Chapter Title: Introduction to "Empirical Studies of Strategic Trade Policy"

Chapter Author: Paul R. Krugman

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Introduction

Paul Krugman

The papers in this volume represent the latest stage in an ongoing research project that has occupied international economists around the world for more than a decade. The project is the reconstruction of international trade theory to take account of increasing returns and imperfect competition; it is a project that has increasingly focused on the problem of quantification, of making the models operational.

In this introduction I will try to put the research reported in this volume in context. I begin with a brief review of the origins of the concept of strategic trade policy, then turn to a summary of the results of earlier quantitative work, and finally summarize some of the key points raised in this volume.

The Concept of Strategic Trade Policy

The revolution that swept through the theory of international trade in the first half of the 1980s—the rise of the so-called new trade theory[1]—left many of the insights of traditional trade theory intact. In particular, introducing imperfect competition and increasing returns into the picture does not alter the fundamental point that trade is a positive-sum game, generally carried on to countries' mutual benefit. Indeed, the new trade theory adds to the positive sum: by enlarging markets, international trade increases competition and allows greater exploitation of economies of scale, both of which represent gains over and above those due to comparative advantage.

Paul Krugman is professor of economics at the Massachusetts Institute of Technology and a research associate of the National Bureau of Economic Research.
1. The "new trade theory" began with models that used the device of monopolistic competition to show how economies of scale could be a source of trade and gains from trade, alongside conventional comparative advantage. Early papers include Lancaster (1980), Krugman (1979, 1981), Dixit and Norman (1980), Helpman (1981), and Ethier (1982); much of the positive theory is summarized in a common framework by Helpman and Krugman (1985).

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Yet the new trade theory also suggests some new reasons why government intervention in international trade might prove beneficial. Traditional theory, of course, has long offered potential justifications for deviating from free trade. Large countries, able to affect their terms of trade, can benefit if they impose optimal tariffs (and other countries do not retaliate). Even small countries have a second-best justification for tariffs on particular industries if domestic distortions lead to a divergence between private and social costs. In traditional theory, however, domestic distortions are in effect imposed on a basic model of undistorted competition; the second-best justifications for activist trade policy, however plausible, always have the feel of footnotes to an approach in which free trade is optimal in the absence of specific reasons to the contrary.

In the new trade theory, by contrast, the "distortions" are woven into the basic fabric of the trade models themselves. A theory of trade that is based at least in part on increasing returns must allow for oligopoly, external economies, or both. Either necessarily implies a failure of the usual conditions for optimality of laissez-faire: if economies of scale are internal to firms, there must be an oligopoly in which price exceeds marginal cost; if the economies of scale are somehow purely external to firms, and perfect competition is thus preserved, social marginal cost is less than private. The new trade theory suggests that in practice many traded goods are produced by industries that are both oligopolistic *and* subject to external economies (e.g., because of economies of scale in the production of nontraded intermediates). Thus instead of a picture of an international economy that is at a Pareto optimum, the new trade theory offers a picture of one in which markets normally lead to suboptimal results.

But simply saying that free trade is unlikely to be strictly optimal is not the same thing as saying that any particular deviation from laissez-faire is likely to improve matters. Can anything be said about the likely direction of the bias and the likely type of national policy that can improve on free trade?

In the early 1980s James Brander and Barbara Spencer (1983, 1985) created a considerable stir with an analysis of trade policy under imperfect competition. The Brander-Spencer analysis did three things. First, it offered a particularly clever way of setting up the case for activist trade policy, one which simplified the issue enormously and thereby revealed its core. Second, it seemed to suggest that the new trade theory provided at least limited support for a kind of neo-mercantilism, for the assertion that governments could in fact raise national income at other countries' expense by supporting national firms in international competition. Third, and not without importance, the Brander-Spencer approach could be succinctly described with a term that, while accurate, seemed to promise a larger prize than Brander and Spencer themselves ever suggested: "strategic trade policy."

