

Understanding International Trade Patterns: Advances of the 1990s

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Summary

A prerequisite for making informed decisions on trade policy is to have an empirically relevant theory of why countries trade. Yet, until recently, the field of international trade was almost entirely dominated by theory, with little testing of models' predictions. Moreover, the tests that had been carried out inspired little confidence in the veracity of the models, even though they continued to be the foundation for trade policy analysis. The 1990s marked a decisive shift in the analysis of trade patterns. There continued to be important theoretical contributions, notably in the work on economic geography. But more importantly, there has been a flourishing of empirical work. This work has made important progress not only in applying the models but also in identifying the assumptions that have been crucial in prior rejections of the theory. Moreover, the empirical work has moved in the direction of understanding the types of hybrid theories required to fit the data. This "new empirical trade" promises to deliver models which will provide a much more sound foundation for policy analysis.

The survey focuses on the most recent developments in empirical research motivated by evolving trade theory. It therefore provides a picture of an ongoing and still yet maturing process. Given the complexity of much of the work the description provides a guide of lines of work at the frontier of the economic profession for further reading rather than a comprehensive summary of definitive results and answers to policy questions.¹

I. INTERNATIONAL TRADE ANALYSIS INCREASINGLY EMPIRICAL

The first objective of the field of international trade is to understand why countries trade. Such a positive account of the origins of trade must precede policy analysis. The 1980s were dominated by a single major advance in thinking about trade patterns. It is sufficiently important that Krugman [1990] is justified in calling it a "quiet revolution". Imperfect competition in product markets and increasing returns to scale in production long had a toehold in the field, but little more. These topics existed at the margins of the

field, bordering on the exotic. Models based on perfectly competitive goods markets and constant returns to scale constituted the core of the field.² The “new trade theory” of the 1980s turned this upside down. Imperfect competition in product markets and increasing returns technologies were the unifying elements of this transformation. This led to a consideration of the role of oligopolies and monopolistic competition in determining trade patterns and direct investment, the investigation of “strategic” trade policies and, at the close of the decade, to a theory linking research and development, technological advantage, and trade patterns.³

In this essay, I will survey the major developments in the 1990s in the positive theory and empirics of international trade patterns.⁴ On the theoretical side, the major advance of the period is a class of models that is really a continuation of the new trade theory. This is the “new economic geography”, whose principal developers in the international field have been Paul Krugman and Anthony Venables. These models examine trade under increasing returns when costs of trade segment markets. As we will see in more detail below, these models have proven in strictly positive terms to be a more radical departure from orthodoxy than the models that launched the new trade theory revolution.

Arguably the most important development of the 1990s in understanding international trade patterns comes not from theory but from a surge of new work trying to empirically evaluate competing theories. If, prior to the 1980s, imperfect competition and increasing returns were marginal to the field, then empirics were almost not to be found. There were, of course, pioneers in empirical trade such as Wassily Leontief, Robert Baldwin, Robert Stern, Keith Maskus, and Ed Leamer. Their work has influenced strongly the agenda of researchers this decade (and most of these continue to be quite active). But empirics continued to play a limited role in the field. Indeed, Leamer and Levinsohn [1995] argued that there were only two empirical results that had fundamentally altered the way economists thought about trade: the Leontief paradox and the Grubel-Lloyd observation that a great deal of trade is intra-industry. Similarly, Krugman [1994] referred to the new trade theory as «an enormous theoretical enterprise with very little empirical confirmation.» If the objective of the field is to understand international trade patterns in the world that we actually inhabit, then this turn toward empirics holds promise of being every bit as revolutionary as the new trade theory was in its moment.

This survey will consider three principal strands in empirical international trade. The first addresses a rich recent literature on the role of factor endowments in international exchange. A series of papers have dramatically altered the landscape, incrementally identifying the manner and magnitude of failure of the simplest versions of the theory. Yet this also set the stage for a very strong confirmation of an amended version of the theory. While by the close of the 1980s, the leaders of the field were ready to conclude that factor endowments had little if anything to do with the large majority of trade -that within the OECD- a strong reversal is now in the works. While this need not mean at all that scale economies are unimportant in determining trade patterns, it is very clear from the recent work that the differences even within the OECD in endowments are substantial and that they have a systematic effect on trade patterns.

The second major empirical area covered in this survey concerns a body of work concerned with determinants of the pattern of production. Trade is itself the difference between output and absorption. This suggests that a theory of trade should have predictions about each, not merely the net. This work has taken two paths. The first has limited itself to looking at how informative factor endowments are about the pattern of production across and within countries. The second has used this as a framework to integrate comparative advantage and economic geography theories in a common framework for empirical testing. Several key messages emerge. Endowments definitely are informative

about production patterns; however there is much more to the pattern of production than just factor endowments. The hypothesis testing identifies an important role both for comparative advantage and increasing returns in determining international (and intra-national) production patterns.

