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**INDUSTRIAL ORGANIZATION AND
INTERNATIONAL TRADE:
METHODOLOGICAL FOUNDATIONS FOR
INTERNATIONAL FOOD AND
AGRICULTURAL MARKET RESEARCH**



*Organization
and Performance
of World Food
Systems: NC-194*

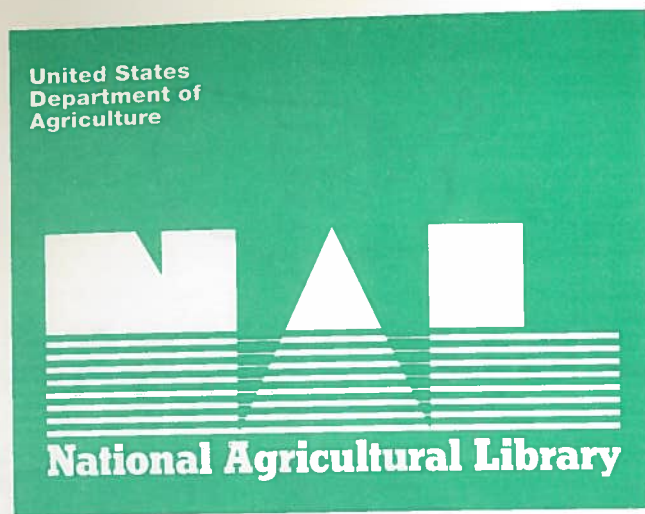
Ian M. Sheldon and Dennis R. Henderson, editors

North Central Regional Research Publication Number 334

NC-194 Research Monograph Number 1

July 1992

Agricultural Experiment Stations of California, Connecticut, Florida, Illinois, Indiana, Iowa, Kentucky, Louisiana, Maryland, Michigan, Minnesota, Mississippi, Missouri, Nebraska, New York, North Dakota, Ohio, Oklahoma, Oregon, Texas, Virginia, Washington, and Wisconsin, and the U.S. Department of Agriculture, cooperating.



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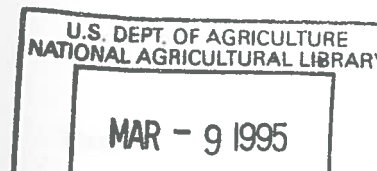
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Proceedings of a Workshop On Industrial Organization and International Trade

Editors:
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Published at The Ohio State University
Columbus, Ohio 43210-1099



Discussion: Imperfect Competition and International Trade: The Use of Simulation Techniques

Philip Abbott

International trade theorists have been exploring the notions of industrial organization and imperfect competition in revisions of our standard trade theory over the last decade. Some among them have begun to explore the empirical relevance of these issues by creating and applying partial equilibrium models embodying these ideas. IPECACs (or Industrial Policy Exercises Calibrated to Actual Cases) are reviewed in detail in Sheldon's paper. Following the spirit of this workshop, Sheldon has sought to de-mystify these models and make these approaches relevant for further empirical work. Underlying theory for each model is presented in clear and consistent notation. Calibration methods to fit these models to real world data are revealed. Less attention is paid in his paper to critiques of these models, although ample attention to their problems has been raised in other papers at this workshop, especially Marvel's. Sheldon lays out the most important criticisms leveled against these models, though with less enthusiasm for this latter task.

Two extremely useful contributions from Sheldon's paper should be noted first. His Table 4.1 provides a detailed taxonomy of approaches to introducing industrial organization concepts into trade models, showing which issues are addressed in each paper reviewed and what underlying assumptions are invoked to complete each model. His use of a clear and consistent notation across all four papers, and his complete presentation of the rather tedious algebra required to implement these models, makes his presentation more readable than the original papers and fills in many gaps needed by empirical researchers who might wish to use these models and their approaches.

My comments are for the most part an elaboration of the critiques leveled against the papers reviewed by Sheldon, along with some additions, on both critiques of this approach and suggestions as to additional literature Sheldon's review might have examined. I will also try to put this literature into a proper perspective.

