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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*

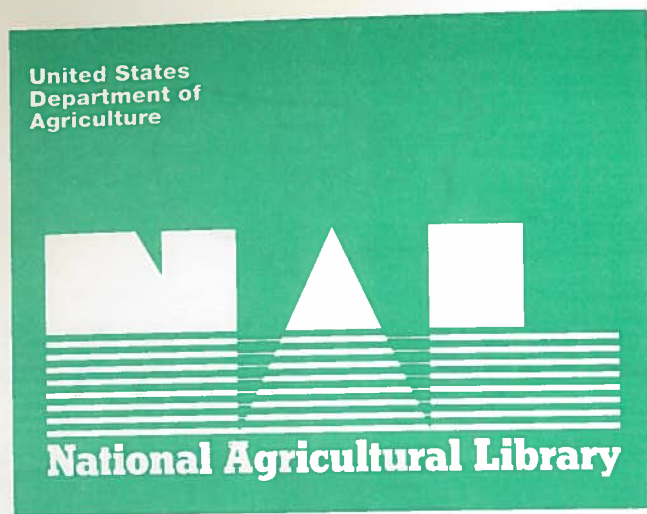
Ian M. Sheldon and Dennis R. Henderson, editors

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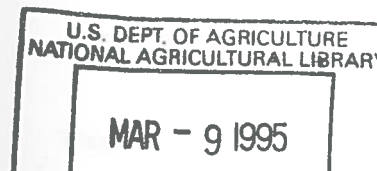
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Chapter 1: Perspectives on Imperfect Competition and International Trade

Howard P. Marvel

1.1 Introduction

This paper offers an idiosyncratic and somewhat curmudgeonly guide to the intersection of two new and very exciting literatures. These two literatures share a common concern with imperfect competition. They share a common technique as well, the technique of noncooperative game theory. The older of the two literatures is still new enough to be termed the "new industrial organization" (NIO). Its more recent cousin, which we shall label the New Trade, is quite recent indeed. This paper is an assessment of the contributions of these new literatures provided from the viewpoint of the more traditional literatures they aim to supplant.

The NIO literature applies the techniques of noncooperative game theory to understanding the interactions of rivals who are large relative to the markets in which they operate. Indeed, each is large enough so that no individual rival can safely ignore the way in which its rivals will respond to its own actions. The NIO literature models a world in which strategic concerns predominate. The technological factors that produce markets dominated by a small number of rivals also imply substantial quasi-rents, the distribution of which becomes the object of strategic manipulation. Firms cannot concern themselves solely with managing their own affairs, but must also attempt to manage each other through their own actions. The NIO thus expands dramatically the range of possible interpretations for firm behavior, illuminating in the process much conduct that had previously been difficult to interpret.

The NIO is a break from the past tradition of modelling markets as either competitive, in which event firm interactions could be safely ignored, or monopolistic, in which interactions were assumed absent. The literature thus fills a gap between two existing approaches, each of which continues to be useful for a wide range of questions. In comparison, the second literature, the New Trade, is more radical. The New Trade springs from a belief that its predecessor, the Heckscher-Ohlin-Samuelson model of trade flows based on factor endowments, has been overtaken by events. Given this perceived inadequacy, New Trade attempts nothing less than a complete replacement of existing theory.

The trade flows of interest are those between developed nations and consist of goods produced with similar technologies and factor combinations. Indeed, the traded goods are thought to be so similar that one can reasonably take the location of production capacity as arbitrary, at least from the standpoint of global efficiency. From a distributional standpoint, however, location decisions can matter a great deal. In this view, productive facilities throw off rents and beneficial externalities that can be captured locally, within the confines of the political unit within which the facility resides. Thus, while from some perspectives location does not matter much, location decisions will be of intense interest to policymakers responsible for the well-being of the residents of individual political units. The result is a neo-mercantilism in which strategic considerations are again paramount. The goal of policy is to induce local producers to capture as large a share of world trade as possible, throwing off local benefits as they do.

The lineage of these new approaches is clear. The NIO developed a set of tools to cope with the strategic behavior thought to be pervasive under imperfect competition. As trade analysts came to believe that trade flows were increasingly intra-industry and thereby not susceptible to interpretation by comparative advantage considerations, it was natural to adapt the strategic analyses of the NIO as a substitute. But while the intellectual trade flow between the NIO and the New Trade has thus far been primarily one-way, characteristics of the trade application suggest that this intellectual trade imbalance may soon be diminished. When firms are residents of the same political unit, there may be reasons for policy to try to affect the common set of rules under which each operates. But such intervention is likely to be non-discriminatory in character.

In the trade arena, however, some firms will clearly be preferred to others. Tariff and non-tariff barriers now, however, need no longer concern themselves solely with protecting existing rents from competitive intrusions, but can also be designed to affect the strategic interplay of foreign and domestic rivals. For the NIO, this additional scope for policy offers the possibility of application and testing of theories that have proven difficult to assess within the contexts for which they were developed. Trade policies put in place to alter the strategic environment in which firms compete afford the possibility of doing a form of comparative statics on strategic equilibria that is not ordinarily possible.

For this approach to work, government policy must be viewed as an exogenous influence on market conduct, rather than a product of an intimately linked strategic process. The literature has appeared willing to accept this limitation in order to make progress. In any

case, the extension of the NIO models to trade increases the range of possibilities available for testing and thus promises to enrich both the trade literature and the industrial organization (IO) literature.