What the Brander-Spencer approach actually consisted of was the following: we imagine two firms, from each of two countries, competing for some export market. Domestic consumers in this sector are ignored or assumed away,

so that the approach is inherently biased toward a view of trade as competition rather than mutual gain. The firms compete by choosing the level of some strategic variable: perhaps output, perhaps capacity, perhaps R&D.

In this kind of competitive situation, firms would like to convince each other of their aggressiveness. That is, each would like the other to believe that it will invest or produce massively, thereby inducing the other to produce or invest less, perhaps even to avoid entering the market at all. The problem is to find a way to make the threat of aggressive competition credible. The answer suggested by industrial organization theorists is that firms will make "strategic" moves—that is, take actions that do not directly raise profits, but that are intended to make aggressive behavior more credible and therefore have a deterrent effect on potential rivals. The quintessential strategic move is construction of excess capacity, which a firm does not expect to use, but which it builds in order to deter entry of potential competitors.

What Brander and Spencer pointed out was that trade policies could serve the same strategic purpose. Suppose that one of the two firms is backed by a government, which commits itself to subsidize the firm's sales. Then the other firm will know that an aggressive policy by the subsidized firm is rational and will curtail its own plans. The result can be to raise the firm's profits by much more than the actual subsidy outlay. And as a result, such a "strategic" trade policy can raise the aggressive nation's income at the other country's expense.

The Brander-Spencer analysis nicely cuts through the complexities. But it is also subject to abuse: it has enabled advocates of aggressive trade policies to give their views a new intellectual gloss. Thus the theory of strategic trade policy has been subject to an unusually detailed academic critique, the upshot of which has been to show that what Brander and Spencer offered was an example, not a general result. Eaton and Grossman (1986) showed that the case for strategic aggressiveness was sensitive to the assumed form of competition; Horstmann and Markusen (1986) showed that the benefits of strategic trade policy might be dissipated by entry of new firms and the resulting excess capacity; Dixit and Grossman (1986) showed that competition for scarce resources among industries complicates greatly the task of devising a welfare-improving policy; and Dixit and Kyle (1985) argued that strategic trade policies should be seen as part of a larger game in which it would often be better for governments to rule out their possibility.

What this academic critique showed was not that the strategic trade policy concept was wrong, but that it was not necessarily right. Or to put it more accurately, the case for strategic trade policies was not like the traditional case for free trade, which (in the old trade theory) could be made a priori without consideration of the specific details of industries. Strategic trade policies could be recommended, if at all, only on the basis of detailed quantitative knowledge of the relevant industries. So what the new trade theory gave rise to was not a prescription for policy, but a program of research.

Efforts at Quantification

It is not an easy task to arrive at quantitatively operational conclusions from models of imperfect competition. Indeed, to a considerable extent, the field of industrial organization seems to have given up on the task, in that the remarkable and fascinating body of industrial organization theory that has been developed since 1970 has an equally remarkable lack of operational content or empirical confirmation.

In international economics, however, such a state of affairs has been viewed as unacceptable by all concerned. Perhaps because free trade is such a powerful symbol, perhaps because international economists are more likely than industrial organization theorists to commute to Washington or Brussels, there was almost immediately a demand to put up or shut up: to determine when, if ever, the new arguments against free trade were relevant.

But how was this to be done? Ideally one would estimate models of imperfectly competitive industries econometrically. In practice this is extremely hard to do, because of the difficulty of identifying firm behavior. In fact, it is actually very hard to estimate models even of perfectly competitive industries; adding the potential complexities of oligopoly is beyond what anyone has managed to do.

In a seminal paper, however, Dixit (1988) offered a way to make some progress. (Harris and Cox [1984] independently developed a similar approach.) He suggested using a "calibration" technique similar to that used in computable general equilibrium (CGE) models. In this technique, parameter estimates are drawn from econometric and engineering estimates wherever possible; the number of remaining parameters of the model is then narrowed down by a priori assumptions until the model can be fully identified by requiring that it match data for some base period. Dixit used this technique to quantify a simple model of the U.S. auto industry. Once the model has been quantified, it then becomes possible to carry out policy experiments. It is also possible to carry out sensitivity analysis, to see whether the conclusions of these experiments are crucially dependent on the particular a priori assumptions made.

