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“The Floating World: Issues in International Trade Theory”

by

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A deranged publisher decided to produce a volume of some of my papers and asked me to write some comments. Since these amount to a summary of my views about international trade theory over the latest forty years or so, I'm giving the comments a separate alternative existence as a discussion paper.

General Introduction

In this volume I've collected together some of my published papers. It's what you do when you get old. This collection is a combination of (sort of) well-known papers and more obscure ones, for which I have a fondness. A fondness often shared, I suspect, with no one else.

The papers are grouped according to major concerns and research programs about trade theory that have preoccupied me during my career. For each I supply comments, beyond those in the papers themselves, about why I thought the program important, about how the included papers contributed to it, as well as some trivia. But I also sometimes cite additional papers (not included in this volume), that were part of the respective research program, and explain how they fitted in.

I warn you that much of this will be self-indulgent. Perhaps you will bear with me.

I am a “pure” trade theorist, in the sense that I have done no formal empirical work, and the papers in this volume reflect this. But I have always been motivated strongly by real-world problems and by what I have thought could properly be inferred from good empirical work. Perhaps the papers also reflect this. The quality of empirical work in international trade has improved enormously over the course of my career (not that I’ve had anything to do with that).

There is, in some quarters, a recent attitude that any valid paper on trade should include an empirical component. For trade economists to disdain, in their own work, the principle of comparative advantage, is indeed a most delicious irony. I love it!

For over half a century the more important developments in trade theory have been driven by a tension between that theory and the apparent implications of empirical work. This has been true, above all, in two areas: **i** the (actual) challenge to the factor-endowments theory posed by the Leontief Paradox, and **ii** the (apparent) challenge to comparative-advantage theory posed by the empirical realization of the practical importance of trade in similar goods between similar (industrial) countries.

Certainly much of my own work has been thus motivated, and I have more to say about this below. This empirical literature, however, is not directly relevant to my major concerns during the more recent decade or so: trade policy and its underlying political-economy basis. But here also I have been centrally motivated by a concern about what really drives policy makers and also about what I perceive as a tendency for really-good empirical work to be interpreted really badly. But, again, more about this below.

Let me bore you a bit with something of my background. When I arrived, as a new undergraduate freshman, at the University of Rochester in 1961 I had no idea at all of what economics was about, and I also had a vague intention to become an electrical engineer. But in my dormitory I met two first-year economics graduate students also residing there: Hiroshi Atsumi and Vikas Chitre. They influenced me enormously. Atsumi, in particular, was in effect my mentor.

Also of critical importance was the state then of Rochester’s economics department. Some years earlier, Lionel McKenzie had been brought in to rejuvenate that department, which he had done spectacularly. One of his accomplishments was to identify and hire a collection of very able young economists. This collection included the trade theorist Ronald Jones. The courses I took from Jones and McKenzie were critical to my development, and the papers in this collection vividly display Jones’s influence.

Factor-Endowments Theory

The factor-endowments theory (*aka* the Heckscher-Ohlin theory) is a central component of trade theory. Indeed, until fairly recently it was also commonly referred to as the “modern” theory of international trade. But it reflects a very pronounced research philosophy.

This philosophy is to take for granted that trade is due to comparative advantage — as opposed to economies of scale or imperfect competition. And that comparative advantage is determined by differences in relative factor endowments — as opposed to differences in technology, tastes, or government policies. And its basic results are derived in the 2x2x2 Heckscher-Ohlin-Samuelson (HOS) model. The pay-off to this courageous but limiting stance is a set of very strong predictions about, in addition to the pattern of comparative advantage itself (the Heckscher-Ohlin theorem), the relation between trade and the domestic distribution of income (Stolper-Samuelson), the relation between economic growth and trade (Rybczynski), and the respective roles of international markets in goods and in factors (Factor-Price Equalization).

This is an approach just made for a confrontation with reality. That was not long in happening: the Leontief Paradox. The resulting empirical literature, after more than half a century, has yet to reach a generally accepted conclusion.