We will see in the following discussion that the possibilities offered by the conjunction of trade and IO are too wide to be useful. Strategic models can be constructed to yield virtually any conclusion that a competent investigator might wish to reach. Policy advocates of all stripes can find comfort in this literature, in part because the conclusions reached are distressingly non-robust to changes in the set of rules that players are supposed to follow. It is useful to have an understanding of this sensitivity and of the degree to which the New Trade modeling rests on a reasonable basis in fact. Accomplishing this task will also permit us to assess whether extensions of existing work, most of which appears to target trade in manufactures, can be useful in understanding the economics of trade in agricultural products.

We begin by providing a brief survey of applications of the NIO to trade analysis. As already noted, the New Trade is a radical challenge to existing trade theories, one that is called for by a belief that trade flows no longer operate as if determined by comparative advantage operating under competition. Trade flows are instead presumed to be the result of essentially arbitrary firm location decisions, decisions that matter for trade because pervasive economies of scale are presumed to prevent cloning of productive facilities in areas of similar factor endowments. Section 1.2 considers the evidence for this view and finds it wanting. Nevertheless, this defect is far from fatal, so we then proceed to consider the strategic trade models themselves in section 1.3. We then turn to a discussion of the attempts to tie the theoretical realm of strategic interaction to the markets in which the industrial policy debate goes on. Section 1.4 offers comments on calibration exercises. Section 1.5 comments on the difficulty of bringing the results of these exercises to bear on the policy process. Section 1.6 offers brief concluding remarks.

1.2 Empirical Underpinnings of the New Trade

The New Trade literature is predominantly an application of the analysis of strategic behavior developed in the NIO literature to problems of interactions among firms in different nations. The perceived need for this literature is peculiar to international trade. It arises from a casual empirical judgment that the nature of the bulk of international trade has changed dramatically, so dramatically indeed that conventional trade theories need to be supplanted by new

approaches more sensitive to the realities of trade at the end of the twentieth century. In this section, we consider whether this perception of change has a basis in the trade data. We consider as well whether the way in which strategic considerations have been introduced into international trade is optimal.

To place the trade data in context, it will be useful to begin introducing strategic behavior at this point, but we shall do so in ways different from those that characterize the New Trade. To do so, we turn briefly to the NIO literature. This literature can be characterized reasonably, if somewhat incompletely, as the application of the techniques of noncooperative game theory to understanding the decision making of small numbers of "players". The NIO is, in other words, predominantly the analysis of strategic interactions, generally among firms. While it is apparent that the scope for strategic actions to affect trade flows is substantial, it is less clear why the trade literature has followed its current path. The focus has been primarily on the strategic interaction of firms producing internationally traded goods, and not on the strategic interactions of governments. Indeed, in an era in which trade itself has served to increase the array of options available to consumers and thus seems to reduce the importance of strategic interaction, the choice of the firm, rather than trade negotiators, as the centerpiece of the analysis seems somewhat quaint.

Perhaps the most obvious way to consider strategic decisions that impinge on trade is to think of trade negotiations as confronting the trading partners with a situation akin to the familiar prisoners' dilemma. Kreps (1990) suggests such an application:

"Consider two countries that are trading-partners. Each can engage in various sorts of protectionist measures that, in some cases, will benefit the protected country, holding fixed the actions of the other. But if both engage in protection, overall welfare in both countries decreases. Again we have the rough character of the prisoners' dilemma game, and insights gained from, say, the context of rivalrous oligopolists might be transferable to the context of trade policy."
(pp. 3-9)

Obviously there is considerable scope to expand the payoff matrix and array of strategies available to the players of this negotiators' dilemma game. Techniques of cooperative game theory can handle multilateral trade negotiations. We ask when the "Grand

Coalition" solution of free, or at least liberalized trade can be achieved, and when this world-welfare maximizing solution fails to obtain because it is blocked by potential sub-coalitions. This approach seems particularly germane today, given the number of regional trading alliances that are either in place or under consideration. It is important to ask whether these alliances will hinder progress toward worldwide liberalization within the GATT framework. Here the techniques of game theory should have ready application.¹

The New Trade does not take this approach of trying to expand the payoff matrix to encompass the various possible subcoalitions and the rules governing their formation. Instead, it can be understood as arising from a reassessment of the payoffs themselves. In comparison to a more traditional approach, the New Trade makes modifications both to the gains associated with reduced intervention (the cooperative solution) and the size of the gains associated with imposing one's own trade restraints, given that the other's restraints are fixed.

Why do the gains from cooperation shrink in comparison to more traditional assessments? The answer comes from the first pillar of the New Trade, namely, the notion that nonconvexities, and not more traditional considerations of comparative advantage, have come to dominate modern trade flows. The argument is that recent increases in trade have come between developed nations, countries that are quite similar in terms of access to technology and in factor combinations. These burgeoning trade flows are often intra-industry - two way flows (cross-hauling) of, at most, modestly differentiated products. The similarities of the products being transported in both directions suggests to many observers that the old Heckscher-Ohlin-Samuelson model based upon comparative advantage is no longer binding. In a great leap, the New Trade suggests that the location of the production facilities generating the products flowing in such trade is arbitrary, the product of either historical accident or active intervention to fix productive facilities in particular locations.

It is a leap to go from the proposition that trade flows are determined from other than comparative advantage considerations to the claim that location does not matter, and it is a further leap to arrive at the conclusion that the payoff to cooperative behavior in trade negotiations is thereby diminished. But this is the nature of the New Trade. In the New Trade, the location game is nearly zero sum. The

¹ Another paper prepared for this workshop, Davis and Mitchell (1992), contains a review of some interesting work done along these lines. See pages 182-187, particularly 185-187.

notion that productive facilities are located arbitrarily is combined with an appreciation that the number of such facilities is limited, presumably either by technical scale economies favoring large facilities or by first-mover advantages such as those implied by learning-by-doing.