In general, this technique is no worse when applied to the new trade theory than in conventional constant-returns models (e.g., Whalley 1985). In imperfect competition, however, there is a special problem: the need to represent the behavior of firms. In the theoretical literature in trade (and for that matter in industrial organization) it is generally simply assumed that firms act noncooperatively, either as Bertrand price-setters or as Cournot quantity-setters. When one tries to calibrate a model, however, the data generally seem to be inconsistent with either assumption. Dixit's answer to this problem was to represent firms' behavior by the device of conjectural variations, leaving the conjectural variation parameter to be decided by the data. The problems with this method are, first, that the use of conjectural variations cannot be properly justified analytically and, second, that there is no reason to expect the conjectural variations parameter to remain stable in the face of alternative policies.

This has not stopped other authors from using the conjectural variations technique—Baldwin and Krugman (1988b), for example, is an early post-Dixit paper that complains about the approach but uses it nonetheless. In another key early paper, however, Venables and Smith (1986) proposed an alternative. They suggested that the modeler assume either Bertrand or Cournot behavior and reconcile this with the data by positing an unobserved elasticity of substitution between the products of different firms. This approach has the virtue of theoretical tightness; it has the defect that the data are not given the chance to tell us anything about the behavior of firms.

By 1985, then, an approach had been developed that allowed quantification of imperfect-competition models of trade and industrial policy. It was by no means an ideal method—most papers in this area contain some kind of disclaimer, an acknowledgment that the results should not be taken too seriously—but it at least allowed research to go beyond purely theoretical speculation. There is now a reasonably large selection of calibrated new trade models, including Dixit (1988), Baldwin and Krugman (1988a, 1988b), Smith and Venables (1988), Venables and Smith (1986), Baldwin and Flam (1989), and others.

What do these models tell us? Three main points seem to have emerged. First, the models generally suggest that the *positive* economics of trade policy—its consequences for output and trade flows—are quite different from the predictions of conventional trade theory. In particular, protection, by encouraging entry of domestic firms, often promotes exports. In some cases, as in Baldwin and Krugman (1988b), this result alone is of some importance for policy disputes.

Second, the models have for the most part supported the view that modest tariffs and/or subsidies, if imposed unilaterally, do improve on free trade. Dixit's initial model suggested that tariff rates in the low double-digit range were optimal; similar results have recurred in a number of other papers.

Third, however, the calibrated models generally suggest quite large costs to trade war and, conversely, large gains from mutual removal of trade barriers. These pro-free-trade results have actually played a significant role in two key policy debates in recent years: Harris and Cox (1984) provided some valuable ammunition to Canadian advocates of free trade with the United States, and Venables and Smith provided much of the technical background to the Cecchini Report (Emerson et al. 1989) that stated the economic case for the completion of the European internal market in 1992. But these results are all very preliminary. It is clearly necessary to refine and extend empirical work in strategic trade.

An Overview of the Volume

The papers in this volume fall into three main groups: refinement of the basic calibration technique, efforts to extend that technique to encompass in-

dustry dynamics, and renewed efforts to replace some of the ad hockery of calibration with actual testing of the implications of new trade models.

Refining the Technique

Krishna, Hogan, and Swagel offer a paper very much in the Dixit mode but suggest that an alternative formulation of demand is more satisfactory. In implementing this formulation, they discover something disheartening— namely, that even the sign of optimal policies is sensitive to the prior assumptions. They confirm, however, Dixit's basic results that the gains from intervention are in any case small, and that the gains from trade policy are negligible if appropriate domestic policies are implemented.

Venables and Smith, in their separate contributions to this volume, follow their own slightly different approach—in Venables's case by calibrating a basic model to a battery of industries, in Smith's by a more in-depth look at European autos. The results here are more heartening than in the Krishna et al. paper. Venables, in particular, notes that when the a priori assumptions about the form of competition are changed, the parameters fitted in calibration change as well; fortunately, they do so in a way that usually makes the predicted results of policies similar. So he finds that the typical result that modest tariffs or export subsidies can produce small gains is quite robust. Smith, too, finds some results that are robust to changes in the specification of the model, but other aspects of his results are quite sensitive to ad hoc assumptions.

