge's *Estimation and Statistical Inference in Economics*

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My first knowledge of this field was obtained at Iowa State in 1936 when, as a graduate assistant, I was asked by Professor Geoffrey Shepherd to work with J. Russell Ives in an attempt to show the relation between graphic and mathematical regression analysis. After much work, and a transfer to the old Bureau of Agricultural Economics in the USDA in Washington, we developed a paper which we submitted to the *Journal of Farm Economics*. The editors of the *Journal* replied that it was an excellent paper but that they felt it should be submitted to the *Journal of the American Statistical Association*. In the meantime we had decided that it should be consigned to the wastebasket!

In about 1940 the USDA hired a mathematical statistician, M. A. Girshick, to work on the measurement of clothing sizes for children. I told Girshick that the graphic method yielded approximation to the true mathematical partial relations. He said, "You are wrong." I said, "I am sorry, sir, but I know I am right." So he said, "I will prove that you are wrong." After five minutes of what to him was simple algebra, he proved that I was right. So he became interested in the problem and showed that the Bean [1929] method of successive approximation is a geometric equivalent of a mathematical iterative approach that converges to the true least squares regressions. Problems arise at times because, if the independent variables are highly correlated, the initial estimates of the regressions probably will deviate widely from the true partial

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regressions and the speed of convergence is very slow. Thus in such cases the graphic analyst is apt to stop long before he reaches the true slopes. These conclusions were published by Foote and Ives [1941] in a mimeographed BAE report. Since the distribution of this report was limited, the conclusions were restated in an article by Foote [1953].

In the meantime, serious questions were raised by agricultural economists about whether multiple regression could be used to measure economic relations. The argument was advanced that these methods had been developed to apply to experimental data and could be used only with such data. At a conference on price analysis at the 1936 annual meeting of the American Farm Economic Association, Sturges [1937, p. 699] said: "our current attitude toward correlation constants, whether of mathematical or of graphical derivation, is one of skepticism, or at best, of uncertainty. With a decade or more of none-too-successful economic forecasting behind us we rightfully wonder if a high correlation coefficient or a low standard error of estimate is really any basis for assurance that our forecast, or inference as to the future, will be sufficiently correct for practical needs. Our inferences as to the future are being based very largely upon our own personal opinion of what 'common sense' consists and not upon a thoughtful, but purely objective, consideration of the data as a sample from an infinite universe. Probability, in its precise sense, is seldom a factor in our inferences."

A year or so later Sturges left the Bureau of Agricultural Economics, returned to farming, and has not been heard from since. Some time later George Judge gave up and turned to linear programming, but the references cited by him relating to the 1970s suggest that he has returned to the fold. Still later economists in the USDA became involved with the logistics of food allocation and supply for agricultural commodities during World War II and forgot about theoretical problems relating to multiple regression.

In 1946 I was employed by a consulting firm to develop price analyses to predict where prices would go when ceilings were removed. This was a challenge because historic data on free-market prices ended in 1941, and predictions were needed for 1946-47. Armed with desk calculators, two clerks and I worked for six months and developed least squares regressions for ten or so commodities that saved our client millions of dollars by telling the client whether to go long or short in futures contracts. This restored my faith in the application of regression analysis to economic data.

In 1950 I returned to the USDA in Washington. Karl Fox introduced me to the simultaneous equations approach and, with the help of Klein [1953], I learned to use this fascinating new tool. Our first model was developed from a notion formulated by Bob Post, who had been writing material for *Wheat Situation* (a quarterly report by the USDA Economic Research Service on

market intelligence, outlook, and policy for wheat) for twenty years or more. Highly experienced clerks worked for six months on desk calculators to fit the model by limited information. Fortunately, the coefficients seemed reasonable. The rationale for the model was described by Meinken [1955], and Foote and Weingarten [1956] showed how this and a related model could be used as a guide to price-support policy.

Other models worthy of mention that came out of the USDA during the period were those by Rojko [1957] on dairy products and by Gerra [1959] on eggs, and later models by Harlow [1962] on hogs and by Hee [1967] on potatoes. George Kuznets and I attempted to develop methods of analysis for consumer panel data on citrus and related products (Kuznets and Foote [1954]), but little of practical value was completed.

In 1957 I joined a newly formed consulting firm that wished to emphasize application of econometric methods to price forecasting. Harry Eisenpress, who had done much of the programming on the Census method of seasonal adjustment, joined IBM at about this time to develop a full information program for their computers. We fitted models by full information relating to Maine potatoes, eggs (including the yolk/albumen/whole egg complex), and cocoa beans and products. The egg model was a dud. As a last resort, I used what now would be called a first-round equation. But the cocoa model was a great success. The model we presented to our clients was bearish. The clients argued that the price trend, if one prevailed, could only be up. Within six months, the price had dropped by 30 percent! I have been told that the model has predicted several other major turning points, some of which were missed by the trade.

Making and losing money occupied my time for the next several years. As they say, "Those who can, do; those who can't, teach." So I ended up as a professor at Texas Tech. Somehow I learned about three-stage least squares. The Thornber and Zellner computer program [1965], adapted for the IBM 360/50, was obtained from the USDA in Washington. I had traded on the pork belly futures market and was convinced that a system of equations was needed to predict these prices. A graduate student fitted quarterly models by three-stage least squares as a term paper, and we then obtained a grant from the United States Commodity Exchange Authority to refine them. The results were published in two articles (Foote, Craven, and Williams [1972] and Foote, Williams, and Craven [1973]). The models gave useful price forecasts outside the period of fit for a year and a half.

What can we conclude from my experiences and Judge's excellent review? First, successful models have been formulated and fitted despite all the theoretical objections and the fact that one generally must work with poor data and small samples. To fit good models, one must have access to sound knowl-

edge about how the particular sector operates and a certain minimum volume of acceptable data. Second, based on my research, methods that have been shown to be best by Monte Carlo studies—namely, full information and three-stage least squares—also seem to give the best results in applied work. Third, the need exists for sound research to adapt these methods and to develop other methods of this type as decision-making tools both for industry and government. This, I believe, is generally recognized, although many economists may doubt that existing methods are practical. Fourth, progress will continue to be made, first by mathematicians who develop and refine the methodology and then, perhaps much later, by those of us who work in applied areas. As noted by Judge, the electronic computer has been of immense value in the application of some of the newer techniques.

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