Arbitrary location alone does not suggest that policy should be designed to fix productive capacity within one's own borders. An additional piece of the puzzle is required, namely that such excess capacity benefits are not captured by the location decision makers. These benefits can consist of rents capable of being captured by workers or other host country residents (though not by the owners of the firm in question) or externalities generated by the activity in question. Thus an aircraft manufacturer may generate better jobs than would have been available from alternative employers. Its expertise may permit national defense to be purchased more cheaply and reliably than would be possible were the plant located offshore.

In terms of our prisoners' dilemma, this means that the returns to cooperation are smaller than are generally thought, and that the benefits of managing one's own trade while one's rival sits idly by are very substantial. Therefore the temptation to manage trade becomes almost irresistible. Since others will succumb to such temptation, one's own authorities ought to follow in self-defense, if for no other reason. Note also that the prisoners' dilemma framework leads both trading partners in a bilateral negotiation to choose the noncooperative path. This caution should be recalled when we turn to a discussion of calibration exercises.

Put in these terms, the underlying assumptions of the New Trade do not seem particularly palatable. To many, the remarkable feature of post-war international trade has been the success of GATT rounds of liberalization. Tariffs that were once prohibitive or at least substantial have been pared back. Table 1 provides an indication of the increasing importance of the GATT.² Non-tariff barriers have been resorted to, but not in sufficient stringency to prevent an explosion in trade volume. The ratio of U.S. exports to U.S. GNP was 0.064 in 1970, before the implementation of the Kennedy Round tariff reductions. By 1987, the ratio had risen to 0.093. Worldwide exports grew even more rapidly than U.S. exports over this interval.³ An analysis that seems to suggest that protection is on the rise would

² Table 1 is from Jackson (1990), p.37, as reprinted in Bhagwati (1991), p.8.

³ Data from the *Statistical Abstract of the United States* (1989).

seem to be less than perfectly suited for dealing with this evolution, unless its proponents see their predictions as self-fulfilling prophecies.⁴

Table 1: GATT Negotiating Rounds

| Round | Dates | Number of Countries | Value of Trade Covered (dollars) |
|---------|-------|---------------------|----------------------------------|
| Geneva | 1947 | 23 | 10 billion |
| Annecy | 1949 | 33 | Unavailable |
| Torquay | 1950 | 34 | Unavailable |
| Geneva | 1956 | 22 | 2.5 billion |
| Dillon | 1961 | 45 | 4.9 billion |
| Kennedy | 1962 | 48 | 40 billion |
| Tokyo | 1973 | 99 | 155 billion |

Less casual empirical research is also unkind to the presumptions underlying the New Trade. Marvel and Ray (1987) estimate the determinants of intra-industry trade. They employ the standard measure of intra-industry trade:

$$z_i \equiv \frac{2 \min(x_i, m_i)}{x_i + m_i} \quad 1.1$$

where z_i represents intra-industry trade in industry i , x_i measures that industry's exports, and m_i measures imports. Using the most disaggregated U.S. data available (generally four-digit SIC industries), they regress z_i on a set of product characteristics. The estimates suggest that economies of scale are antithetical to intra-industry trade -- higher capital-labor ratios and minimum efficient scale are

⁴ It is clear that a rapid expansion of import competition will sharply constrain the profits of domestic firms, thereby adding to calls for industrial protection. See, for example, Marvel (1980). What is surprising is the degree to which such calls have proved resistible.

associated with less intra-industry trade. This is not surprising, for one would expect that far from encouraging intra-industry trade, scale economies should lead production to be concentrated at a single source. Comparatively low scale economies permit exploitation of the gains arising from specialization. Not surprisingly, then, intra-industry trade is higher in industries where inventories tend to be held in the form of raw materials or work in process as opposed to finished goods. This pattern of inventory holding suggests that the industry's products are often made to order, taking advantage of specialized expertise held by producers.

These pieces of evidence are far from fatal to the New Trade, but they do limit the potential applicability of the analysis. It is also useful to keep in mind that the intra-industry trade estimates are based on data that are probably too crude for the purpose at hand. Broad aggregates -- and even the four-digit SIC is broad from the standpoint of trade flows -- can mask considerable differences in the products included in a particular industry's trade flows. The scope for comparative advantage to operate remains substantial even for such disaggregated data.

The evidence that location is arbitrary, either guided by providence or by outside (policy) intervention, tends to be anecdotal and there is no anecdote more popular than that of Boeing and airframe manufacturing:

"Why are aircraft manufactured in Seattle? It is hard to argue that there is some unique attribute of the city's location that fully explains this. The point is, instead, that the logic of increasing returns mandates that aircraft production had to be concentrated *somewhere*, and Seattle just happens to be where the roulette wheel came to a stop. In many of the new models of trade, the actual location of production is to some degree indeterminate. Yet what the example of Seattle suggests, and what is explicit in some of the models, is a critical role for history: Because Seattle (or Detroit or Silicon Valley) was where an industry initially got established, increasing returns keep the industry there." (Krugman, 1990, p.2)