Before exploring further these critiques and alternative literature which might have been examined, it is useful to review what these models do accomplish. Four concepts from the industrial organization/imperfect competition literature are introduced into standard partial equilibrium trade models:

Product differentiation
Economies of scale
Free entry and exit by firms
Imperfect competition/oligopoly/game theory

In each case, simple ideas on modifications to standard partial equilibrium trade models are implemented, but the implementations often require rather complex algebra. Sheldon has shown that introduction of product differentiation is a rather straightforward process which is critical to several of these models. Economies of scale require more tedious algebra and at times require some tenuous assumptions, especially if free entry and exit are to be assumed. Zero profit conditions must be specified to set entry/exit rules, and these are unlikely to correspond well with observed industry conditions. Oligopoly (imperfect competition) is far and away the most difficult concept to introduce, however.

While the extremes of monopoly/monopsony and perfect competition are easily handled, both in theory and practice, all models reviewed here resorted to conjectural variations to capture the imperfectly competitive dimensions of these markets. Industrial organization economists criticize this as an *ad hoc* approach neither well grounded in theory nor revealing much about strategies employed by firms. Trade theorists are not particularly pleased with the need to use conjectural variations, either. Sheldon notes Krugman's (1986) observation that "committing" conjectural variations may be an unpleasant modeling choice, but at least for the moment it is the only game in town.

The use of conjectural variations to capture imperfect competition is subject to the strongest criticism leveled against these models in the literature. But game theorists offer only multiple equilibria and complex yet unrealistic firm strategies (from the perspective of actual, observed firm strategies) as alternatives. What is needed are simpler means of introducing firm strategy from the game theorists, more soundly grounded in the actual behavior of firms, or better interpretation of the conjectural variations results, if these objections are to be overcome.

Agricultural trade economists have used several other *ad hoc* modelling methods with even less theoretical underpinnings than conjectural variations (e.g., price transmission elasticities). What is vexing about this technique, however, is that there seems little reason for the conjectures to remain stable over time. Paarlberg's (1983) work in the wheat market in fact finds estimated conjectures varying from year to year. Given the *ad hoc* nature of this modeling approach, and

the possibility that firm and country trade strategies vary from year to year, this problem should not be surprising. It makes simulations which require assumed conjectures less useful as policy tools, however. These approaches are more useful as *ex post* analytical tools.

Poor econometric approach (or lack thereof) is the second most important criticism leveled against these studies. Only Thursby and Thursby (1990) estimate the parameters of their model. The remaining authors take parameters from previously estimated models, in which perfect competition was assumed, and make no attempt to correct elasticities for their assumed market structure. The complexity of the estimation problem in Thursby and Thursby suggests that this is no trivial problem for researchers. Obtaining stable and meaningful parameter estimates will be troublesome. Imperfect market structure exacerbates considerably the identification problem inherent in estimation of market behavior parameters.

These studies have also been attacked as inadequate empirical exercises. The feeling conveyed at this workshop is that these studies needed to pay greater attention to empirical and institutional details if useful simulation models are to be created. This theme of a need for studies well grounded in empirical work on industry structure and firm behavior ran throughout the conference.

The only estimated model, that of Thursby and Thursby, which also had the most institutional detail of any of these four studies, was also subject to the most criticism at this workshop. Agricultural economists have studied the international wheat market for years, so they found the exclusion of Australia from this model and treatment of the Japanese Food Agency as an entity with no market power as inadequate. Having observed this model presented at a conference, I believe the authors were more interested in showing an implementation of their theoretical multi-level oligopoly model, which introduced marketing boards as economic agents, than in developing a policy tool for the wheat market. Similar observations and empirical critiques could probably be aimed at the other three studies, and some were, although this group knew most about the wheat market, so the wheat study drew the strongest criticism.

