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Discussion: Empirical Analysis of Agricultural Commodity Prices, A Viewpoint

David A. Bessler*

I congratulate Professors Tomek and Myers on a fine review of "structural" and "time series" methods as applied to the econometric analysis of agricultural commodity prices. Further, I second their list of recommendations for improved practice in the field of price analysis. My discussion covers two points. First, I discuss their paper from the perspective of Kuhn's model of "Scientific Revolutions". Perhaps the two major modeling efforts described in their paper can be understood as alternative paradigms. Viewing the subject from this perspective may help us see the future of applied price analysis. Second, I comment on their use of the word structure. I argue that it will be difficult for us to obtain structure from either Tomek and Myers' "structural models" or their "time series" models. This may show up in our work as fragile parameter estimates and is due to the "omitted variables" problem. While I offer no solution to the problem, I make one suggestion which may help in some settings.

Paradigm Shift

It appears as if Tomek and Myers have offered us two separate papers. Perhaps this is because the authors have spanned two alternative paradigms in applied econometrics. One might view "structural econometrics" as the existing paradigm from about 1945 through 1975. What we did as price analysts using "structural models" was the mopping-up of normal science. This work had at its core the Cowles Commission agenda for econometric research, as laid out in Haavelmo (1944). Strong doses of prior theory (essentially the neoclassical theory of the firm and/or the consumer) were used to suggest a particular model - - that is, the set of endogenous and exogenous variables.

For the most part parameter estimates were generated using either ordinary least squares or one of several simultaneous equation estimators applied on observational (not experimental) data. The work resulted in an impressive list of accomplishments - - probably none more impressive than the modeling work done at the USDA in the 40's, 50's and 60's (see Fox (1989) for an interesting description of this period).

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The Cowles Commission approach to modeling fell on difficult times in the early 1970's. Rausser describes the experience at the USDA:

To the U.S government officials who were struggling to control inflation... the tremendous increase in food prices was indeed a bitter disappointment, it became crystal clear that the constructed models of the USDA were no longer viable. The forecasts generated by these models appeared to be outliers in comparison to the actual behavior of the system (p. 2).

While one approach to the crisis of the early 1970's was to build larger models, seeking linkages with the macro sector and/or the international sector, another approach was to seek models which forecasted well -- it is this forecasting approach which gave us the new paradigm, which is the subject of the "second paper" in the Tomek and Myers offering.

That the material of this paper represents a episode of a scientific revolution, note that following the period of crisis (the early 1970's) the econometric literature engaged in essay writing of a type not common in previous work. Indeed Sims' 1980 *Econometrica* piece of 48 pages was unusual in style and length. Other papers which came about at roughly the same time were Leamer's *American Economic Review* plea for taking the "con out of econometrics" and Lucas' 1976 Rochester series critique on the instability of parameter estimates under differing policy regimes. Kuhn suggests that such essay writing will be the case (p. 91):

Confronted with anomaly of crisis, scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research.

The Leamer, Sims and Lucas papers were unlike the usual journal paper, which represented the "mopping up" and efficient style of paradigmatic communication.

Following Kuhn we should not be particularly surprised to note the two literatures don't fit together well. Problems addressed by the former are not addressed by the later and vice versa. Thus in "paper one" Tomek and Myers are discussing parameter estimates and elasticities; while in "paper two" they are discussing impulse responses and forecast error decompositions. Researcher workers who follow the pattern laid out in paper two virtually never discuss parameter estimates. Indeed Sims (1980) cautions us not to bother.

Finally, the new paradigm reaches back to earlier identified ideas or solutions, which were overlooked or not viewed as revolutionary at the time when they first appeared. Thus the VAR

modeling of Sims is motivated by identification problems brought on by rational expectations and general equilibrium. Liu (1960) twenty years earlier had identified this problem and suggested the reduced form solution which looks very much like Sims' VAR. Further, similarities can be found between the time series approach and the even earlier literature of Burns and Mitchell (which was temporarily rejected by the established paradigm see Koopmans (1947)).

If one accepts my interpretation that Tomek and Myers review two distinct paradigms, then it is, perhaps, helpful to consider what Kuhn says about the "scientists" behavior in the face of two paradigms:

Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. Because it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about particular choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense.

This point speaks directly to Tomek and Myers' suggestion:

. . . multivariate models are presented in stark contrast to structural models. It is our view, however, that no single approach is best. Rather, models should vary with the research problem and available data. . . . When either a time-series or a structural model can be considered, it is important to appreciate the similarities of the two approaches rather than just focus on the differences.

Perhaps they are correct, but my experience, which appears to be consistent with Kuhn's observation, is that we operate in groups or camps, with little crossover between. Reviewers of refereed papers (and perhaps even journal editors) will be trained as either a "structural" or a "time series" econometrician and will offer recommendations which reflect (to some degree) their paradigm. Actually, the fact that Tomek and Myers look for and find similarities between the approaches is one of the commendable aspects of their paper. They do what (my reading of) Kuhn suggests won't happen.

Structure

Here I have argued that the paper by Tomek and Myers is a summary of techniques from two distinct research paradigms. Yet, I think there is one common ground which both share: the illusion that their modeling exercise will result in structure. It would be helpful had they given us a formal definition of structure. Having none, perhaps I've misunderstood their paper. Let me suggest that they interpret "structure" as the parameters of demand and supply as suggested by economic theory. And thus, in a linear model, structural parameters are the derivatives of the dependent variable with respect to the independent variable. There are very good reasons for us to question our ability to find this type of structure with observational data (or at the very least to convince ourselves that we have it when we've finished an econometric exercise). All of this is set out in Pratt and Schlaifer (1988). In my view, an econometric model fit with observational data (be it a Cowles Commission FIML model or a VAR) can only summarize regularities in the historical data. Put in another way, our work with observational data is associational inference (Holland, 1986). We may be able to convince ourselves that it makes sense to partition shocks into demand side and supply side shocks, however we can never be sure that our included variables are not correlated with other "missing" variables which are the fundamental driving (causal) force.

This point might explain some of the fragile parameter estimates that the authors describe in the paper. The only conceptual way around this problem is to set up an experiment, so we can be sure by design that our right hand side variables are not correlated with omitted variables. In lieu of an experiment, evidence to convince ourselves that we have a structural relationship, from both "structural" models and "time series" models, would be robustness of results with respect to many alternative sets of otherwise omitted variables (Pratt and Schlaifer (1988)). But even after many such alternatives have been tested, we still can not say that we have properly accounted for all possible omitted variables.

If we are willing to admit experimental methods, analyses with observational data provide a first step in a dynamic communication between researchers. Those working on observational data would provide candidate relationships which appear to be structural. These are then tested in the laboratory, with proper random assignment of subjects to treatments, to "guarantee" the internal validity of our results. Ruppel and Fuller's (1992) study of information disclosure in imperfect markets using laboratory methods follows such a dynamic. Earlier results with observational data, related to railroad deregulation, had suggested certain results, these were tested with experimental methods. The hypothesis generation ideas discussed by Tomek and Myers are certainly a necessary first step in this type of research program.

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