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# TESTING TRADE THEORY

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## ABSTRACT

This review of the empirical literature in international micro economics observes that data have had a disappointingly small effect on the intellectual lives of international economists. The reasons for this are many and diverse, but one general comment is made here: We need better balance between the three layers of the argument: the issues, the theory and the data. The profession as a whole is imbalanced in favor of theory. But most of the empirical work is also imbalanced. Some empirical studies take the theory too seriously and lose track of the issues. Others do not take the theory seriously enough and try to make do with ad hoc but inappropriate empirical models. Some of these which lack the theory layer lack as well any clear issues. We need better balance.

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# Testing Trade Theory

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## 1 INTRODUCTION

A close look at the empirical work in international trade reveals that the methodological parable of hypothesis formulation, testing, rejecting and reformulation is a very misleading characterization of the intellectual life of international economists. An essay on "Testing Trade Theory" would be very short indeed, if it took the word "testing" literally and if it dealt with reality rather than myth. Empirical work has had some impact on the way that economists think about the determinants of international trade, but the effects have not been great. Certainly no theory has been tested in the sense of placing it genuinely at risk and as a result all models that were ever contemplated survive empirical testing. Some models have been discarded, but not because of empirical findings.

It seems to me neither surprising nor inappropriate that all models survive empirical scrutiny. It is not surprising partly because our data are limited and noisy. But even with more data and more accurate data, we should not expect to refute any models. A model is a powerful

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device for organizing our thoughts; it is not literally true; indeed it derives its power from the very fact that it is *not* literally true. Thus there is no reason to test it. Instead of testing, we should be determining its accuracy and its usefulness. We should attempt to identify empirical circumstances in which the model is useful and other circumstances in which it is misleading.

Consider two variants of the law of one price: purchasing power parity and factor price equalization. Purchasing power parity has been "tested" incessantly, and often "rejected." Factor price equalization has not been tested: we know it isn't so. But both theories are alive and well, in fact they seem equally healthy. What accounts for this? The "testing" mentality of many of those who have studied PPP preordains their work to have little genuine impact. Economists know in advance of any organized study of the data that arbitrage is not perfect and instantaneous. Thus PPP is surely not exact. An empirical finding that PPP is not exact therefore cannot be at all upsetting. Studies of PPP that hope to leave an imprint should instead be determining the circumstances under which the model is adequately accurate, and the circumstances under which it is highly misleading. Rather than "testing" they should be "estimating": estimating the speed of arbitrage.

In great contrast to PPP, factor price equalization has not been subject to "testing." It is not clear to me why there is such a difference in the intellectual history of these two models. Perhaps the reason is that factor price equalization refers to indirect international arbitrage, through trade in goods rather than trade directly in factors. Perhaps empirical workers could not fool

themselves into taking indirect arbitrage literally. Anyway, when factor price equalization has been studied, an "estimation" attitude has been taken, not a "testing" attitude. For example, Krueger(1968, p 658) found that "more than half of the difference between the United Nations estimates of per capita income of each of the less developed countries in the sample and the United States is explained by demographic variables alone..."

"Estimate, don't test" is one theme of this review. Empirical studies of trade theory have had relatively little impact because many researchers have had a "testing" mentality. A second theme is "Theory before estimation." Empirical studies have had relatively little impact because the theoretical foundations of the work are unclear.

If it feels like these two comments are pulling in opposite directions, indeed that is the case. I believe that an influential data analysis needs a clear theoretical foundation, but not one that is taken too literally. Adequately satisfying one of these requirements without badly violating the other is the art of data analysis.

A third, related, theme of this paper is "the three layers." An influential research program must include three layers of intellectual activity. First a set of <u>issues</u> must be identified. Typically the issues in economics are policy problems, but there are also interesting scientific questions that can be pursued without any specific policy problem in mind. The second phase of a research program is the formulation of a conceptual framework, or <u>theory</u>, for addressing the issues. The language in which the theory is formulated and communicated may be mathematics, but English is an alternative that is now too often overlooked. The third layer is the <u>data</u>-layer which develops a

correspondence between the important aspects of the theory and observable events. The tools of formal econometrics may be useful here, but tables and graphs can often be more persuasive.

We need some sensible balance between these three layers- issues, theory and data. But the theory layer is now way too thick in economics training and research. For example, think about how we discuss dimensionality questions in the context of the Heckscher-Ohlin model. We are required to learn that the production frontier has flats of dimension equal to the number of goods minus the number of factors. Thus, given certain product prices, the choice of output is indeterminate. But wait a minute; what issue is being addressed by this theoretical discussion? And how can we map the theory into observable phenomena? How do we actually go about counting factors and goods? And if not that, what observable phenomenon would make one think that there are flats on the production frontier?? I am worried that this is an example of the pursuit of theory for its own sake. But economics is not a branch of mathematics, and though the question of dimensionality is intriguing, we have to insist on clear references to the issues and to the data.