It must be borne in mind that the research reviewed by Sheldon comes from a group of researchers with a theoretical rather than empirical tradition. Their motivation in conducting these studies was probably to demonstrate that the theoretical concepts they developed are relevant to policy. While the empirical content of these studies is seldom adequate to use in setting or even assessing policy, that was never the primary intention of these authors.

There is other work implementing industrial organization concepts in trade models not reviewed here. One body of literature, often not counted as part of the trade literature, is Computable General Equilibrium (CGE) modeling. Recent attempts to introduce economies of scale and "rationalization" of industrial structure after the removal of protection, by Harris (1984) and his colleagues, in CGE models is clearly of relevance to simulation of trade with imperfect competition. Their results suggest that elimination of imperfect competition may be an important source of benefits from trade liberalization, and the small benefits found in earlier studies are due to the exclusion of such effects. Several authors have subsequently pursued this idea, especially in CGE models evaluating trade liberalization. Product differentiation, at least between home goods and tradeables, has also long been a characteristic of empirical CGE models. Furthermore, the CGE model structure is closer to the "ideal" Heckscher-Ohlin-Samuelson model which provided the underpinnings for "old" trade theory, than is the partial equilibrium structure of the IPECACs.

Another lesson to be learned from the CGE modeling literature concerns the calibration methods applied in the IPECACs. CGE models have used restricted and general functional forms (linear expenditure systems, constant elasticity of substitution functions and translog functions) to great advantage in benchmarking empirical models to industry conditions. The authors of IPECACs were more interested in maintaining general theoretical forms than providing useful frameworks for empirical analysis, while the latter has been a paramount goal of the CGE studies. Sheldon's review suggests that the calibration methods of these approaches are either convoluted or not revealed. His review gives nearly the same scant attention to this practice as is found in the studies reviewed, with far greater attention to theory. CGE studies generally do a better job of marrying empirical approach with theory than is found here, especially in presentation, but also in implementation.

The bulk of the related literature in agricultural trade has focussed on imperfect competition by public rather than private bodies, and there is a considerable literature developing trade simulation models in imperfectly competitive markets. Karp and McCalla (1983), Kolstad and Burris (1986), and Paarlberg and Abbott (1986) all used variations on conjectural variations models, though for government marketing boards. The contribution of note in Thursby and Thursby relevant to agricultural markets comes in their attempt to model the interaction of public and private institutions, both with potential market power.

An important element of the agricultural trade literature also concerns the policy objectives of governments, which are clearly related to income distributional concerns. The failure of the IPECACs to address questions of appropriate policy objectives and income redistributional consequences has surely been one source of dissatisfaction with these models as policy simulation tools.

If we admit that conjectural variations is an *ad hoc* modelling approach, there are alternatives to modeling imperfect competition among firms in an open economy which should be of interest to this audience. Structuralist macroeconomists such as Taylor (1983) have departed from the CGE modeling approach for reasons similar to those of concern to industrial organization economists. They argue that adjustment mechanisms in most CGE models are inappropriate because they fail to capture real world institutions, including oligopolistic behavior by large firms. Structuralists use mark-up pricing rules for the oligopolistic firms, and profit rates drive investment (entry) without resorting to zero profit conditions. Their studies have examined structural adjustment reforms and exchange rate devaluation by LDCs and find very different conclusions from those of orthodox trade theory. These are admittedly much simpler ways of introducing imperfect competition, but there may be an advantage in their simplicity. And the structuralists would admonish us to pay attention to institutional detail, to the way firms actually behave, and to more realistic market adjustment mechanisms.

In summary, Sheldon has provided a useful review of a set of papers which should prove very valuable to agricultural trade researchers wishing to address the problems raised by industrial organization economists. His review is particularly helpful in comprehending and filling in missing details for the four papers reviewed. His review took a rather narrow focus, and emphasized less than others at this conference the shortcomings of those models. Especially as empirical policy exercises, there are important limitations to those studies. They do not (nor should they) offer a general trade theory to substitute for the old standard. They do point in several directions for further work in this area, however. Several related literatures suggest improvements or alternatives to these approaches, as well.

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