My concern, though, was not with the empirical literature itself. Though that certainly has influenced me greatly. Rather, I was anxious to address the issue of theoretical robustness. Accepting a focus on comparative-advantage trade (that is, ignoring trade due to economies of scale or to imperfect competition), I wondered whether the four basic results reflected the nature of factor-endowments based trade or simply the extreme simplicity of the HOS model.

That the answer was the latter seemed to be the conclusion that trade theory had reached by the 1970s, at least by implication. Many papers had striven to show that the four basic results did not hold literally outside the 2x2x2 HOS model, or that they could be made to hold only with additional restrictions that in effect required that a more general framework had to behave like the 2x2 HOS model. By contrast, Ron Jones has eloquently argued the practical usefulness of a low-dimensional approach.

In my paper, “Some of the Theorems of International Trade with Many Goods and Factors,” included in this volume, I tried to reformulate the four basic results for a higher-dimensional context than HOS and to ask whether they held more generally. My conclusion was that, yes, the

four basic results, properly interpreted, reflected the basic nature of factor-endowments trade, not the dimensionality of HOS. (I regard this as among my very best papers. But I've always been embarrassed by how badly written this paper is. So over the years I've tried to convince myself that the bad writing was really due to the insistence of a constipated editor: But in the end, alas, it's my own name on the paper).

Alan Deardorff, Avinash Dixit and Victor Norman had meanwhile given higher-dimensional interpretations of comparative advantage (and of the Heckscher-Ohlin theorem) as properties holding "on average," or in terms of correlations. My paper, "The General Role of Factor Intensity in the Theorems of International Trade," included in this volume, used their perspective to examine the four basic results. This furnished another avenue of support for the basic generality of those results.

These two papers were but part of my research program into the theoretical robustness of the four basic results of the HOS model. The survey [1] summarized, explained, pontificated, and further developed the argument that the basic economic core of these results is independent of the dimensionality of the HOS model.

Other papers comprising the program addressed limitations of the model other than dimensionality. The addition of a non-traded good to HOS, the subject of [2], also changes the model's dimensionality. This paper reported the results of my very first efforts in economic research. So it predated the conception of the research program.

The HOS model assumes a production function for each good that produces only that good, *i.e.*, it excludes joint production. This is relied upon by the proofs of the Stolper-Samuelson and Rybczynski theorems. In [3] and [4] Murray Kemp, Winston Chang and I asked how dependent upon this assumption the basic messages of the four results really are. Our conclusion, surprising to us, was that this dependence is actually limited. This is dramatically true when, as in [4], the rest of the panoply of the HOS structure is maintained. (Of course, you should remember that the basic purpose of this research program was to determine what would still be true without much of that panoply).

A prominent part of the trade-theory literature in the 1970s concerned time-phased production. This amounted to replacing the abstract notion of "capital" with an explicit modeling of goods being produced today but used tomorrow. This aspect of trade theory was an offshoot of the "Cambridge controversies" of the day. If you do not know what those were, good for you. This literature argued strongly that traditional trade theory in general, and HOS in particular, delivered conclusions that were deeply suspect and misleading, because of its neglect of the time-phased nature of production.

I was intrigued by this. Were these people actually on to something, or were they just using a different language for obfuscation? Answering this required a common language. So, in [5] and [6], I adopted the time-phased models, defined “capital” as the value of the goods produced today but used tomorrow, and restated the conclusions of the time-phased literature in these terms. This made it clear that that literature was simply restating well-understood results in its own language. (There were some distinct results, but they were minor and peripheral). That is, the time-phased literature really was just using a different language for obfuscation. This literature fortunately disappeared into obscurity in the 1980s. I deny any culpability.

The HOS model excludes international markets in the factors of production, although the factor-price equalization theorem addressed the consequences of that limitation. In [6] Lars Svensson and I investigated directly the consequences of that exclusion for the general validity of the HOS results. Our basic conclusion was that the HOS messages, adjusted in a natural way for the new environment, continue to hold.