The central issue of international economics is: <u>How, if at all, should governments intervene in international commerce</u>? It is easy to lose track of this issue and any other issue when one teaches and studies international economics. The teaching of international economics usually proceeds through four different models, each of which is extensively manipulated to derive many interesting conclusions. These models are:

(i) The Ricardian model with a single labor input and with technological differences between countries.

(ii) The Ricardo-Viner model with a single mobile factor, labor, and a set of specific factors which are immobile across industries.

(iii) The Heckscher-Ohlin model with at least two internally mobile factors and at least two commodities.

(iv) A variety of models with increasing returns to scale and market power at the level of the firm.

These models serve as vehicles for discussing the sources of international comparative advantage and the consequent effects that government interventions may have. The sources of comparative advantage are: (a) differences in technology, (b) differences in factor supples, (c) economies of scale (d) differences in tastes, and (e) barriers to trade.

The myth of positive economics would make us expect to find in the literature some decisive empirical tests that have led the profession to discard one or more of these models. In fact, only two empirical findings seem to have had a major impact on the way that economists think. The first was Leontief's (1953) discovery that U.S. imports were more capital intensive than U.S. exports. This was widely regarded to be a great blow against the Heckscher-Ohlin model, which was met by a variety of theoretical responses that either amplified or altered the Heckscher-Ohlin model. The second major empirical finding was the extensive amount of "intra-industry" trade catalogued by Grubel and Lloyd(1975). The extent of intra-industry trade is also regarded as a blow against the generality of the Heckscher-Ohlin model and is at least partly responsible for the large theoretical literature on models with

increasing returns to scale and product differentiation. Other than these two results, beliefs about the sources of international comparative advantage have not been greatly affected by any observations.

That seems a bit disappointing, which leads to the question: How can we do empirical work that is more influential? My answer has already been provided: We need balance between the three layers. The empirical work needs a solid base in economic theory. But the theory cannot be taken so seriously that we lose track of the issues. We must put real intellectual capital at risk.

It may seem obvious that empirical work should have a solid base in economic theory, but in fact it is usually extraordinarily difficult to translate a theoretical model into an empirical application. There are many examples of work in international economics in which the translation of the theory into an empirical exercise was casual and "intuitive", and which later were discovered to have been fatally flawed.

It may also seem obvious that influential empirical work requires interesting and refutable hypotheses. But in fact a lot of time is wasted testing hypotheses that are surely false, a fact that is revealed whenever the data sets are large enough. The sharp hypotheses that are represented by each of the four models listed above, or each of the five sources of comparative advantage, are not sensibly tested. These models are only tools, each of which is appropriate in some circumstances and inappropriate in others. Empirical enterprises should therefore not attempt to test the validity of the theories. Instead, empirical work might identify the circumstances under which each of the tools is most

appropriate, or measure the "amount" of trade that is due to each of the sources. Neither of these tasks has been accomplished or often even attempted.

## 2 EMPIRICAL STUDIES OF THE RICARDIAN MODEL OF COMPARATIVE ADVANTAGE

A simple model of comparative advantage was offered by Ricardo in 1817. This familiar model includes two countries (England and Portugal), two goods (cloth and wine), a single input (labor), and constant ratios of output to input. The purpose of this model seems to lie primarily in the area of political philosophy, not economic science. Ricardo brilliantly makes the argument that both countries benefit from trade, even if one has an absolute productive advantage in both goods. Thus the imposition of trade barriers makes one and possibly both worse off. But it is difficult to detect anything genuinely empirical about this model. What are it's testable propositions? Here are three:

1) International trade is beneficial. This proposition has been subjected to a great deal of theoretical scrutiny. Theorists have pointed out that some individuals may be made worse off if the ownership of productive factors is unequal and if factors are sector specific. Theorists have proposed many other models with features such as economies of scale, non-competitive market structures, etc. in which the proposition may not be true. But, to my knowledge, there has never been an attempt to test the very basic premise of economics that individuals and/or economies possess enough knowledge and skill to assure that "voluntary" trades can only occur if both parties "expect" to be made better off. Furthermore, I cannot imagine how this proposition might be tested. Surely this is a Lakatosian(1978) "hard core" proposition that is protected from falsification by the undefinable word "voluntary".

2) The observed terms of trade is bounded between the comparative labor cost ratios of the two countries. This proposition is too closely associated with the very simple 1-factor, 2-good, 2-country model to be worthy of empirical scrutiny. And multi-dimensional generalizations of the Ricardian constant cost model, such as the 1-factor many-good many-country model, are complex enough to make the link between the theory and the empirical work very difficult. It should thus not come as a surprise that (to my knowledge) there have been no studies of the relationships between relative prices of different commodities and relative labor costs. For that matter, if one were to do a serious study of the technological determinants of commodity prices, one would surely include more than just the labor input.

3) A country exports the commodity in which it has a comparative labor cost advantage and imports the commodity in which it has a comparative disadvantage. Again this proposition is too closely associated with the very simple model to be worthy of empirical scrutiny. But loose versions of this proposition were the first to be studied empirically. Before discussing these studies, we need to increase the dimensions of the Ricardian model, and also allow for imperfect substitutability between foreign and domestic goods.