The third major empirical area covered in this survey concerns the limits to international integration. Traditionally international trade economists have downplayed the role of costs of trade in inhibiting international integration. Recently «globalization» has become a central concept defining international relations. But the extent of globalization, while high by some historical measures, is really astonishingly low if one only considers the traditional costs of trade, tariffs and transport costs. Hence this new branch of the literature has sought to provide an empirical framework within which to discuss the limits to international integration, as well as to develop an analytic and empirical analysis of why integration is indeed so limited.

In sum, the work of the decade aimed at understanding international trade patterns has been extremely important. The work on economic geography represents a maturation of the new trade theory, one that will ready it for serious empirical tests. But, I believe, the work on international trade patterns of the 1990s will principally be remembered for the «new trade empirics».

II. TRADE AND THE NEW ECONOMIC GEOGRAPHY

One of the most exciting theoretical advances in the positive theory of international trade in recent years is the advent of trade models as part of the «new economic geography». On one level, and in formal terms, this new work may appear to be a trivial extension of canonical new trade theory models. The only new ingredients are the addition of costs of trade and ad-hoc adjustment dynamics. This is an illusion. These seemingly small extensions yield very rich conclusions about the nature of world trade patterns. Substantively the trade models of the new economic geography are a more radical departure from traditional approaches to trade than the new trade theory itself.⁵

There is a good reason that traditional trade models largely ignored costs of trade. It was not due to a conviction that these costs are small. Rather it is because for most questions that the traditional trade models consider, trade costs add little of analytic interest. They are sand in the gears, preventing equalization of goods prices and thus, in models where this is relevant, also of factor prices. They limit opportunities for specialization, so also reduce the volume of trade. But they do little or nothing to alter the underlying determinants of the pattern of trade.

Trade costs work quite distinctly in the world of the new economic geography. We can take this in steps. Trade costs segment markets, even if not absolutely. From the viewpoint of consumers of differentiated goods, this means that large markets will have a lower price index. This is important directly for welfare calculations. For our interest, the important point is that insofar as labor is mobile, this price index effect leads labor to want to concentrate in large markets, a tendency that may be limited either by direct limits on mobility, congestion effects in the large market, or rising marginal productivity in competitive sectors in the region from which labor is departing. Thus the endowment mix of regions itself becomes endogenous, with an important role for market size. It should also be apparent from this discussion that there is a possibility of circular causation and multiple equilibria (of which more shortly).

From the producer's perspective, trade costs introduce two new concerns. The first is that producers want to be close to consumers of their product. This is a so-called demand or backward linkage. What is striking in the literature of the new economic

geography is the strength of this linkage. Krugman shows in a *ceteris paribus* experiment that unusually strong demand for a good in a particular market leads production of that good to move more than proportionally towards that market (Krugman [1980]).⁶ As a consequence, and in strong contrast to traditional models of comparative advantage, unusually strong demand for a good subject to scale economies leads a country to export that good. Krugman termed this the «home market effect.» Subsequently, empirical researchers such as Davis and Weinstein [1996, 1998, 1999] have used this phenomenon as a critical test of which industries are «comparative advantage» versus «economic geography» sectors. This will be discussed at greater length below.

The second new concern for producers is the cost or forward linkage. The key idea is that if you produce in a large market, you have access to a large variety of differentiated inputs without having to incur trade costs. In reduced form, this appears almost as a Ricardian technical advantage in production. This forward linkage, and particularly the role of market size in determining productivity, does not appear in traditional models because size does not directly present a constraint about the variety of goods produced given assumptions of constant returns to scale.

Among the most powerful ideas to emerge from the new economic geography literature is one due to Venables [1996] which focuses on vertical linkages. He writes down a model that simultaneously features forward and backward linkages. Backward linkages lead producers to locate where demand is high and forward linkages lead them to locate where they have excellent access to differentiated inputs. But because they are themselves both producers and consumers of the differentiated inputs, they also constitute the demand providing backward linkages for others, and provide the inputs that constitute forward linkages for others. Again, there is a circularity of producers wanting to be near one another that can give rise to a great deal of agglomeration of manufacturing activity. Functionally, the mobility of demand for manufactures via demand for inputs serves as a substitute in the international models for the role played by factor mobility in the closed economy models.