The basic message from this research program was that, in contrast to the predominant attitude in the 1970s, the basic HOS results reflect fundamental properties of comparative-advantage trade due to relative factor-endowment differences, and *not* the special HOS assumptions. At least, I drew that conclusion.

Included Papers

Ethier, W. J. “Some of the Theorems of International Trade with Many Goods and Factors,”

JOURNAL OF INTERNATIONAL ECONOMICS 4 (2), May 1974, pp 199-206.

Ethier, W. J. “The General Role of Factor Intensity in the Theorems of International Trade,”

ECONOMIC LETTERS 10, December 1982, pp 337-42.

The Other Papers

[1] Ethier, W. J. (1984), “Higher Dimensional Issues in Trade Theory.” In: P. B. Kenen and R. W. Jones (eds.), **HANDBOOK OF INTERNATIONAL ECONOMICS** (Amsterdam: North Holland) pp 131-84.

[2] Ethier, W. J. (1972), “Nontraded Goods and the Heckscher-Ohlin Model,” **INTERNATIONAL ECONOMIC REVIEW** 13 (1), pp 132-47.

- [3] Ethier, W. J. and M. C. Kemp (1976), "A Note on Joint Production and the Theory of International Trade." In: M. C. Kemp, **THREE TOPICS IN THE THEORY OF INTERNATIONAL TRADE** (Amsterdam: North Holland) pp 81-4.
- [4] Chang, W. W., W. J. Ethier and M. C. Kemp (1980), "The Theorems of International Trade with Joint Production," **JOURNAL OF INTERNATIONAL ECONOMICS** 10 (3), pp 377-94.
- [5] Ethier, W. J. (1979), "The Theorems of International Trade in Time-Phased Economies," **JOURNAL OF INTERNATIONAL ECONOMICS** 9 (2), pp 225-38.
- [6] Ethier, W. J. "A Reply to Professors Metcalfe and Steedman (1981)," **JOURNAL OF INTERNATIONAL ECONOMICS** 11 (2), pp 273-7.
- [7] Ethier, W. J. and L. E. O. Svensson (1986), "The Theorems of International Trade with Factor Mobility," **JOURNAL OF INTERNATIONAL ECONOMICS** 20 (1/2), pp 21-42.

Economies of Scale

As mentioned in the **General Introduction**, much work in the 1970s and 1980s was motivated by a dawning realization that a dominant share of world trade involved the exchange of relatively similar, manufactured goods (intra-industry trade) between relatively similar (industrial) countries.

This realization was significantly due to the work of Grubel and Lloyd. (Though there had been important earlier contributions, notably Balassa's empirical work and the unjustly ignored contribution by Gray). Herb Grubel was sometimes dismissed as a lightweight, but the fact is that, during the 1960s and 1970s, he was consistently ahead of the curve on the important issues. And I confess to a soft spot for him: Herb was the first established economist to say that he thought I had done something useful and interesting.

Comparative-advantage trade, of course, is trade to exploit international *differences*. So there was a wide belief that this actual trade could not be comparative-advantage trade. It was this wide belief that stimulated a huge amount of work into economies of scale and imperfect competition as alternative reasons for trade.

I recall Herb mentioning that Harry Johnson had told him that intra-industry trade was no big deal: just a matter of getting into the comparative-advantage details. For those too young to know, Johnson was a dominant influence in trade (and macroeconomics and development economics) from the 1950s through the 1970s. This dominance was based largely on personality: Johnson was a bully. Of course such dominance also required a supporting intellect, certainly not lacking in Johnson's case.

Although Johnson was wrong about much, I think he was basically right here. But he died in 1977, and the literature on scale economies and imperfect competition then proceeded at a heady pace. (If your influence depends upon intimidation, your influence disappears when you do).

To put it succinctly, the comparative-advantage assertion that: "trade is due to international differences" simply does *not* imply "the greater the differences, the greater the trade." Countries are similar, in the relevant economic sense, if, in the absence of trade, they would have autarky relative prices that do not differ greatly. This means, in a comparative-advantage world, that trade cannot cause large changes in relative prices within countries. But if the similar countries are trading similar goods, price elasticities should be quite high. So modest price changes can indeed be expected to produce large trade volumes.