Haberler(1933) and Viner(1937) generalize the simple Ricardian model to the case of two countries and many goods, producing the familiar "chain of comparative advantage." This chain is formed by ordering commodities by their relative comparative labor productivities in the two countries. Multiplying the inverse of these productivity ratios by the relative wage ratios in the two countries produces an ordered set of relative prices. One country produces the first subset

of commodities with price ratios less than one, and the other country produces the second set of commodities with price ratios exceeding one. The borderline between these two sets of commodities depends on demand conditions, but the ordering does not.

The sharp implications of this theory concerning the extent of specialization can quickly be rejected by any data set. At even finely defined commodity categories, complete specialization is not the rule. Clearly the model needs amendment. The tradition, sometimes only implicit, is to treat goods produced by different countries as imperfect substitutes. This may be a casual way to deal with aggregation problems caused by the fact that only commodity aggregates are observed.

The following is a Ricardian model of price formation coupled with the assumption that goods produced by different countries are imperfect substitutes. Given that there is only labor input, we can use the labor productivity in industry k in country i and the prevailing wage to solve for the price of the commodity. Let

$Q_{ik}$ = output of industry k in country i

$L_{ik}$ = labor used in industry k in country i

$q_{ik}$ = labor productivity = $Q_{ik}/L_{ik}$.

If labor is uniform in quality, mobile across industries, and the only input, then we can define

$w_i$ = the wage rate in country i

and solve for the product price

$p_{ik}$ = wage$_i$ / (output per man)$_{ik}$ = $w_i/q_{ik}$ .

Further, assume that the relative demand for the commodity offered by countries i and i' satisfies the relationship:

$$X_{ik}/X_{i'k} = f_k(p_{i'k}/p_{ik}) = f_k([q_{ik}/q_{i'k}]/[w_i/w_{i'}])$$

where $X_{ik}$ = exports of commodity k by country i.

In words this last result asserts that export success depends on relative labor productivity. However, this is not enough to justify the cross-commodity comparisons that are now to be discussed because of the commodity subscript on the demand function $f_k$. It is necessary also to eliminate this commodity subscript by assuming that the elasticity of substitution between goods produced at different locations is the same for all goods $f_k$ -f. This seems pretty doubtful.

The earliest study of the commodity composition of international trade by MacDougall(1951) implicitly uses this framework and explains the export performance of the United States relative to the United Kingdom in terms of the relative labor productivity. Using 1937 data MacDougall finds the export ratio $X_{ik}/X_{i'k} > 1$ whenever the U.S. productivity advantage $q_{ik}/q_{i'k} > 2$ and notes that wage rates in the U.S. were approximately twice wages in the U.K. This seems supportive of the Ricardian model, but MacDougall notes that even in industries in which the U.K. has a strong comparative advantage, the U.K. share of the U.S. market is small. He suggests that trade barriers may be the reason and finds that U.S. tariffs did offset the U.K.'s comparative cost advantage in many products.

Balassa(1963) extends the work of MacDougall and offers "An Empirical Demonstration of the Classical Comparative Cost Theory" Here are two of his results based on 1950 data on 28 manufacturing industries (standard errors in parentheses).

$$X_k = -53.3 + .721\ P_k \qquad R^2 = .64$$

$$X_k = -181.2 + .691\ P_k + .14\ W_k \qquad R^2 = .81$$

$$(.167) \qquad (.102)$$

where $X_k$ = the export ratio $X_{ik}/X_{i'k}$, $P_k$ = the productivity ratio $q_{ik}/q_{i'k}$, and $W_k$ is the wage ratio: $(\text{U.S. wage})_k/(\text{U.K. wage})_k$. Balassa's first result seems to support the comparative cost model in the sense that US export performance tends to be relatively good in industries in which it has a relatively large labor productivity. The second result is something of a mystery. Why should high wages lead to export success? This is very suggestive of a multi-factor model including human capital as one of the inputs.[2]

OK, what have we learned from this? I think not too much. The regressions just described have not had a detectable impact on the collective consciousness of international economics. Why not? For the same reasons that much empirical work is limited in value. First, the Ricardian model is not sensibly interpreted literally when it is studied empirically, and the nonliteral translations of the model seem to have a lot of loose ends. Secondly, the studies are done without referring adequately to the range of alternative hypotheses that might be considered. There must be any number of reasons why success in exporting is related to productivity. For these reasons there is little intellectual capital at risk when these regressions are estimated.