The consequences of these insights are explored in Krugman and Venables [1995]. They develop a minimalist model with a homogeneous goods sector, a differentiated goods sector featuring both forward and backward linkages, two countries and a single factor. The parametric variation they explore is what happens as trade costs fall. At very high trade costs, there is a single symmetric autarkic equilibrium. At somewhat lower trade costs, there emerges a possibility or even necessity for equilibrium that a large share of manufacturing will locate in one country, leaving the other underdeveloped. This is true in spite of the fact that the countries are structurally identical. This has important implications both for trade patterns and for welfare, with the net exporter of manufactures being the country with the higher per capita income. As trade costs continue to fall, however, the advantages of location in the large country dissipate, leading to deterioration in wages there as well. Globalization first leads to a process of strong differentiation, then convergence in both production patterns and welfare.

Davis [1998] raises questions, both analytic and empirical, about the role of market size in affecting industrial structure. This is an important question for countries that contemplate integration with partners of different sizes. Davis starts with a model from Krugman [1980], with one homogeneous and one differentiated goods sector and two countries that differ only in size. The twist is that he also allows for homogeneous goods to have trade costs. On the analytic side, he demonstrates that for the case in which trade costs are equal between the two types of goods, the home market effect disappears. On the empirical side, he shows that the best available evidence suggests that, if anything, the trade costs for homogeneous goods tend to be even higher than for differentiated goods,

again tending to undermine the conditions that would lead to home market effects. In practical terms, this suggests that small countries need not fear that integration with larger neighbors will lead to deindustrialization. Krugman and Venables [1997] offer several useful observations on these results. The first is that the home market effect may arise again if the constant returns goods are themselves differentiated by location. The second is a caution on the interpretation of Davis's empirical results, which refer to average trade costs for the two types of goods. If some homogeneous goods trade at very low costs, then this provides a margin for home market effects to arise again.⁷

A collection of very interesting papers on the new economic geography appears as a symposium in the February 1999 issue of the *European Economic Review*. I will briefly note some of the key papers there. Baldwin [1999] develops a model with endogenous capital accumulation that, like Venables [1996] allows for agglomeration of activity without requiring factor mobility. It has the added advantage of a simpler structure and extends the boundaries of the new economic geography to consider investment, growth, and unemployment. Martin and Ottaviano [1999] take a step toward integrating the new economic geography and the R&D-based literature on growth. Markusen and Venables [1999] extend the analysis to think about the impact of FDI on local markets. They note that traditional analysis suggests that competition in product and factor markets should lower profits of existing national firms. However, the strengthening of forward linkages through expansion of the market could in their analysis actually lead to the creation of new centers of activity that before could not gain a foothold.

III. THE FACTOR ENDOWMENTS THEORY FINDS EMPIRICAL SUPPORT

One of the most important developments in the positive analysis of trade patterns in the last decade concerns the Heckscher-Ohlin-Vanek (HOV) model of trade. The paper by Bowen, Leamer and Sveikauskas [1987] (henceforth BLS) played an extremely important role in the debates over determinants of trade patterns. In intellectual terms, it bridges three periods. In one respect, it represents the highest form of an older approach to trade in which it was assumed that endowment differences were the principal driving force of trade patterns and that an assumption of factor price equalization would not so distort the world as to make the model useless. The paper's most striking result is that measured endowments provide no more information than a coin flip about which factor services a country will import or export. Perhaps oddly, given its analytic framework, the paper also fit in very neatly with the broader themes of the new trade theory because the seemingly devastating rejection of the Heckscher-Ohlin-Vanek theory left a yawning gap in accounting for trade patterns that was naturally staked out by the new trade theory. However, the paper also contained a first effort at what became the dominant thrust of work in the last decade, namely to find parsimonious modifications of the theory that allow it to work. In spite of their best efforts, they were unable to identify a model that performed better.

The next real landmarks in this literature are due to Trefler [1993; 1995]. Trefler asked two key questions. The first follows up directly on the work of BLS: are there simple amendments in the spirit of HOV that allow the theory to work? The second is more novel (at least within empirical trade): are the failures systematic? The latter, in particular, proved to be an extraordinarily fruitful question. And the answers Trefler provides are striking. The most memorable regularity he identifies in the data is what he terms the «mystery of the missing trade». In simple terms, the measured factor content of trade is an order of magnitude smaller than that predicted based on national incomes and endowments. This characterization of the data has been extraordinarily useful in focusing subsequent research on the types of amendments that might be needed to fit the pieces of the puzzle together.⁸

It seemed clear in the wake of BLS that the pure factor price equalization version of HOV would be a dismal failure if applied to a broad cross section of countries. This left two paths open.⁹ One approach to this is to look for ways to sidestep the problem while continuing to work broadly within the HOV framework. This is pursued in Davis, Weinstein, Bradford and Shimpo [1997] (henceforth DWBS). The starting point for that paper is to ask what HOV predicts if only a subset of the world shares FPE - an FPE club. This has a definite answer and provides the basis for tests provided a suitable FPE club can be identified. Importantly, the focus on general equilibrium prohibits discarding information on the rest of the world (ROW). However the ROW must be incorporated appropriately.