The airframe industry is particularly suited for trade analysis since there are so few players and since the European Community has chosen to subsidize Airbus heavily to foster potential competition for Boeing (and the other U.S. airframe manufacturers). But one must

avoid generalizing too easily from such an unusual case.⁵

If we consider agricultural products, the perils of easy generalization become even more apparent. Fertilizers are not particularly capital intensive products, and neither is their measured z_i large. SIC major group 20, food and kindred product manufacturing, does contain a number of industries with either high capital-labor ratios, minimum efficient plant sizes, or concentration ratios, but the conjunction of these factors with large values for z_i is rare. Indeed, the industries with relatively large values for z_i include a number of cases in which it is apparent that aggregation could mask significant underlying variation in the commodities traded. Such industries include meatpacking plants (2011); dry, condensed and evaporated products (2023) and canned specialties (2032). High concentration industries in this group include cereal breakfast foods, wet corn milling, chocolate and cocoa products, chewing gum, and malt beverages. Of these, only breakfast cereals and chewing gum record values of z that are above the average for all manufacturing, and in neither case is the difference from the mean striking.

It is difficult to identify the agricultural equivalent of the airframe industry, a market in which firms are arbitrarily located, in which technological considerations preclude the entry of more than a small number of firms worldwide, and in which the activities of the firms in question throw off rents worthy of protection by the government of the political unit in which they are located.

The point of this discussion is to suggest that the need for a New Trade is based on a conception of trade flows that has modest empirical support. This is hardly devastating to the New Trade literature, however, as most of the existing New Trade studies have been focused on very narrowly defined markets. Our discussion here simply suggests that sweeping generalizations from such narrow studies are unlikely to have much validity. With this caveat in hand, we can now turn to an overview of the way in which the models of the NIO have been put to use in trade analysis. Given the large number of papers that have appeared on this topic, this review is necessarily quite selective. Indeed, it is more a collection of impressions about the

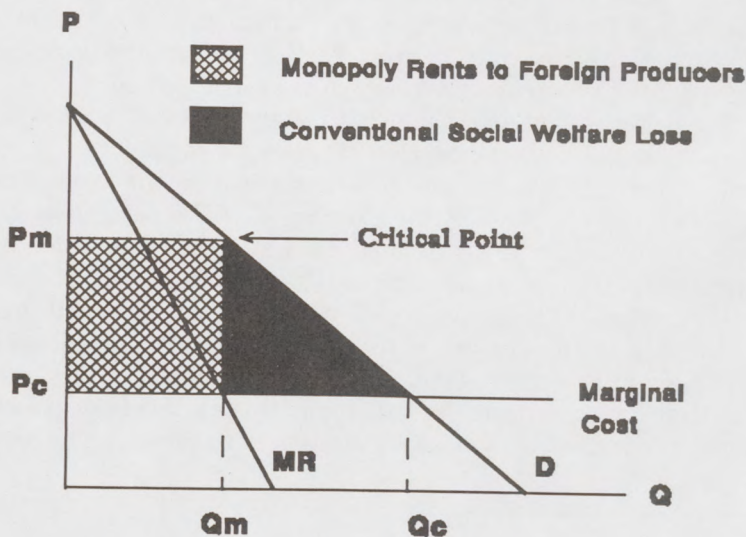
⁵ The airframe industry crops up with distressing regularity in managed trade discussions, leading one to wonder whether it might be appropriate occasionally to use a second anecdote. One possibility is the auto industry, often offered as another arena for managed trade. It is curious, then, that the 'arbitrary' location decisions of Japanese auto assemblers led to all but one of their plants being established in the fairly small cluster in the American midwest, a region that also, presumably arbitrarily, houses the bulk of the U.S. automobile industry as well.

New Trade literature than a conventional survey. I have tried to convey a sense of the source of the results of the literature without getting mired in its details.

1.3 Imperfect Competition and Trade

The actual analysis of imperfect competition in international trade is straightforward. When perfect competition is absent, price will exceed (at least short run) marginal cost. The absence of perfect competition is a consequence of scale economies, learning by doing, or other nonconvexities that limit the number of firms a particular market can accommodate. But while marginal cost pricing closes the model, in its absence some other way must be found to characterize price and quantity setting. Whatever device is chosen needs to yield enough rents to cover the fixed costs incurred to enter the industry. How do these rents affect policy?

Figure 1.1
Foreign Monopolist and Welfare

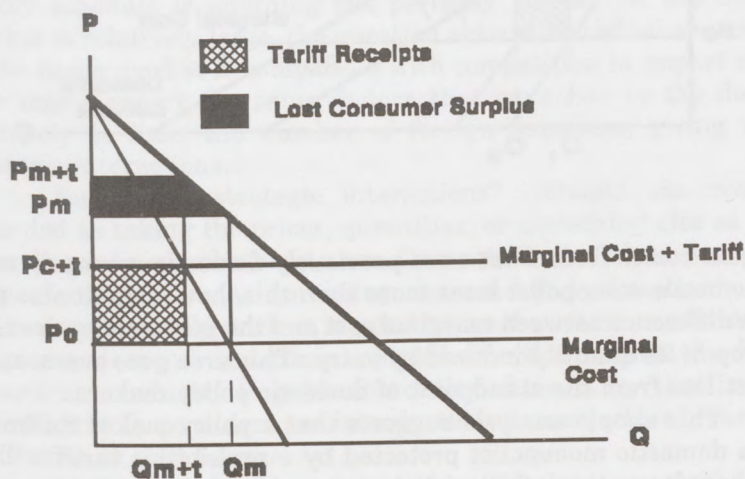


Suppose that a foreign monopolist sells to a domestic market with linear demand as shown in Figure 1.1. If the monopolist were a domestic firm, we would count its social welfare loss as the conventional consumer surplus triangle indicated in the diagram. But

if the monopolist is foreign, from the domestic stand-point, the entire loss in consumer surplus in comparison with marginal cost pricing becomes the (domestic) social welfare loss since the transfer from domestic consumers to foreign producers in the form of monopoly rent is pure cost to the domestic economy. Such rents become ready targets for policy intervention.