Consider this in the context of the HOS model addressed in the previous section of this book. If the home country and the rest of the world have only modest differences in factor endowments, the factor content of trade will indeed be small. But if they trade similar goods, *i.e.*, goods whose relative factor intensities do not differ greatly, a large volume of commodity trade will be required to effect even a modest volume of factor-content trade. I recall John Chipman arguing years ago (at a conference I do not otherwise recall) that a large volume of trade in similar goods between similar countries was quite consistent with comparative advantage. At the time I berated him for saying what I thought was obvious. But he was right and I was wrong: Most of the profession did indeed have the quite different, erroneous, view that John was critical of.

So the "new" trade theory's emphasis on scale economies and imperfect competition was based on false pretenses. Like the US invasion of Iraq to deal with weapons of mass destruction. But that does not imply that it was not constructive ("new trade theory," I mean, not the invasion). It stimulated much interesting work. Indeed, it was very helpful to me. I was at the time much interested in scale economies, and so the desire for a "new trade theory" helped give me an audience.

There was, at the time, a small but old literature on trade and increasing returns. The main concern of this literature was how increasing returns could alter established, comparative-advantage, results. The concern of my research project was the idea that, in a globalized economy

(but we were then not yet saying “globalization”), Adam Smith’s classic notion of the division of labor constrained by the size of the market amounted to scale economies determined by the size of the world market.

My first, preliminary, step, in “Decreasing Costs in International Trade and Frank Graham's Argument for Protection,” was to reformulate the existing theory in a way more useful for my purpose. Though this was the first of the three papers in this research program to be written, it was the last to be published, because of contemporary chaos at *Econometrica*.

The second paper, “Internationally Decreasing Costs and World Trade,” tried to set out my basic views on the subject. I have always regarded this paper both as one of my very best papers and as the center-piece of this project that contained its basic contribution. The other two papers, however, have each received much more attention.

This paper was published in the *Journal of International Economics* at the start of 1979. Other central contributions to intra-industry trade by Paul Krugman, Kelvin Lancaster, and Elhanan Helpman, were also submitted there in due course. As my paper had been the first to arrive, the editor, Jagdish Bhagwati, sent me each of the other papers to referee as it was submitted.

In each case I submitted a report and enthusiastically recommended acceptance. For example, my letter of 21 November 1978 to Jagdish Bhagwati concerning Krugman’s paper stated that the paper, “is skillfully and elegantly executed, and yields interesting and original insights about trade. ... [my] suggestions do not address the substance of the paper, and, even if Krugman should be completely recalcitrant, you should not hesitate to publish the paper.” I was proud to have assisted in the publication of these excellent papers.

Years later *The Journal of Economic Perspectives* published an article by Joshua S. Gans and George B. Shepherd that quoted Jagdish Bhagwati as saying, about Krugman’s paper, “I published it myself *despite* two adverse referee reports by very distinguished experts on the theory of increasing returns! It did take some courage and also a strong sense of the importance of the paper for me to do so.” So much for my pride about being helpful.

“National and International Returns to Scale in the Modern Theory of International Trade,” the third paper, exploited the second paper, linking it with the literature on trade and monopolistic competition as well as with the factor-endowments theory. A useful exercise, I thought, but derivative of the earlier papers. Still, it has probably been the most cited of my papers.

Included Papers

Ethier, W. J. "Decreasing Costs in International Trade and Frank Graham's Argument for Protection," **ECONOMETRICA** 50 (5), September 1982, pp 1243-68.

Ethier, W. J. "Internationally Decreasing Costs and World Trade," **JOURNAL OF INTERNATIONAL ECONOMICS** 9 (1), February 1979, pp 1-24.

Ethier, W. J. "National and International Returns to Scale in the Modern Theory of International Trade," **AMERICAN ECONOMIC REVIEW** 72 (3), June 1982, pp 389-405.