## 3 EMPIRICAL STUDIES OF THE RICARDO-VINER MODEL OF COMPARATIVE ADVANTAGE

When international economists think of the effects of tariffs, they often have the Ricardo-Viner model in mind. This model has a general mobile factor, say labor, that is used in all industries, and a set of specific factors each used in one sector only. One interpretation is that the specific factors represent the Ricardian

[2] Bhagwati(1964) makes the comment that a critical step in the logic above is the linkage between relative product prices and relative labor productivities, but he finds no significant relation exists.

technological differences. Viewed this way, the differences between the models are minor. Ricardo assumes a fixed marginal product of labor whereas the Ricardo-Viner model uses diminishing marginal products. Another interpretation of the model is that the specific factor is fixed in the short run, but not the long run. This second interpretation suggests a time series study based on a model with one set of equations that take capital in place as given and allocate the mobile factors across sectors and another set of equations that allocate new investment.

Although there have been studies of the Ricardian model, to my knowledge there have been no studies of the Ricardo-Viner model. This does not seem surprising if the specific factors are taken to be fixed over time. Then there is not enough of a conceptual difference between the Ricardian model and the Ricardo-Viner model to merit the attention of empirical workers. But a study of the effect of international events on the allocation of new investment could be based on the Ricardo-Viner model and could be very interesting.

## 4 EMPIRICAL STUDIES OF THE HECKSCHER-OHLIN MODEL

Economists generally regard the Heckscher-Ohlin model to be superior to the Ricardian model for the intellectual reason that it offers a "deeper" and more "appealing" explanation of trade which does not have to resort to the "gimmick" of technological differences. England trades cloth for wine with Portugal not because the technological knowledge of cloth production is unavailable in Portugal or because grape growing and wine production are a genetic mystery to the British, but rather because the Portuguese are relatively well

supplied with land in a grape-growing climate and relatively poorly supplied with capital.

An elegant version of the Heckscher-Ohlin general equilibrium model is based on the assumptions: (1) identical homothetic tastes, (2) constant returns to scale and identical technologies, (3) perfect competition in the goods and factor markets, (4) costless international exchange of commodities (5) internationally immobile factors of production that can move costlessly among industries within a country, (6) equal numbers of goods and factors, and (7) sufficient similarities in factor endowments that countries are all in the same "cone of diversification". These assumptions imply that all countries have the same factor prices (factor price equalization), and identical input/output ratios. These assumptions also imply that the vector of net exports is a linear function of the vector of factor supplies.

The production side of the general equilibrium model with equal numbers of goods and factors can be summarized by the system of equations:

$$\mathbf{Q} = \mathbf{A}^{-1}\,\mathbf{V} \tag{1}$$

$$\mathbf{w} = \mathbf{A}'^{-1}\mathbf{p} \tag{2}$$

$$\mathbf{A} = \mathbf{A}(\mathbf{w}, t) \tag{3}$$

where Q is the vector of outputs, V is the vector of factor supplies, A is the input-output matrix with elements equal to the amount of a factor used to produce a unit of a good, p is the vector of commodity prices, and w is the vector of factor returns. Equation (1), which translates factor supplies V into outputs Q, is the inverted form of the factor market equilibrium conditions equating the supply of factors V to the demand for factors AQ. Equation (2), which translates product prices

into factor prices, is the inverted form of the zero profit conditions equating product prices p to production costs A'w. Equation (3) expresses the dependence of input intensities on factor prices w and on the state of technology t, A(w,t) being the cost minimizing choice of input intensities at time t. The assumption of constant returns to scale implies that A depends on the factor returns w but not on the scale of output Q.

The consumption side of the model is neutralized by the assumption of identical homothetic tastes. Then, in the absence of barriers to trade, all individuals face the same commodity prices, and they consume in the same proportions:

$$C = s\, C_w = s\, A^{-1}\, V_w \qquad (4)$$

where C is the consumption vector, $C_w$ is the world consumption vector, $V_w$ is the vector of world resource supplies, and s is the consumption share. Thus trade is

$$T = Q - C = A^{-1}\, V - s\, A^{-1}\, V_w = A^{-1}\, (V - s\, V_w) \ . \qquad (5)$$

The consumption share s will depend on the level of output and also on the size of the trade balance, $B = \pi' T$, where $\pi$ is the vector of external prices which in the absence of trade barriers would equal the internal prices p. Premultiplying (5) by the vector of prices $\pi$ and then rearranging produces the consumption share:

$$s = (\pi' A^{-1}\, V - B)/\, \pi'\, A^{-1}\, V_w = (GNP - B)/GNP_w, \qquad (6)$$

This is often called the Heckscher-Ohlin-Vanek model referring to Vanek's(1968) use of the assumption of homothetic tastes. Using this H-O-V model, trade is a linear function of the endowments. The more basic Heckscher-Ohlin proposition makes no reference to linearity and merely asserts that trade arises because of the unequal distribution of

resources across countries. A pure H-O model thus implies that if the ratios of resources were the same in all countries then there would be no trade. Several of the assumptions listed above can be altered without affecting this basic H-O proposition. These assumptions only introduce nonlinearities in the relationship between trade and factor supplies.

One rather silly assumption that cries out for change is equal numbers of commodities and factors. An alternative is that the number of commodities exceeds the number of factors. Then factor price equalization need not occur, and if countries are sufficiently different in their relative factor supplies, they will have different factor prices and they will produce different subsets of commodities which use intensively their relatively cheap factors.