One of the simplest imaginable interventions is a tariff on imports supplied by the foreign monopolist. If demand is linear, a tariff will almost surely increase welfare when viewed from the domestic perspective. This is because the foreign monopolist will absorb a portion of the tariff. The domestic economy loses additional consumer surplus owing to the higher tariff-induced price of the imported good. This loss is more than offset by the higher tariff revenues received by the domestic government (as long as the additional conventional social welfare loss induced by the tariff is smaller than the additional transfer from consumers to the foreign monopolist). This situation is illustrated in **Figure 1.2**. This result is not robust to changes in the assumptions of linear demand and

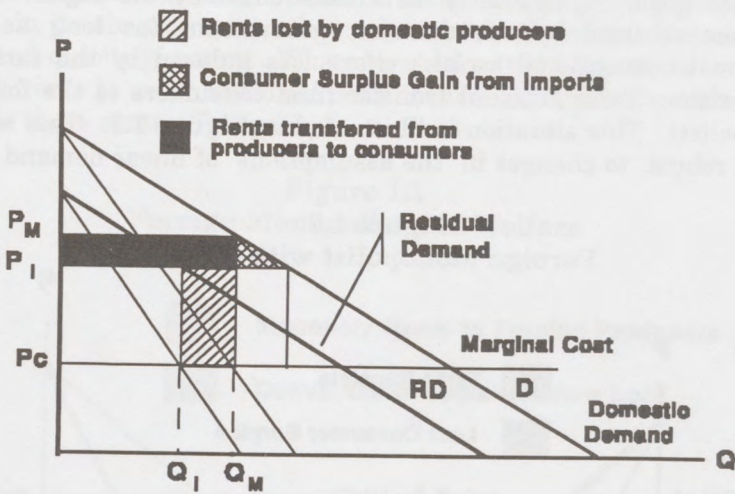
Figure 1.2
Foreign Monopolist with Tariff



constant costs, however, as Brander and Spencer (1984) demonstrate. Note also that a quota will not have the same effect as the tariff.

The situation changes drastically if the monopolist in question is a domestic firm. In this instance the monopoly rents are simply domestic transfers and the social welfare loss is the conventional deadweight loss triangle. But what if the monopolist faces a foreign entrant? Let the monopolist assume that the *quantity* offered for sale by the foreign firm is given. The monopolist then chooses price based upon its residual demand curve, as illustrated in Figure 1.3. In comparison to the autarky solution, but apart from a small triangle,

Figure 1.3
Domestic Monopolist with Foreign Entry



that gain comes from what were previously domestic monopoly rents. The domestic monopolist loses more than this, however. It also gives up the difference between marginal cost and the post-entry price times the drop in its quantity induced by entry. This area goes overseas and is a net loss from the standpoint of domestic policy makers.

This simple analysis suggests that a policy maker confronted with a domestic monopolist protected by a prohibitive tariff will not find the alternative of liberalizing through use of a quota to be particularly palatable. A quota fixes quantity as in our example. What would have happened if, instead of the quota, the policy maker had instead chosen a tariff? Suppose that instead of perceiving that the quantity of imports was fixed, the domestic monopolist takes the price of imports as given. Suppose also that the tariff is set to raise

the price of imports to P_1 , in **Figure 1.3**. The outcome is very different. The monopolist can exclude all imports by setting its price slightly below P_1 ; the tariff still appears prohibitive, but the threat of foreign competition has reduced the social welfare loss of the domestic monopoly without actually shipping rents overseas. Clearly the tariff policy dominates the quota, unless, of course, the object of the liberalization is to show progress to some offended foreign government on trade access issues.

Even though this discussion is couched in terms of strategic variables -- does the domestic firm take price or quantity as given -- it clearly does not require game theory to reach the above results. Indeed, this discussion is a variant of Bhagwati's (1965) demonstration of the nonequivalence of tariffs and quotas in the presence of domestic monopoly. But what if the response of importers to domestic conditions lies somewhere between fixed quantities (Cournot behavior, equivalent to a quota) and fixed prices? There is an immediate temptation to estimate a model that permits intermediate import responses, but with the temptation comes a dilemma. The cleanest way to model import response is with a supply function. This approach, however, assumes that foreign suppliers are price takers in the domestic market. If the domestic market is small, this may be reasonable, but then there is little reason to suppose that the import supply schedule is anything but perfectly elastic. If the domestic market is relatively large, the question arises as to whether monopoly in the home market is compatible with competition in import supply. One might expect the same factors that gave rise to the domestic monopoly to limit the number of foreign suppliers, giving rise to strategic interactions.

But which strategic interactions? Should the rivals be regarded as taking the prices, quantities, or something else as given? How do we know whether to use Cournot, Bertrand, or some other form of competition as governing the market in question? These choices are normally unclear in practice, so that there is a tendency to adopt a conjectural variations approach in order to allow the data for a particular industry to guide the choice of model for that industry. Perloff (1992) provides an introduction to conjectural variations estimation in this volume.