We chose the regions of Japan as our FPE club. This has a number of advantages, including the high quality and comparability of the data, and the heightened plausibility of FPE for regions of a single country. A second important characteristic of DWBS is that while prior work focused solely on the factor content of trade, we were able to examine separately the HOV theories of absorption and production. This allowed us to see directly where the failures in predicting factor contents might arise, rather than needing to rely on indirect inferences.

The DWBS paper replicates the failures of the theory identified in prior work for the case in which it assumes that the whole world shares FPE. The mystery of the missing trade is then very evident. However, it also shows that when you drop the assumption of universal FPE, restricting this to the FPE club of Japanese regions, the results improve dramatically. The regions export the services of their abundant factors, and they do so in approximately the right magnitude. The mystery of the missing trade is in large measure eliminated for the regions of Japan. Both the production and consumption theory of HOV fare reasonably well in the Japanese data. This provides a first case of HOV working while considering the problem within a full world general equilibrium.

The problem of getting HOV to work while directly confronting the failure of FPE internationally is addressed in Davis and Weinstein [1998]. Prior work on an international sample had focused on two key reasons for the failure of HOV: (1) Countries use different techniques of production, possible reasons being efficiency differences or a breakdown of relative FPE; and (2) the absorption theory based on identical and homothetic preferences may be at fault. Our starting point was to note that while the key hypotheses for the failure of HOV concerned technology and absorption, the prior work employed only a single observation on technology (that of the US) and no data on absorption. An obvious strategy was to assemble more data to explore the nature of these failures directly, which should help in selecting which among the competing hypotheses really matters in trying to get an amended HOV to work.

For details of implementation, consult Davis and Weinstein [1998]; we focus here just on the conclusions. In line with the literature on cross-country productivity (e.g. see Jorgenson and Kuroda [1990]), efficiency differences matter. The failure of factor price equalization matters, even within the OECD: more capital abundant countries use more capital intensive techniques within each industry. Non-traded goods play an unexpectedly important role, both in allowing us to make inferences about the failure of FPE, and also by the fact that when FPE fails they tend to absorb a great deal of the «excess» factor supplies that otherwise might have been available for factor service exports. Finally, trade costs matter, by reducing the opportunities to arbitrage the factor price differences.

Having directly estimated the nature of efficiency differences, the failure of FPE and its implications for production techniques, and the role of trade costs in reducing trade flows, how well does the model predict net factor trade? In considering the answer, it is well to keep in mind that due to the «mystery of the missing trade», the answer in the prior literature is that the model correctly predicts almost nothing. Here, having taken

advantage of the new and richer data set, measured factor trade is approximately 60 to 80 percent of predicted factor trade. The mystery of the missing trade is, in large measure, resolved. Countries export their abundant factors and they do so in approximately the right magnitude. Suitably amended, HOV works.

It is surely important that we are now able to provide a version of the factor endowments theory that works on international data. It is surprising how few modifications of the original theory were required to attain this position. However, the world that it depicts, one with large technical differences, a failure of FPE even in adjusted form, and in which economic integration is far from perfect, is also a world which is harder to model and harder to make simple and definite statements either about comparative statics or welfare from growing integration. One of the major professional challenges of the coming years will be to determine how this new approach to viewing trade relations will be mapped into the comparative static and policy questions of interest.

IV. UNDERSTANDING THE PATTERN OF PRODUCTION AND TRADE

ESTIMATING MODELS OF COMPARATIVE ADVANTAGE

Trade is the difference between output and absorption. Hence a theory of trade must posit a theory of the location of production and a theory of the pattern of absorption, plus possibly their interaction. Leamer [1984] pioneered an endowments-based approach to explaining net trade patterns. While he met with a good deal of success, the underlying theory posits that the structure of endowments has nothing to say about the pattern of consumption once the level of income is accounted for, hence that all of the variation is coming from the production side. While there are reasons to question whether this is a very precise view of actual absorption patterns, nonetheless it suggests the advantage in the first instance of trying to consider separately the determinants of the pattern of production.