International Factor Markets

My interest in international factor transactions involved both factor trade in general and the mobility of particular factors. Regarding the latter, I have been concerned, to some degree, with both capital and labor. But not land.

Regarding factor movements in general, my work with Lars Svensson [2] established the general relevance of the HOS results in a context where goods trade and factor trade were clearly substitutes. In a key paper, Jim Markusen established that, if trade were not driven by factor-endowment differences, as in the HOS world, goods trade and factor trade would much more likely be complementary.

This intrigued me. It seemed obvious that in virtually all public debate where the relation between goods trade and factor trade mattered (*e.g.*, debates about immigration policy, trade agreements, *etc.*), all sides to the debate accepted without question that the two were substitutes, not complements. But I found Markusen's arguments persuasive. My explanation, in [3], was that the relation between goods trade and factor trade really matters for the welfare of most people only when there are dramatic differences in factor endowments. And this is just when the two are most likely to be substitutes.

My interest in labor migration was sparked by the realization that very much of it was temporary, at least in original intent, as opposed to the "across the wide ocean" story of

permanent mass movements. So my attention was naturally drawn to the likely expected contractual (or non-contractual) relation between migrant and employer. I addressed this in “International Trade and Labor Migration,” which I think of as my basic contribution to labor migration.

Of course, I could not resist the usual temptation to spin off. “Illegal Immigration: The Host Country Problem” looked at the issues faced by host countries that declare much migration illegal and then try to control it. In [1] I tried to relate labor migration to the traditional theory of factor movements.

My interest in international capital movements centered on direct investment. The dominant theoretical paradigm of direct investment views it as the result of three considerations. **i** *Ownership* (the investing firm has some attribute it can exploit abroad); **ii** *Locational* (it pays to exploit the attribute by producing abroad rather than by exporting); **iii** *Internalization* (there is a reason to have control over the foreign production rather than completely delegating it to another firm). There was a well-developed theoretical literature, with empirical support, based on **i** and **ii**. Much less had been done regarding internalization aspects. This was my concern in “The Multinational Firm,” my basic contribution to the literature on direct investment, and another paper that I regard as among my best.

My basic approach was that contracts between firms are necessarily incomplete, in an uncertain world, and that internalization basically amounted to retaining *ex post* control. Thus internalization concerns should mandate direct investment, rather than arms-length contracts, where such control was likely to be important. In the industrial-organization literature, Grossman and Hart were at the same time grappling with similar concerns. But I was unaware of their efforts, and so did not profit from them. Too bad: Could have saved me some time.

A basic result was that *ex post* control was likely to be most important when source and host countries were similar. In that case unexpected shocks were most likely to dominate the division of activities between the two countries rendering *ex post* control crucial. If the countries are very different, the division of activities between them is much less likely to be subject to unexpected shocks. This offered an explanation of why direct investment is much greater between countries with similar factor endowments than between those with dissimilar endowments.

Included Papers

Ethier, W. J. “International Trade and Labor Migration,” **AMERICAN ECONOMIC REVIEW** 75 (4), September 1985, pp 671-707.

Ethier, W. J. "Illegal Immigration: The Host Country Problem," **AMERICAN ECONOMIC REVIEW** 76 (1), March 1986, pp 56-71.

Ethier, W. J. "The Multinational Firm," **QUARTERLY JOURNAL OF ECONOMICS** 101 (4) November 1986, pp 805-833.

The Other Papers

- [1] Ethier, W. J. "International Trade Theory and International Migration (1986)," in: O. Stark (ed.), **MIGRATION THEORY, HUMAN CAPITAL AND DEVELOPMENT** (Greenwich: JAI Press), pp 27-74.
- [2] Ethier, W. J. and L. E. O. Svensson (1986) "The Theorems of International Trade with Factor Mobility," **JOURNAL OF INTERNATIONAL ECONOMICS** 20 (1/2), pp 21-42.
- [3] Ethier, W. J. (1996), "Theories about Trade Liberalization and Migration: Substitutes or Complements?" In: Lloyd, P. J. and L. Williams (eds.), **INTERNATIONAL TRADE AND MIGRATION IN THE ASIA PACIFIC REGION** (Oxford: Oxford University Press), pp 50–68.