Unfortunately, the conjectural variations approach entails significant costs in comparison to the alternative of positing a strategic variable and developing the characteristics of the resulting equilibrium. Game theorists object that conjectural variations do not make sense theoretically. The Nash equilibria that conjectural variations are to represent are the result of simultaneous moves by the

parties. Each must move without knowing what the other does. Reactions are therefore obviously impossible. Conjectural variations are assumptions a firm makes about responses of its rivals to its own actions. Thus, conjectural variations different from zero are incompatible with the underlying theory.

There is a serious empirical objection to conjectural variations as well. As Perloff points out, estimation of a conjectural variations model yields an estimate of market power, but that estimate is based simply on the residual demand curve of the firm in question. Is this residual demand curve the result of a monopoly being confronted by a competitive fringe, or is it underpinned by strategic interaction of the firm and its rivals? Once we have used the conjectural variations approach to, in Tirole's (1989) term, escape "the discipline of game theory", we cannot then reapply that discipline to obtain additional insights about behavior from the data. It is widely lamented that conjectural variations are a necessary evil in estimating market behavior. The chief source of the evil is the breaking of the link between theory and evidence. Much of the NIO was developed owing to dissatisfaction with earlier unstructured empirical work. But until this estimation bottleneck is broken, the link between the new theory and the evidence it was to illuminate will remain unforged.

How can the situation be improved? One way is to concentrate on clarifying the institutional details that govern the operation of individual markets. The goal here is to obtain enough information to infer the nature of the game which a market's participants must play. We can 'tailor-make' a model to such markets and test to see whether its implications are borne out. The informational requirements for such an exercise are very large, so this approach is likely to be of most use for auctions and for carefully defined markets such as that for medical interns. But this tailor-made approach is likely to be infeasible for most markets. Our other alternative is to formulate a number of models for a market and then to check for implications common to all. The problem with this approach is that such implications are likely to be difficult to find and are also likely to be the sort of thing that non-game-theorists will claim to have known all along.⁶

Based upon the current state of the New Trade literature, one must conclude that the implications of the New Trade are distinctly non-robust to changes in the specification of the game at issue. The most fascinating results of the New Trade concern the way in which

⁶ For an optimistic assessment of the prospects of this 'robust implication' approach, see Sutton (1990).

government intervenes to change the outcome of a game in favor of firms it wishes to aid. Governments can shift reaction functions by making credible promises or threats that would not be credible had they been made by firms alone. This literature has been surveyed elsewhere and need not detain us long here.⁷ What matters for our purposes is that the predicted effect of a policy shift on domestic welfare will often be reversed by a change in the assumption made about competition (Eaton and Grossman, 1986). Robust results are not on the horizon here.

1.4 Calibration Studies

In the New Trade literature, ultra-micro studies take the form of calibration exercises. The investigator picks a target industry inhabited by a small number of players. He or she then consults the theoretical tool box in search of a model that bears a tolerable resemblance to the workings of the target industry. The model is then customized, or 'calibrated' to the industry by using estimates of crucial parameters from industry data wherever possible. The remaining parameters are then filled in by benchmarking the model using data for a suitably chosen base period. Finally, the customized model is disturbed by changing the value of a policy parameter, and the effects of the change are traced out.

There is little need to go into these models in detail at this point, given the very useful survey of calibration prepared for this conference by Sheldon (1992). Sheldon's paper mixes enthusiasm for these exercises with a clear appreciation of their limitations. It is, therefore, sufficient for our purposes here to touch briefly on a few of the more significant problems with the calibration approach.

The most obvious limitation of the calibration studies is their total reliance on the sophistication of the investigator concerning the choice of model. Models are maintained, not tested. Investigators typically conduct sensitivity analyses to see whether their policy implications are robust to plausible ranges of underlying parameter values. These analyses do not, however, address the question of whether the basic specification of the model is reasonable.

To see how pervasive the problem is, consider Sheldon's classification of calibration models into those "that assume a fixed market structure, irrespective of changes in government policy" and

⁷ See, for example, Krugman (1989), p.1201ff, and Krishna and Thursby (1990).

those that "assume there is freedom of entry and exit, such that in equilibrium profits are driven to zero". The choice of models determines which of the possible rationales for trade policy can be studied: "the models with fixed firm numbers allow for a direct test of the Brander and Spencer 'rent-shifting' argument for trade policies, as firms will be making profits in the base-line equilibrium. In contrast, the free-entry models focus on the gains from policy where firms are able to more fully realize economies of scale and consumers benefit from greater variety as new firms enter into differentiated product markets." (p.115)

Now consider the two fixed structure models Sheldon summarizes. The first covers the U.S. automobile industry. Approximately 30 miles northwest of the site of this conference you will find an American Honda plant. Perhaps that plant is not located in Ohio because of government policy or the threat of policy action. Perhaps the same can be said for the Toyota, Nissan, Mazda, Mitsubishi, and Fuji Heavy Industries plants sprinkled about the Midwest. But it would certainly seem a stretch to regard market structure as fixed in the presence of such plants. Perhaps these plants confer benefits not captured by the Japanese parents on the political units in which they locate. Surely a number of midwestern governors accept the existence of such benefits as an article of faith and a stimulus for policy activity. But, while (fortunate) firms will earn quasi-rents on their sunk investments in capacity, it is difficult to see how any of this industry's participants will be able to hold onto anything approaching monopoly rents. This market may not be perfectly contestable, but it is surely strongly contested.⁸

It is also worth noting in connection with models based on a stable underlying market structure that demands for policy intervention are unlikely to appear on the policy agenda in the absence of the stimulus of some new crisis confronting the policy maker's constituency. Ordinarily this new crisis will consist either of the fact or the threat of significant expansion of foreign competition. Rent