Among the most important contributions to this line of work are two recent papers by Harrigan [1995, 1997]. In the former, he uses both cross sectional and panel estimation to investigate the links between cross-country differences in endowments and production structure for a sample of OECD countries. In the second paper, he allows not only for Heckscher-Ohlin influences on the production pattern, but also Ricardian differences at the industry level.

The message that emerges from these papers is decidedly mixed for the neoclassical model of production. Endowments clearly matter. They are always jointly significant and frequently are individually significant. Capital and unskilled labor are typically sources of comparative advantage for manufacturing industries, while skilled labor is only rarely so. When technology differences are allowed for, as in the latter paper, these clearly matter for sectoral level output. The bad news is that forecast errors are large. While the neoclassical determinants of the location of production, here captured by endowments and technology levels, do matter, they are also clearly not the only things that matter.

This clearly leaves a huge question open: Why is the model doing such a poor job explaining the cross-country pattern of production? There are potentially a large number of answers to this question. One explored in Harrigan [1997] seeks to determine whether industry-level scale economies paired with identical technologies can improve on a model of constant returns to scale with Ricardian differences. Before reporting results, it is important to note that the industry-level scale economies do not correspond to the product-level scale economies underlying the simple Krugman [1979] type of monopolistic competition. The latter will exhibit constant returns at the level of the industry even if individual products

exhibit scale economies. Harrigan's findings favor the model with constant returns to scale and technical differences across countries over one with increasing returns at the industry level and common technologies. At least as a first pass, this would indicate that adding industry-level scale economies is not likely to significantly change the ability of the models to predict the location of production.

Bernstein and Weinstein [1998] pursue an alternative research strategy. Their starting point is a result from Davis et al. [1997] that the Heckscher-Ohlin-Vanek model works well when the FPE set is restricted to be regions of Japan, but not when it encompasses the whole world. Here they examine how well endowments predict the location of production for regions of Japan versus for the larger set of countries. The surprise is that the Leamer [1984] type regressions have much larger proportional forecast errors for production for regions of Japan than for countries of the world in spite of the fact that a common technology is much more plausible for regions of Japan than for the broader sample of countries. What interpretation do Bernstein and Weinstein place on these surprising results? First, they note that the fit of the model for both samples is significantly better for non-traded sectors than tradables. The model that they suggest to accommodate these facts is one in which there are a large number of goods and a small number of productive factors. We know from general equilibrium theory that in a frictionless world, this can lead to indeterminacy in the location of production. When there are high costs of trade -either because the goods are non-traded or because we are speaking of international rather than interregional trade- then the degree of indeterminacy will be reduced. In the case at hand, the high trade costs between countries help to make the production patterns determinate and so help in predicting output levels to a sufficient extent that this overcomes the problem that international technologies are quite unlikely to be common.

COMPARATIVE ADVANTAGE VERSUS ECONOMIC GEOGRAPHY

There are two principal theories of why countries trade: comparative advantage and increasing returns. As the increasing returns theory began to receive a fuller treatment, as in Krugman [1979] and Lancaster [1980], a first question was whether it would be possible to integrate the two perspectives. This was done brilliantly in Helpman [1981], Krugman [1980] and Helpman and Krugman [1985]. In particular, the last of these showed that the basic insights about trade patterns were robust to a wide variety of imperfectly competitive market structures, including monopolistic competition, free entry oligopoly, and contestable markets.

However, the robustness of this relation across varying market structures itself became a point of contest, since it suggested that the specific trade patterns of interest arose from a deeper phenomenon: specialization. This point was made by Davis [1995, 1997] and Deardorff [1995], who pointed out that various aspects of the trade patterns, such as a large share of intra-industry trade, a great deal of North-North trade, and a good fit of the gravity equation, could all be accounted for within traditional models of comparative advantage.

It is worth emphasizing that the fact that the traditional models can account for these facts does not imply that they are indeed the models generating the data. But it does mean that if we hope to separate the models, we need to turn to characteristics that truly distinguish the models. This is difficult with respect to the first round of positive new trade theory models, because the sole thing that scale economies do in the models is force specialization, which could arise from the other forces noted.

One approach to hypothesis testing is to depart from the simplest new trade theory models to a class within what is now termed the «new economic geography».¹⁰ A

model from Krugman [1980], while in timing near the beginning of the new trade theory, is in analytic approach the foundation of the new work on geography. The key addition to the new trade theory model embodied there is costs of trade. Krugman argued that this framework provides a clear contrast between models based on comparative advantage and those based on increasing returns. In a comparative advantage model, a country with unusually strong demand for a good leads that country to import that good. However, when there are economies of scale, there is a non-trivial location decision. Producers want to locate near unusually strong sources of demand in order to cut trade costs. It turns out that this effect is in fact a magnification of the demand deviation itself, leading countries with unusually strong demand to become exporters of a good, all else equal.