4.1 Factor Content Studies of the Heckscher-Ohlin Model

The first and by far the most influential study of the Heckscher-Ohlin model was done by Leontief(1953) who found that U.S. imports in 1947 were more capital intensive relative to labor than U.S. exports. This empirical "paradox" sparked a search of great breadth and intensity for a theory that could explain it. Among the explanations were labor skills, trade barriers, natural resource abundance, capital-biased consumption, and technological differences.

Surprise! The Leontief finding is compatible with the U.S. being capital abundant, Leamer(1980). This is a good illustration of the need for a clear conceptual framework when empirical work is being carried out since in its absence substantial mistakes can be made.

One suspicious step in Leontief's calculation is that he separately computes the factor content of exports and imports, whereas the H-O-V

theory relates to net exports. The H-O-V theory implies that the factor content of trade satisfies the relationship $\mathbf{F} \equiv \mathbf{AT} = \mathbf{V} - s\,\mathbf{V}_w$, where the consumption share is $s = (GNP-B)/GNP_w$. From this set of equations we can separate the capital and labor content of trade: $F_K = X_K - M_K = K - sK_w$, $F_L = X_L - M_L = L - sL_w$. where X and M refer to exports and imports respectively. Leamer(1980) shows that the Leontief finding, that exports are less capital intensive than imports, $X_K/X_L < M_K/M_L$, is compatible with capital abundance, $K/L > K_w/L_w$. Using $X_K = M_K + K - sK_w$, and $X_L = M_L + L - sL_w$, it is possible to write

$$X_K/X_L - M_K/M_L \quad \propto \quad \frac{K_w/L_w}{M_K/M_L}\left[\frac{K}{K_w} - s\right] - \left[\frac{L}{L_w} - s\right]$$

where the proportion symbol indicates that a positive number multiplies this expression to create an equality. This expression indicates that if capital is more abundant than labor and if the consumption share separates the abundance ratios, $K/K_w > s > L/L_w$, then exports must be more capital intensive than imports, $X_K/X_L > M_K/M_L$. (Under these conditions, both parts of the expression are positive.) But if the consumption share is small enough that $K/K_w > L/L_w > s$ and if imports are capital intensive, $(K_w/L_w)/(M_K/M_L) < 1$, then the last term can be sufficiently negative that imports are more capital intensive than exports even though the country is relatively capital abundant. For example, suppose there are two manufactures that are produced with capital and labor, and one agricultural product that uses land, labor and capital. If land is very abundant and if agriculture uses a lot of capital compared with labor, after allocating factors to agriculture the capital may be scarce compared with labor, and a capital abundant country can import the most capital intensive manufacture and export

agricultural products and the labor intensive manufacture. This cannot happen if there are only two factors, but a three-factor numerical example is given by Leamer(1980) and corrected by Heravi(1986).

A correct way to use the H-O-V theory to infer the relative abundance of factors from the factor content of trade refers to the factor content adjusted for the trade imbalance, $F^A = AT\text{-}V_wB/GNP_w$. Using (5) and (6), this adjusted factor content is $F^A = AT\text{-}V_wB/GNP_w = V - (GNP_i/GNP_w)V_w$. Multiplying each side by $(V_{wk})/(GNP_i/GNP_w)$ produces

$$Z_{ik} = (F^A_{ik}/V_{wk})/(GNP_i/GNP_w) = (V_{ik}/V_{wk})/(GNP_i/GNP_w) - 1 \qquad (7)$$

The ratio of the resource share $(V_{ik}/V_{wk})$ to the GNP share $(GNP_i/GNP_w)$ of the right hand side of this expression is a measure of the abundance of factor i. On the left-hand side of this expression is the exported share of the domestic supply adjusted for the trade imbalance. Thus the theory suggests there are two ways to measure factor abundance. Directly by $(V_{ik}/V_{wk})/(GNP_i/GNP_w) - 1$ or through the adjusted factor content of trade $(F^A_{ik}/V_{wk})/(GNP_i/GNP_w)$.

Measures of the adjusted factor content of trade $Z_{ik}$ for the United States, the United Kingdom and Japan in 1967 using U.S. factor intensities are reported in Table 1.