⁸ Long-time IO scholars may recall that the Federal Trade Commission argued two decades ago that large auto companies possessed an insurmountable advantage due to the economies of scale they could exploit, so that FTC action might be required to protect customers from their unchecked exploitation of monopoly power. The auto market proved somewhat less of a fortress than the FTC imagined. It is comforting to recall that MITI, the inspiring industrial policy arm of the Japanese government, reached a similar conclusion that led it to discourage Honda from automobile production.

conservation, not rent shifting, seems more the characteristic goal of such policy exercises.⁹

If the fixed structure models seem somewhat implausible in view of the structural ferment that seems to accompany trade policy debates, can we do better by allowing competition to be first? The Baldwin and Krugman (1988) calibration of the 16K RAM market is a very clever attempt to do so. There are a number of reasons to be hopeful about such an approach. First mover advantages must exist empirically, if only because so many product introductions fail. Successes must carry with them a supra-competitive *ex post* return if *ex ante* returns to product introductions are to be competitive in the presence of pervasive failure. For the above competitive returns to persist, firms introducing successful products must have some advantages over imitators. Baldwin and Krugman postulate a learning curve advantage, a plausible though easily overstated benefit to being first. Their approach seems more reasonable than the fixed structure methods, but again one is dependent on the investigators' comprehension of the institutional details of the market in question.

One remaining problem with the calibration studies was anticipated in Section 1.2 above. In that section, we suggested that the strategic interaction of most interest will involve governments negotiating either bilateral or multilateral trade relations. The calibration studies treat governments as if they operated in isolation. The reader can judge whether this treatment is an appropriate simplification.

The chief problem with the introduction of strategic behavior into our models should, by now, be becoming clear. It is simply that there is no end to the places such behavior can pop up. Suppose that we wish to use calibration analysis as a basis for policy. We will first need to convince ourselves that we can alter parameter values without

⁹ Sheldon also discusses the Thursby and Thursby (1990) calibration of U.S. and Canadian wheat exports. The Thursby's exercise involves an intermediate step of estimating demand elasticities. The resulting estimates show both U.S. and Canadian own-price elasticities to be very small in absolute value. Given that Canadian wheat marketing, at least, is handled by a marketing board, the inelastic estimate seems implausible. Moreover, one would think that the very low elasticities faced by the U.S. (ranging from -0.099 to -0.149) would lead to the adoption of a marketing board faster than you can say "California Raisins". These odd parameter estimates are particularly troubling since the usefulness of a calibration study depends so closely on the reader's beliefs in whether the underlying benchmark model is sensible.

Thursby and Thursby note the inelastic demand in passing, attributing it to "the fact that wheat is purchased through the Japanese Food Agency". Apparently, the Japanese Food Agency is resolutely non-strategic.

provoking a response by foreign governments. We must then convince ourselves that the threat of intervention does not alter the baseline aggressiveness of foreign firms as reflected in the estimate of the coefficient of conjectural variations we employ. That is, even if we accept the exemplifying story in question as an appropriate representation of industry behavior, the structure we estimate may well not be invariant to our policy intervention.

An action designed to increase the rent share of the domestic industry is obviously more attractive the larger the post-intervention rents available for distribution. The rents available for domestic capture are a function of the intensity of competition among domestic and foreign producers. Imagine that the policy at issue is a selective unilateral tariff to be imposed to ward off a new foreign entrant to the home market. Does it pay to impose such a tariff? If competition is sufficiently aggressive, rents in excess of the quasi-rents needed to support entry in the first place will be dissipated; therefore, aggressive competition will reduce the likelihood of government intervention, if it is indeed post-entry rents that are at issue. (If pre-entry rents are the determining factor in a decision to intervene, the problem is better analyzed as one of political economy than of strategic competition.) The threat of government intervention, if anticipated by foreign rivals, thus can easily make our calibration unstable at best.

What, then, are we to make of the calibration studies? Sheldon suggests caution in employing the results, a recommendation that is obviously warranted. Perhaps calibration studies should come with a warning label such as, "Results obtained by a professional economist operating under tightly controlled conditions. Don't try this at home (or in a newspaper)." But with this caveat in mind, it is important to judge such exercises using reasonable standards. Bhagwati (1991), a harsh critic of the New Trade, argues that:

"the issue has always been not whether arguments *in theory* can be constructed for policy interventions that require departure from free trade but whether these arguments apply to the *specific empirical situations* in whose context we must consider the appropriate choice of policy" (p.110, emphasis in original).

Calibration studies are attempts to endow New Trade models with specific empirical contexts. They are essentially back-of-the-envelope calculations (albeit requiring very large envelopes!) that try to predict the effects of policy changes given a reasonably sophisticated understanding of the industry in question. Their acceptance by a

particular reader is likely to be determined more by the standards of proof that the reader adopts. If the standard is one that requires a rejection of a strongly held null that non-interventionist policy is desirable in most or all circumstances, the calibration studies will not be compelling. But their proponents could and do reply that a fair comparison would require an alternative model of the same empirical context. Such comparisons might well prove more favorable to the calibration studies.