This approach to distinguishing comparative advantage from increasing returns is implemented empirically in Davis and Weinstein [1996, 1998, 1999]. The first and last consider alternative specifications based on OECD data, while the second considers the same issue based on Japanese regional data. The estimating equation augments the Heckscher-Ohlin theory of the location of output with a variable reflecting idiosyncratic components of demand. If the industry in question is a comparative advantage industry, then the response of production to idiosyncratic demand will be less than one to one. By contrast, if it is an economic geography sector, locational preference leads producers to respond more than one for one to idiosyncratic demand. The results indicate that both comparative advantage and increasing returns play a role in determining production location. While the results are not strictly comparable, due to different divisions of output within the OECD and Japanese regional data, there is some evidence that both data sets are identifying similar types of industries as those where increasing returns matter.

THE GRAVITY MODEL AND THE DETERMINANTS OF TRADE

The gravity model of trade holds that the volume of bilateral trade is proportional to the product of two countries' incomes and inversely proportional to the distance between them. This empirical model was long described as a theoretical orphan. Hence an important selling point of the new trade theory models of monopolistic competition is that they provide an extremely simple rationalization of this empirical relation. Helpman [1984] provided a pioneering theoretical and empirical implementation that he interpreted as broadly supportive of the monopolistic competition framework.

In the last decade there has been an ongoing exploration of the theoretical boundaries between the comparative advantage versus scale economies models. This discussion has featured papers by Davis [1995, 1997], Deardorff [1995], and Trefler [1998]. The paper by Deardorff, in particular, points out that the gravity model may arise from a wide variety of analytic models that give rise to a high degree of specialization. Feenstra, Markusen and Rose [1998] go a step beyond this, showing that the gravity model can arise even in a reciprocal dumping homogeneous goods model of oligopoly. In the space of little more than a decade, the gravity model went from theoretical orphan to having several competing claims to maternity.

Given the competing claims over which model actually accounts for the gravity relation, a first question is whether the empirical model can be used to provide information about the sources of trade. Hummels and Levinsohn [1995] provide a first approach. Their reasoning was that if scale economies and product differentiation associated with high end products was the basis for the gravity relation, then the gravity equation should work well for a set of rich countries for which the *ex ante* plausibility of this market structure is greater, rather than for an alternative set of relatively poorer countries. Broadly, their finding was that the gravity model worked quite well for both sets of countries, tending to undercut

the claims that the excellent fit of gravity equations constituted empirical support for the monopolistic competition model.

Feenstra et al. [1998] exploit their theoretical results to develop a highly novel empirical strategy. They note that while various models can give rise to a gravity-type equation, not all implications of the models are the same. In particular, they focus on the contrast between the relation between GDP and exports in gravity models based on monopolistic competition versus reciprocal dumping. Using the empirical division of Rauch [1999] to classify goods as differentiated or homogeneous, the former is shown, both analytically and empirically, to have a higher income elasticity of exports, giving rise to a home market effect in the differentiated goods and a reverse home market effect in the homogeneous goods.¹¹ Their results suggest that indeed the success of the gravity equation requires a composite theory appropriate to the respective types of goods.

V. HOW WIDE IS THE BORDER?

The starting point of the theory of international trade is that borders matter. Traditionally, though, the emphasis has been on the fact that borders matter more for movement of factors than of goods. Indeed, a long tradition in international trade has abstracted entirely from the cost of trading goods across borders, even as it treats movement of factors as prohibitively costly.¹² We know as a matter of history that there have been important episodes of tremendous factor movements (Hatton and Williamson [1998]). Yet, until recently, comparatively little attention has been paid in the analytic and empirical literature to the nature and magnitude of barriers to trade (O'Rourke and Williamson [1999]).

These positive questions about the role of barriers to trade stand at the very heart of numerous empirical, analytic and policy issues of the first order of importance. In a period in which «globalization» has become a buzzword, it is important to be clear about how integrated international markets are or are likely to become. In a period in which regional trade agreements are sometimes viewed as the merging of economies, it is useful to look at how deeply integrated seemingly close sovereign countries -or even national economies- have become. The degree of international integration or segmentation of markets is also important for thinking about myriad policy issues, such as the need or use of global competition policies or the implications of exchange rates for incomes and employment. It is likewise important for thinking seriously about the type of locational issues that lie at the heart of the recent work on economic geography.