Regional Integration

Beginning in the late 1980s and early 1990s, the formation of regional trade agreements exploded, and they have now become virtually the sole arena for new international negotiations about trade policy. At essentially the same time, multilateralism became truly multilateral after the Uruguay Round, communism collapsed in most of the large part of the world that it had characterized, much of the less-developed world turned its back on decades of anti-trade policies, and direct investment also exploded and became much more multilateral. An extraordinary coincidence?

But my first foray into this area — at the time referred to as "customs-union theory" — was quite conventional. This was with Henrik Horn in "A New Look at Economic Integration." We

were simply concerned with the implications of the then emerging theories of intra-industry trade for regional integration. Our results included the proposition that a limited regional arrangement, under certain weak conditions, would be Pareto improving, and that a limited retreat from a regional arrangement, again under weak conditions, would also be Pareto improving. These are much relevant to the current international situation, but we did not realize that at the time (at least, I did not realize it: probably Henrik did).

My interest in the “new regionalism” came later, as I began to ponder the “extraordinary coincidence” referred to above. A key moment for me was when I attended a seminar on the topic by John Whalley. I recall neither the occasion nor the title of the paper. John’s argument was that the new regionalism was a way in which developing countries were trying to obtain insurance against a possible collapse of multilateralism into nationalistic trade wars. I did not buy that argument. But John’s way of posing his question, and his setting of it in the context of the developing multilateral order, was a huge stimulus to my different approach to these issues. Thank you John.

My explanation of the new regionalism, and of the “extraordinary coincidence,” came in the paper, “Regionalism in A Multilateral World.” (This was expanded upon and clarified [I hope] in “The New Regionalism”). I regard this as among my very best papers. I do not mean most influential: The vast bulk of the academic literature in this area remains fixated on Vinerian concerns.

Included Papers

Ethier, W. J. and H. Horn. “A New Look at Economic Integration,” in: H. Kierzkowski (ed.), **MONOPOLISTIC COMPETITION AND INTERNATIONAL TRADE**, 1984 (Oxford: Oxford University Press) pp 207-229.

Ethier, W. J. “Regionalism in A Multilateral World,” **JOURNAL OF POLITICAL ECONOMY** 6 (106), December 1998, pp 1214-1245.

Ethier, W. J. “The New Regionalism,” **ECONOMIC JOURNAL** 108 (449), 1998, pp 1149–1161.

Administered Protection

During much of the late 1980s and 1990s I concentrated on issues involving administered protection. I now regard that as a fallow period. Or, more candidly, as time I wish I had spent otherwise — like sipping sake in Kyoto.

Still, there are some contributions I do not wish to repudiate. The early paper, “Dumping,” combined the theoretical contractual literature of the time with a hard look at actual anti-dumping policy to give a theory of how I thought antidumping actually worked. I think it is still centrally relevant, though the increased reliance since then on antidumping has made other concerns more prominent than they had been. Still, this paper did not attempt to set antidumping into a more general context of the international commercial system.

I attempted the latter in the (less formal) paper, “The Economics and Political Economy of Managed Trade.” This analyzed an international equilibrium, and national welfare, in a context of trade governed by voluntary export restraints. I rather like this paper. But one must ask how relevant it now is in view of the Uruguay Round decision to limit severely such measures. Very relevant, I think. Also, thanks to the Uruguay Round, safeguards can now be used in ways much like voluntary export restraints. The explicit bilateral aspect is not there, but perhaps concerns about diplomacy produce much the same effect.

I also looked at administered protection in an imperfectly-competitive environment. In papers ([1] and [2]), forgotten by everyone except, sometimes, myself. What impressed me was that, the more closely I looked at antidumping law, the more likely it seemed (in the imperfectly-competitive environment) to be beneficial to national welfare, even though the law itself excludes all consideration of such welfare. This possibility — of trade policies ignoring national welfare considerations nonetheless enhancing it precisely because of that — was to figure later in my analysis of the political economy of trade policy.