Table 1
Ratio of Adjusted Net Trade in Factor to National Endowment times 100

| | U.S. | U.K. | Japan |
|---|---|---|---|
| Capital | 0.08 | -12.86 | -5.47 |
| Labor | -0.25 | 0.63 | 0.10 |
| Prof/Tech | 0.23 | 1.77 | 0.44 |
| Manager | -0.11 | 2.04 | 0.48 |
| Clerical | -0.19 | 1.37 | 0.33 |
| Sales | -1.10 | 1.30 | -0.05 |
| Service | -0.68 | 1.32 | -0.03 |
| Agriculture | 1.54 | -18.57 | -1.54 |
| Production | -0.34 | 1.11 | 1.18 |
| Land | | | |
| Arable | 19.45 | -313.42 | -341.42 |
| Forest | -23.82 | -2573.99 | -268.58 |
| Pasture | -1.63 | -91.89 | -1998.58 |

Source: Bowen, Leamer and Sveikauskus(1987)

The qualitative content of equation (7) has been studied in at least two ways: by examining the signs of the numbers $Z_{ik}$ or their rank ordering. A Leontief type of study selects a country i and compares the numbers $Z_{ik}$ for different factors k, say capital and labor. If $Z_K > Z_L$ where K and L refer to capital and labor, then trade reveals that the country is capital abundant compared to labor. Indeed that is Leamer's(1980) comment on Leontief: if you do the calculation right, then the U.S. is revealed to be relatively capital abundant. This is true also for the 1967 data reported in Table 1 since the U.S. capital number of .08% exceeds the overall labor number of -.25%. According to the data in Table 1, the United States is most abundant in arable land and most scarce in forest land.

It is also possible to make comparisons across countries. The United Kingdom is more scarce in capital than Japan which is more scarce than the United States. The United Kingdom is most abundant in labor, overall. Japan is scarcest in arable land.

A test of the H-O theory compares the numbers in Table 1 with direct measures of factor abundance. Tests of this form are what Bowen, Leamer and Sveikauskus(1987) call rank tests since they compare the rank order of factor abundance measured directly and through the factor content of trade.

It is also possible to perform "sign" tests that compare the signs of the left and right of equation (7). This was first done by Brecher and Choudhri(1982) who mention that a feature of Leontief's data is that the net export of labor services is positive, even after adjusting for the trade imbalance. Using the right-hand side of (7) this implies that the U.S. per capita GNP is less than world per capita GNP, which is impossible to square with the facts. Another way to describe sign tests is that they compare the resource abundance of one factor with an average of all the other factors since the GNP ratio is an earnings weighted average of all the factor abundance ratios. By examining the signs in Table 1 we infer that the United States was abundant in capital, professional workers and arable land and scarce in unskilled labor. Both the United Kingdom and Japan were scarce in capital and land and abundant in labor. Sign tests would compare these signs with the corresponding signs of direct measures of the factor abundance(7).

Bowen, Leamer and Sveikauskus(1987) in a study of 1967 data on 27 countries and 12 factors find about thirty five per cent violations of the signs implied by (7) and about fifty per cent violations of the

ranks. This seems disappointing, but what did you expect? In the absence of a clearly stated alternative theory, it seems impossible to determine just how many violations are enough to cast substantial doubt on the theory.

Remember, I think all three layers of an economic argument(issues, theory and data) need to be present if the argument is going to be genuinely persuasive. This research has a very clear theory and a close link between the theory and the data, but the issues have been forgotten.

4.2 Cross-commodity Comparisons

The Heckscher-Ohlin model has often been studied empirically with cross-commodity comparisons implicitly based on the assumption that the export performance "should" depend on the characteristics of the industry. Simple correlations were rather common early in the literature, but these gave way to multiple correlations in the 1970's.

For example, Keesing(1966) reports some simple correlations of export performance (U.S. Exports)/(Group of 14 Countries Exports) with skill intensities that are reported in Table 2

Table 2

Keesing's(1966) Simple Correlations
of Labor Share and Export Performance
(U.S. Exports)/(Group of 14 Exports)

| | Skill Groups | 46 Industries | 35 Industries[1] |
|---|---|---|---|
| I. | Scientists and Engineers | .49 | .72 |
| II. | Technicians and Draftsmen | .37 | .55 |
| III. | Other Professionals | .41 | .58 |
| IV. | Managers | .16 | .06 |
| V. | Machinists | .22 | .37 |
| VI. | Other Skilled Manual Workers | .11 | .21 |
| VII. | Clerical and Sales | .35 | .44 |
| VIII. | Unskilled and Semi-skilled | -.45 | -.64 |

[1] Excluding Natural Resource Industries

These results are suggestive of human capital abundance in the United States because the largest positive correlations occur at the highest skill levels and because the unskilled labor share is actually negatively correlated with export performance.

A typical multiple regression is Baldwin's(1971) (reported incompletely):

$$X_k = -1.37(K/L)_k + \sum_f \beta_f P_{fk} - 421s_k + 343u_k \qquad R^2 = .44$$

where

$X_k$ = U.S. (adjusted) net exports of commodity k in 1962

$(K/L)_k$ = capital/labor ratio in industry k

$P_{fk}$ = percentage of labor force in skill group f

$s_k$ = an index of scale economies

$u_k$ = an index of the rate of unionization.

One thing that might be concluded from this regression is that the negative sign on the capital intensity variable is suggestive of the Leontief paradox that the United States does not export goods that are capital intensive.

These simple correlations and multiple regressions raise a number of questions:

1) How should the export performance variable be scaled? Keesing scales by the exports of a comparison group of fourteen countries. Baldwin uses the unscaled data, which seems a bit uncomfortable since all of his explanatory variables are scaled.