If one is to say how much borders matter for the volume of trade, then one requires a methodology to establish the counterfactual of how much trade would take place in the absence of a border, which is then compared to measured trade flows. McCallum [1995] makes an important contribution. The starting point is the so-called gravity model of trade, a simple version of which holds that trade volume between locations will be proportional to the product of the incomes and inversely proportional to the intervening distance. McCallum's insight was to apply this model to shipments from Canadian provinces to other provinces within Canada as well as to states across the border in the United States. In order to appreciate the results, it is important to recognize that the overt border barriers between the United States and Canada were in most cases very low even before the formation of the Canada-US Free Trade Agreement, typical tariffs being just a few percent.

McCallum's findings are very striking. Canadian provinces trade with one another more than twenty times as intensely as with US states of a similar size and distance. Allowing for the actual differences in relative size and distance, provincial trade with US states should have been ten times as large as inter-provincial trade. However, actual trade among Canadian

provinces is in fact equal in magnitude to their trade with US states. This provides very striking evidence that national borders matter for the volume of trade.

Wei [1996] extends McCallum's approach to consider the extent to which countries trade «too much» within a country rather than between countries. He similarly uses a gravity framework as the core specification. His innovation is to provide a baseline for the internal distance within a country -i.e. in gravity terms how far the country is from itself. Once this is done, the absorption of a country of its own goods can be treated as simply one additional data point, with a dummy picking up the incremental trade that arises for trade with oneself. Also, following Deardorff [1998], Wei introduces a variable intended to correct for the «remoteness» of a country on the premise that my trade with another country will be greater, holding fixed incomes and distance, when I am remote from alternative trading partners.

Helliwell [1998] has done more than perhaps anyone else to probe the robustness of these results. He estimates border effects for OECD countries to be approximately a factor of ten, although less for countries within a trade bloc. Estimates that include developing countries found effects of up to a factor of one hundred. Helliwell raises two important cautions in interpreting these results. The first is that the effects are sensitive to the measure of internal distance that we use, which suggests the value of additional research that makes these measures more theoretically and empirically precise. Second, some of the assumptions about similarity of preferences underlying the gravity regressions may be particularly inappropriate when applied to developing countries, hence may tend to exaggerate the magnitude of the border effects. Helliwell also presents evidence that regional integration, both in North America and in Europe, may have significantly reduced the magnitude of the border effect.

The studies of McCallum, Helliwell, and Wei document the existence of border effects. However, there are two key and related questions remaining. Why do these border effects arise and what welfare cost is involved? One can think about these questions from a variety of perspectives. One approach is to think about this strictly in the framework of the gravity approach within which the estimates were derived. Then one realizes that the missing trade volumes actually reflect two factors, the magnitude of the barriers and the elasticity of substitution between imported and local production. The low international trade volumes could reflect high barriers at the border, in which case they are more likely also to have large welfare implications. Or they may simply reflect the interaction between relatively low barriers with high elasticities of substitution. Even if barriers are high, this may reflect policies whose removal would improve efficiency or real transactions costs perhaps associated with differences in patterns of business networks, shared norms, legal system, etc. Hence even if border barriers are high, one needs to be more precise about their nature to make assessments of the likely welfare impact.

A very interesting approach to this question is pursued by Rauch [1999] and is further discussed in Helliwell [1998]. This approach emphasizes the role that networks of business contacts may play in facilitating the flow of information about products vital to engaging in commerce. Rauch starts by dividing goods into three groups: those traded on an organized exchange; those not so traded, but for which a reference price exists in industry journals; and pure differentiated goods for which no reference price exists. These groups thus span the range from pure homogeneous to pure differentiated goods. The premise of Rauch's approach, then, is that business contacts, here proxied by proximity, and common language or colonial ties, will matter most for the more differentiated goods. Helliwell posits this as an important potential account for the existence of the border effects. Given the *ex ante* plausibility of Rauch's hypothesis, what is striking in the paper is how hard it is to document that this matters quantitatively.

Evans [1999] follows up on the question of the role that product differentiation and the associated need for solid business contacts may play in the existence of the border effect. Her approach is to focus on industry characteristics indicative of product differentiation. These include the share of intra-industry trade, an index derived from Rauch [1999], R&D as a share of sales, and advertising as a share of sales. Very strikingly, the estimates consistently indicate that product differentiation tends to *reduce* the magnitude of the border effect. Similar results are obtained when other indicators of the cost of information are used, such as whether products are made to order, the frequency of service calls, and whether or not it is a major or infrequent purchase. The data consistently point to the border effects being strongest within homogeneous goods categories. She also asks whether observed barriers, combined with reasonable choices of elasticity of substitution between domestic and foreign goods is likely to explain the magnitude of the border effect. The short answer is «no». While the barriers vary quite a bit by industry, implied barriers in the 60-100 percent range are not uncommon.