Finally, the paper by Norman and Strandenes is on a very different subject, that of competition and entry in a regulated service industry. It shows, however, that the basic issues of modeling remain much the same as they are in ordinary trade policy.

Dynamics

Klepper addresses the effects of trade policy in an industry that is often cited as a classic case for strategic trade policy, aircraft. As he points out, however, it is a far more complex industry than is often realized, due to the combined dynamic effects of heavy initial R&D and a steep learning curve. Perhaps as interesting as his results is the evidence his paper offers on the difficulty of adequately modeling even a seemingly straightforward duopoly.

The paper by Harris represents a cleverly innovative attack on a difficult problem: in this case, the role of trade policy in an industry in transition. He models the U.S. steel industry, with its declining large-scale producers and rising minimills, and shows that the U.S. protection of that industry looks very different in the dynamic context than it would in a static model. He also shows, interestingly, that while free trade would be better than the existing protection, a tightening of the current quotas would actually raise welfare.

It has been argued by some analysts—myself included—that the case for strategic policies is substantially stronger if these policies are implemented "upstream," e.g., by subsidizing the supply of specialized inputs like skilled manpower. Ulph and Winters use a calibrated approach to study such strategic

manpower policies; they suggest that such policies may indeed be powerful, but also that issues of international mobility of high-skill workers are surprisingly critical.

Evidence

In the final section of the book, Rodrik and Winters use unusual sources of evidence to test the importance of new trade factors. Rodrik exploits one of the world economy's natural experiments. Taiwan and South Korea are very similar economies by many measures. Their industrial organization, however, is spectacularly divergent. Rodrik argues that Korea's highly concentrated industry ought to be better at assuring customers of quality and finds evidence that this is in fact the case; in so doing, he indirectly confirms the importance of industrial organization in the process of international trade.

Winters uses an even more exotic test. He argues that import surveillance—which puts foreign sellers on notice that they are being watched, but does not impose any current restriction on imports—should not matter for a competitive industry. The fact that it does in fact appear to lead to a reduction in imports is indirect evidence that the typical industry is in fact composed of imperfectly competitive firms that engage in strategic behavior.

Directions for Future Research

What can we learn from the research represented in this volume? The empirical implementation of the new trade theory has not been an easy matter. In spite of the growing body of experience with such empirical work, each new application is a painful process: after hard work learning details of a particular industry, the researcher typically constructs a model that, while capturing some crucial aspects of reality, is less than fully satisfactory. There have been no stunning empirical successes.

It is also true that the research generally provides little support for a drastic rethinking of trade policy. Nobody has yet provided empirical evidence that would suggest large gains from protection or export subsidy. This is itself a useful result, but it does not excite as much attention as would a striking pro-interventionist result.

Yet research in this area will clearly go on. For one thing, it remains important to understand as well as we can the positive effects of trade policy—and while the style of modeling represented here may not represent the final word on the effects of trade policy in imperfectly competitive industries, it is surely a much better guide than treating the market for aircraft as if it were similar to the market for wheat. The experience with the debates over regional free trade agreements in North America and Europe has already shown that even crude models of this kind can enter where conventional trade models are unable to tread. As experience is gained with dynamic models, in particular, we can expect to see them play an increasingly large role in actual policy debate.

It is also true that modeling efforts thus far have focused exclusively on one

source of potential gains from activist policy: the divergence between price and marginal cost. At the time of writing, the cutting edge of theoretical speculation has now shifted to divergences between private and social cost, with new models of the externalities that may lead to long-term growth, to divergences among national growth rates, and to industry localization. Such external economies, arising from interactions in the markets for specialized labor and intermediate inputs, cry out for quantification. Thus there will surely be future waves of empirical work on international trade in the presence of imperfect markets, and this future work will draw heavily on the style of modeling represented in this volume.

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