2) Is it more appropriate to use simple correlations or multiple regressions?

3) How should the "importance" of a resource be inferred? By the size of the simple correlation? By the t-statistic in the multiple regressions?

4) Is it legitimate to exclude the natural resource industries?

5) Is it legitimate to include measures like the indices of scale and unionization?

These questions can only be answered with reference to a clear theoretical framework.

I have argued in several papers, Leamer and Bowen(1981), Leamer(1984) and Leamer(1987), that cross industry regressions generally have an unclear theoretical foundation. In deciding the kind of equation to estimate, the first important question is how to scale the dependent variable in a way that makes the cross industry comparisons sensible. The absolute level of output or trade does not seem to be a very sensible dependent variable because some commodity groups form large shares of output and consumption whereas others form small shares. If no attempt is made to control for scale, any explanatory variable that is correlated with the size of the commodity group will pick up the scale effect. To put this another way, without some way to correct for the relative sizes of different commodity groups, the estimates will be highly sensitive to the level of aggregation. The scale effect has traditionally been controlled by dividing the dependent variable by some measure of market size. The ideal candidate would seem to be total world output. What seems to lie behind this normalization is the intuitive notion that a country's share of world output can be expected to depend on the input mix of the commodity: Thus countries that are abundant in capital "ought" to have larger shares of capital intensive industries than of labor intensive industries. But what seems intuitively clear is not always true. To explore this formally, let us focus on the production side of the Heckscher-Ohlin model with equal

numbers of factors and goods and with sufficient similarity of endowment supplies that all countries have the same factor prices and use the same input mixes.

Equation (1) then identifies a set of relationships between outputs, factor intensities and factor supplies. If data are collected for a single country only, then the endowment vector **V** is necessarily constant and (1) explains the level of production of each commodity as a function of the factor intensities **A**. This equation suggests that the "correct" variables to include in the equation are elements of the inverse of **A**, not elements of **A**. Usually, however, the dependent variable is not selected to be the level of output which can vary enormously if data are in monetary units and oddly if data are in other units. It is traditional to normalize by a variable that represents the "size" of the commodity in world markets such as the level of the world's output of the commodity. By Cramer's rule, the share of the country output of commodity one is

$$Q_1/Q_{1w} = \det[V,A_2,A_3,\ldots,A_N]/\det[V_w,A_2,A_3,\ldots,A_N]$$

where $A_j$ refers to a column of the matrix **A**, $Q_{1w}$ is the world output of commodity one and $V_w$ is the world's vector of factor endowments. Note that this formula indicates that the share of world output of commodity one does not depend on $A_1$, the input mix in industry one![3] This model thus suggests that it is entirely inappropriate to regress output shares on characteristics of industries.

Many cross-industry regression studies in the literature have not used the world shares as the dependent variable. Typically, the dependent variable is the trade-dependence ratio equal to the level of

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[3] Leamer(1988)

net exports as a share of domestic consumption. Exactly the same comment applies if the model (1) - (6) is used. Using Cramer's rule we can solve for the trade dependence ratio for the first commodity as:

$$T_1/C_1 = \det(V\text{-}sV_w, A_2, A_3, \ldots, A_n) / \det(sV_w, A_2, A_3, \ldots, A_n)$$

The same result thus applies: the trade dependence ratio in industry one is altogether unrelated to the characteristics of that industry.

Another comment on cross-commodity regressions is offered by Leamer and Bowen(1981). It has been a tradition to regress trade on factor intensities and to assume that the signs of the coefficients reveal the relative abundance of factors. For example, a country that is relatively well endowed with capital is expected to have a positive coefficient on the capital variable when trade is regressed on a set of factor intensities. But as Leamer and Bowen(1981) observe, there is no assurance that this is true. The regression vector formed when the unscaled trade data T are regressed on the input intensities A is $(AA')^{-1}AT = (AA')^{-1}(V - s\,V_w)$, which has the same sign as $(V - s\,V_w)$ only under special circumstances.

Your "intuition" about the correlation of trade and factor intensities may refer to the simple correlation, not the multiple correlation. The simple correlation between trade and the capital input, for example, is

$$\begin{aligned} \text{Corr}(T,A_K) &= (T'A_K - 1'T1'A_K/p) \,/\, \sqrt{(T'T - (1'T)^2/p)}\sqrt{(A_K'A_K - (1'A_K)^2/p)} \\ &= (K - sK_w) \,/\, p\,\sqrt{\text{Var}(T)}\,\sqrt{\text{Var}(A_K)} \end{aligned}$$

where $A_K$ is the vector of capital requirements with one entry for each industry, p is the number of industries and where I have used $T'A_K = K - sK_w$ and the trade balance restriction $1'T = 0$. Thus, if trade is balanced, $B = 0$, the sign of the simple correlation is the same as the

sign of the excess factor supply $K - sK_w$. For example, a country that is well endowed in capital will have trade positively correlated with capital intensity. By this type of reasoning, the simple correlations in Table 2 suggest that the U.S. was relatively abundant in all the skilled labor categories and relatively scarce in unskilled labor.