Included Papers

Ethier, W. J. “The Economics and Political Economy of Managed Trade,” in: A. L. Hillman (ed.), **MARKETS AND POLITICIANS**, 1991 (Boston: Kluwer Academic Publishers), pp 283-306.

Ethier, W. J. "Dumping," **JOURNAL OF POLITICAL ECONOMY**, 90 (3), June 1982, pp 487-506.

The Other Papers

- [1] Ethier, W. J. (1993), "An Antidumping Law with a Distorted Home Market," Chapter 17 in: H. Herberg and N. V. Long (eds.), **TRADE, WELFARE, AND ECONOMIC POLICIES** (Ann Arbor: University of Michigan Press), pp 279–97.
- [2] Ethier, W. J. (1991), "Voluntary Export Restraints," in: A. Takayama, M. Ohyama and H. Ohta (eds.), **TRADE, POLICY, AND INTERNATIONAL ADJUSTMENTS** (San Diego: Academic Press), pp 3-18.

The Political Economy of Trade Policy

My main concern over the most recent decade or so has been the political economy of trade policy. Arye Hillman, my first grad student, who has himself made central contributions to the area, had long urged me to take it up. Eventually I did so.

The reason was that my work on regionalism, by its nature, had caused me to begin thinking about the overall functioning of the world commercial system. This was addressed in "Unilateralism in A Multilateral World," which concluded that a critical role could be played by policy administrators not concerned with maximizing national welfare, as discussed in the previous section. But this could not be a matter of policy delegation by trade-agreement negotiators, as they would never choose to so delegate. (This is another of the papers that I regard as among my best).

A second reason for my concern with the political economy of trade policy is that it seemed to me that the dominant academic literature had it all wrong (Hillman, etc. excepted). This literature gives the central role to governmental concern about the terms of trade and about trade-tax

revenue, a concern that I could not see was present at all in reality (as regards the industrial countries).

This found expression in “Political Externalities, Nondiscrimination, and A Multilateral World.” I regard this as another of my badly-written papers, though in this case I have been unable to think of anyone else I could possibly blame for that. (But I continue to work on it).

Another aspect of my concern with the dominant academic literature was its empirical component. I thought that this component was of great quality in itself, but that it related in a highly suspect way to both the theory and reality. This motivated the papers, “Selling ‘Protection for Sale’” and “The Political-Support Approach to Protection.” It also played a big role in the survey piece, “The Political Economy of Protection.”

The theoretical literature on trade agreements emphasized the role of trigger strategies in sustaining repeated-game equilibria. This may or may not explain a desire by major countries to adhere to the international order in a general way. But it clearly had nothing to do with the WTO punishment mechanism in its dispute-settlement process. Thus my paper, “Punishments and Dispute Settlement in Trade Agreements: The Equivalent Withdrawal of Concessions.” This attempted a theoretical analysis of the actual WTO dispute-settlement mechanism.

Included Papers

Ethier, W. J. “Unilateralism in A Multilateral World,” **THE ECONOMIC JOURNAL** 112 (479), 2002, pp 266–292.

Ethier, W. J. “Political Externalities, Nondiscrimination, and A Multilateral World,” **REVIEW OF INTERNATIONAL ECONOMICS** 12 (3), 2004, pp 303–320.

Ethier, W. J. “PUNISHMENTS AND DISPUTE SETTLEMENT IN TRADE AGREEMENTS: The Equivalent Withdrawal of Concessions,” **KEIO ECONOMIC STUDIES** 42 (1-2), pp 1–23.

Ethier, W. J., “Selling ‘Protection for Sale,’” **ASIA-PACIFIC JOURNAL OF ACCOUNTING & ECONOMICS** 13 (2), 2006, pp 153–162.