1.5 Making Policy with the New Trade

The appeal of the "New View" of trade is easy to fathom. As Krishna and Thursby (1990) note in a recent survey, "this literature has generated a lot of interest in policy circles as it is seen as providing a rigorous justification for interventionist policies" (p.9). While noting that the leading academic figures of the "New View" are skeptical about whether it can serve as a basis for policy, Robert Kuttner (1991) argues that strategic trade analyses can at least provide intellectual cover for industrial policy advocates:

"The New View radically alters the debate, for it removes the presumption that nations like Japan, which practices [*sic*] strategic trade, cannot, by definition, be improving their welfare. Orthodox economists must now concede that advocates of industrial policy are not economic illiterates after all. And all of this invites a far more subtle debate on the instruments and purposes of departing from Ricardian trade, which is no longer necessarily optimal (p.121).

This immediate policy relevance is simultaneously a blessing and bane. The relevance ensures that the literature will receive considerable attention both from academics and from the lay press. However, the desire to be policy-relevant can lead to charges that models have been designed specifically to yield particular policy implications. Tirole has characterized an earlier attempt to use theory in industrial organization as follows:

"it was felt that in many instances theory was more a way of explaining statistical results or of buttressing particular intellectual positions than a rigorous and systematic investigation" (p.2).

This criticism is rich in irony, its target was not theory adduced in support of intervention, but rather the Chicago tradition, "famous for its very permissive view of market behavior" (p.2). The New Trade is even more susceptible (than the Chicago tradition) to fears that its theorizing is the result of intellectual opportunism.¹⁰ The Chicago School used and uses theory to attempt to explain either empirical regularities across industries or industry practices observed widely enough to suggest that they arise not from mistakes but rather from the informed profit maximizing decision of firms. The New Trade, with its custom crafted models, sorely needs a similar check.

Yet the policy relevance of the New Trade has an important role to play. Peltzman (1991, p.215) criticizes the NIO, the progenitor of New Trade, for its lack of policy relevance. Speaking of the predatory pricing literature, he remarks that, "little effort is spent in demonstrating the practical importance of the theoretical possibilities...". Peltzman is worried that the NIO rests in an ethereal "City of Theory" divorced from both the discipline of empirical testing and the need to generate clear policy implications. Yet, the New Trade has precisely the twin goals of generating empirically useful models and underpinning trade policy.

Therefore, to the extent that the New Trade succeeds, the NIO avoids at least some of the charges of irrelevance leveled against it. This is precisely because of the policy focus of the New Trade and the corresponding need of its proponents to base their work in real-world settings. And, this orientation promises to feed back to the NIO itself. While Paul Krugman was allocated a chapter in the recent *Handbook of Industrial Organization* (Schmalensee and Willig, 1989), that chapter does not seem to fit well with the remainder of the volume. Indeed, Peltzman's extensive review of the *Handbook* does not mention the Krugman contribution. Similarly, Tirole's important text on the new industrial organization explicitly omits treatment of international industrial organization issues.

These omissions are understandable if one interprets the international arena as simply a potential application for theory that is both widely accepted and firmly linked to empirical analysis, for in such instances, additional special case applications are little more than additional interesting elaborations of core results which themselves

¹⁰ Sutton (1990) points out that, "given any form of behaviour observed in the market, we are now quite likely to have on hand at least one model which 'explains' it—in the sense of deriving that form of behaviour as the outcome of individually rational decisions". But if you can get any behavior you want from such models, it is a short step to deriving desired policy outcomes.

need little feedback: But, the new industrial organization is neither empirically grounded nor fully accepted, at least if Peltzman's own testimony is to be credited. Therefore, application of the new industrial organization to international economic issues has the potential to contribute to our confidence in the body of theory being applied.

While the New Trade can be expected to provide feedback effects on the NIO, its policy relevance is less clear. How might one go about implementing a rent-shifting policy? A government that demonstrates its willingness to attempt to *shift* rents will soon find itself beset by interests that *seek* those rents. If the rent seeking activity is dissipative, the benefits of the policy are correspondingly reduced. Does rent seeking matter? If so, it can help explain why the policy implications of the New Trade have received more attention than those of the NIO. The constituencies of the policy agencies toward which the two literatures are targeted differ significantly. The NIO is aimed at the antitrust authorities, each of which has a broad, diffuse constituency. The trade agencies, however, particularly the ITC, are responsible for dealing with the pleadings of individual industries. It is surely possible that protestations that particularly policy actions would be in the general interest might on occasion be tinged with special interest pleadings.

1.6 Concluding Remarks

Much of the preceding discussion has been critical of the New Trade literature. Yet despite this criticism, the literature remains extremely seductive. And many of the criticisms leveled may well be dealt with effectively as the literature acquires a patina of age. The work is clever and done with an eye to real world settings. This concern for relevance is certainly a desirable characteristic, one that promises to enrich not only the trade literature itself but also the game theoretic industrial organization literature that lies behind it.

It is important, too, to remember the caveat that the new game theoretic approaches must be compared to alternative models, not measured against standards built from long-held beliefs. Simply noting that the empirical basis for some of the new literature is shaky does not mean that a reasonably specified alternative can automatically garner superior support. And, while the advocates of the new view sometimes seem to make a fetish of offering their models in place of existing approaches, it is apparent that the potential for complementarity exists. To take just one example, even if we suppose

that trade policy is formed in response to rent seeking activity, the rent seekers most likely to succeed are those whose rents can be shifted from sources outside the political system in question.

Finally, it is worth noting that the New Trade has only just begun to bring the richness of the NIO to bear. The New Trade is predominantly static, while many of the most interesting insights of the NIO emerge from dynamic models. Trade policy is concerned primarily with issues of adjustment, entry and exit, adoption of new technology, and the like. As dynamic models are brought to bear on trade issues we can expect a rich array of implications to emerge.

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