What are the barriers that lead national borders to matter so much? Do we believe that the barriers really are this high? If so, how can we identify the true costs involved? Are they amenable to amelioration via policy? Are the welfare effects large? Or is the apparent paucity of trade across borders an illusion based on an inappropriate analytic framework? These remain extremely important areas for further research. At least so far, business networks do not look like they are going to be a panacea. Hummels is doing important work documenting the level and evolution of actual shipment costs over time (Hummels [1999a, 1999b]). But it seems unlikely that this will provide a full account of border effects. A locational approach might be promising. The question for each of these approaches is not only to devise an analytic story that works, but also to think about how it might be implemented empirically in a convincing way. The answers are likely to be important for regional integration and location theory as much as for the simpler questions of what determines trade patterns.¹³

VI. CONCLUSION

The work of the 1990s seeking to understand international trade patterns does not have a single grand unifying theme, as did that in the prior decade. On the theoretical side, the most important development is an elaboration of themes that first arose directly within the new trade theory. The most hopeful development of this decade is a sustained effort to bring theory and empirics together to identify composite models that successfully describe the principal features of international trading patterns. The decade began with some spectacular failures in matching the theory and the data. It is closing with some notable successes. There is every reason to believe that this effort to match theory and data will continue on through the next decade and help to select which among the myriad theoretical models really help us to understand world trade patterns.

Notes

¹ In addition to this survey, an important study covering the last twenty years of research of International Trade has been produced by Helpman [1998] which could be useful to understand the review done in this article.

² It is worth noting that there are important exceptions. The work of Bhagwati [1969] on the non-equivalence of tariffs and quotas under monopoly is a case in point. Moreover, a great deal of work had been done focusing on imperfections in factor markets, as in Brecher [1974].

³ Drawing a neat boundary line between periods can never be exact. For the purposes of this survey, I consider topics treated in depth in the excellent selection of papers by Gene M. Grossman [1992] to constitute the “new trade theory” of the 1980s. Elhanan Helpman and Paul R. Krugman [1985] wrote the classic monograph on the static new trade theory. Gene M. Grossman and Elhanan Helpman [1992] provide a masterly synthesis and exposition of the work on innovation and growth as well as a more compact survey of the same material in Gene M. Grossman and Elhanan Helpman [1995].

⁴ The focus on the determinants of international trade patterns excludes three other major areas of work in the 1990s: (1) Political economy of trade policy; (2) Regionalism versus multilateralism; (3) Trade and income distribution. The first two are surveyed in Volume 3 of the *Handbook of International Economics* Gene M Grossman and Kenneth Rogoff [1995], and the latter is discussed in a symposium in the *Journal of Economic Perspectives*, Summer 1995.

⁵ Readers interested in an introduction to many of the key ideas can consult Krugman [1991]. Ottaviano and Puga [1997] provide a more systematic survey of key themes. A textbook treatment appears in *The Spatial Economy*, by Masahisa Fujita, Paul R. Krugman and Anthony J. Venables [1999].

⁶ The timing of Krugman's paper places it near the beginning of the new trade theory. However its content strongly distinguishes it from the dominant analytic perspective of the new trade theory, placing it rather as a forerunner of the new economic geography.

⁷ Davis's original article noted one additional caution in interpreting the results. If forward linkages of the type emphasized in Venables [1996] are empirically important, then home market effects may yet again arise as the reduced form Ricardian advantages of locating manufacturing in large markets overcome the inertia of costly trade in homogeneous products. This emphasizes the importance of pushing forward the agenda on empirical work regarding the new economic geography.

⁸ For a more complete discussion of Trefler's methodology and conclusions, see the survey by Helpman [1999] and the references therein.

⁹ A third strategy is pursued in DeBaere [1998] and Hakura [1997]. This relies on looking bilaterally at the *difference* between predicted net factor exports. This has some advantages, but has the major disadvantage of potentially sweeping out systematic departures of the data from the underlying theoretical prediction.

¹⁰ Here we have focused on testing of alternative theories. A valuable paper that instead seeks to estimate directly the parameters of an economic geography model is Hanson [1998].

¹¹ For an alternative strategy, see Evenett and Keller [1998].

¹² For example, Paul A. Samuelson [1949].

¹³ The extent of product market integration has also been examined from the price side, as in Pinelopi K. Goldberg and Michael M. Knetter [1997].

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