Ethier, W. J., “The Political Economy of Protection.” In: Bernhofen, D., R. Falvey, D. Greenaway, and U. Kreckemeier (eds.), **PALGRAVE HANDBOOK OF INTERNATIONAL TRADE**, 2011, pp 295–320.

Ethier, W. J., “The Political-Support Approach to Protection,” **GLOBAL ECONOMIC JOURNAL** 1 (1), 2012, pp 1–14.

Some Topics in Trade Theory

This section includes some papers that I like but that do not fit naturally into the above categories.

i The theory of effective protection was a very active topic of inquiry in the 1970s, but it then quickly disappeared as a subject for theoretical research. I've been told that my paper, "The Theory of Effective Protection in General Equilibrium: Effective-Rate Analogues of Nominal Rates," was part of the reason. I doubt that. But in any case that was not my intent.

In August 1973 the *Journal of International Economics* published a symposium on the theory of effective protection in general equilibrium. This included a paper by Jagdish Bhagwati and T. N. Srinivasan which dealt with ideas I had been in contact with Bhagwati about. I felt that justice had not been done to these ideas, so I prepared the paper reprinted here and submitted it to the *JIE*. The editor, none other than Bhagwati, said he would not accept it, but also said that he was sure that Harry Johnson would want it for the *Journal of Political Economy*.

Young and naive, I followed Bhagwati's advice, and Johnson promptly rejected the paper. [In fairness to Johnson I point out that he had previously accepted for the *JPE* another paper of mine — now completely forgotten — on the same topic of effective protection].

I then submitted the paper to the *Canadian Journal of Economics*, which accepted it and published it. By then, Harry Johnson had died, at an unfairly young age. A Harry Johnson Prize was established to honor the best paper published in the *CJE* each year. My paper, which Harry himself had rejected, shared the prize in its first year. Sometimes things just turn out that way.

ii The formal academic discussion of globalization was largely concerned with explaining its apparent positive effect on the skilled-wage premium. The basic empirical conclusion was that trade liberalization (or globalization) was much less important an explanatory variable than the emergence of skill-biased technical change. This was reinforced by the fact that a Stolper-Samuelson explanation seemed confounded by the fact that the skill premium rose in both sides of the international market.

The paper, "Globalization, Globalisation: Trade, Technology and Wages," was motivated by the suspicion that changes in intra-sectoral substitution due to globalization were much more

important than the traditional trade-theory, inter-sectoral substitutions. This led to my central argument that the skill-biased technical change did not come from god but was itself at least partly an endogenous effect of globalization.

iii As mentioned in the earlier section on Economies of Scale, there was in the 1980s a dominant erroneous view that comparative-advantage could not explain the large volume of trade in similar goods between similar countries. The logical error was just that the statement, “trade is due to differences between countries,” does not imply that “the greater the differences, the greater the trade.” But the theory of comparative advantage did seem to suggest that more differences meant more gains.

So I was motivated to address this in the paper, “The Greater the Differences, the Greater the Gains?” It’s not for me to judge how successful I may have been, but this does seem to me one of the very fundamental issues in the theory of international trade that should have been addressed long ago.

I suspect that one of the reasons that I have a fondness for these three papers is that none was judged worthy of publication in a major journal. But if I had included all the papers that satisfied that criterion the present publication would look more like a library than a volume.

Included Papers

Ethier, W. J. “The Theory of Effective Protection in General Equilibrium: Effective-Rate Analogues of Nominal Rates,” **CANADIAN JOURNAL OF ECONOMICS**, May 1977, pp 233-45.

Ethier, W. J. “Globalization, Globalisation: Trade, Technology and Wages,” **INTERNATIONAL REVIEW OF ECONOMICS AND FINANCE** 14, 2005, pp 237–258.

Ethier, W. J., “The Greater the Differences, the Greater the Gains?” **TRADE AND DEVELOPMENT REVIEW** 2 (2), 2009, pp 70–78.

Concluding Remark

Well, this is what I have done with my life. Perhaps I should have done something more useful, like inventing a new flavor of ice cream. But anyway it's all transient. The Floating World.

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