More than just the sign, it is natural to suspect that the simple correlation between trade and factor intensity is highest for the factor that is most "important", scientists and engineers in Table 2, for example. In theory, the absolute size of the correlation depends on the degree of "peculiarity" of this resource supply $K/K_w - s$ and also on the term $K_w / \sqrt{Var(A_K)} = 1/(Var(A_K/K_w)^{1/2}$. The number $A_{Kj}/K_w$ is the inverse of the amount of the output of commodity j that would be produced in the world if j used only capital. Thus the term $(Var(A_K/K_w)$ compares in a scale-free way the variability of resource use across industries. In that sense, the correlation is high if the supply of the resource is unusual and if the intensities are highly variable across industries.

4.3 Studies of the Heckscher-Ohlin Model Based on Cross Country Comparisons

Cross-country comparisons are another way to study the validity of the Heckscher-Ohlin Theorem. Studies of this type hold fixed the commodity and use the country as the experimental unit. Normally the tool of analysis is multiple regression with some measure of trade performance as the dependent variable and various characteristics of countries as the explanatory variable. Chenery(1960), Chenery and Taylor(1968) and Chenery and Syrquin(1975) were some of the earliest studies of this type although these studies did not deal with details of the structure of trade but rather with more aggregate features of the

economy like the ratio of gross imports to GNP. Leamer(1974) was one of the first to study commodity composition questions, contrasting the performance of three groups of variables as predictors of imports disaggregated by commodity; these groups are resistance(tariffs and distance), stage of development(GNP and population) and resource supplies (capital,labor, education and R&D). Leamer finds that the development group is generally most important in helping to predict import patterns.

The theory underlying many of these cross sections regressions is casual at best. This contrasts with Leamer(1984) which takes equation (5) the H-O-V model, $T = A^{-1} (V - s V_w)$, as the clearly stated foundation for running regressions of net exports on factor supplies. One function of such an estimation exercise implicitly is to infer the value of $A^{-1}$ and to study how this changes over time. The question that is implicitly addressed is: "What resource supplies determine comparative advantage?"

Some typical results from Leamer (1984) are reported in Table 3. These are beta-values from regressions of four commodity aggregates on 11 resource supplies. The data refer to trade and resource supplies of 60 countries in 1975. Incidentally, a beta coefficient is equal to the estimated coefficient times the ratio of the standard error of the explanatory variable divided by the standard error of the dependent variable. A beta coefficient answers the question: if the explanatory variable changes by a typical amount (one standard error), does the dependent variable change by a typical amount as well?

Table 3
Beta Values of Net Export Regressions

| | Cereals | Labor Intensive Manufactures | Capital Intensive Manufactures | Machinery |
|---|---|---|---|---|
| CAPITAL | -.17 | .08 | .78 | .49 |
| LABOR1 | .74 | -1.13 | -1.8 | -.39 |
| LABOR2 | -.55 | .93 | .85 | .18 |
| LABOR3 | -.15 | .08 | .37 | .02 |
| LAND1 | .09 | -.04 | -.03 | -.01 |
| LAND2 | .03 | -.02 | -.01 | .0 |
| LAND3 | .26 | -.04 | -.15 | -.06 |
| LAND4 | .05 | -.15 | -.10 | -.11 |
| COAL | .03 | -.14 | -.09 | -.02 |
| MINERALS | .0 | -.03 | -.03 | -.01 |
| OIL | .72 | -.24 | -.60 | -.21 |

Source: Leamer(1984).
Note: Labor is disaggregated by skill; land by climate

Based on these beta-values, comparative advantage in cereals is associated with abundance of highly skilled labor, land of type 3 and oil. Comparative advantage in the three manufactures is associated with supply of the moderately skilled workers and capital, and is negatively related to the supply of land.

4.4 Studies of the Heckscher-Ohlin Model Using Two-Dimensional Data

Beginning with Leamer and Bowen(1981), I have often made the observation that the Heckscher-Ohlin Model links three separately observable phenomena: trade, resource supplies and technological input coefficients. A full test of the theory accordingly must begin with separate measures of all three of these concepts and must explore the extent to which the observed data violate the H-O restrictions.

Hufbauer(1970) is a notable early study that employs measurements of all three concepts. Some typical results are reported in Table 4.

Table 4
Capital per Man
Source: Hufbauer(1970)

| | Abundance | Exports | Imports |
|---|---|---|---|
| Canada | 8,850 | 17,529 | 11,051 |
| United States | 7,950 | 11,441 | 13,139 |
| Norway | 6,100 | 16,693 | 10,476 |
| Sweden | 5,400 | 12,873 | 11,373 |
| Netherlands | 4,750 | 11,768 | 11,706 |
| ... | | | |
| Korea | 850 | 8,004 | 14,900 |
| India | 500 | 7,339 | 12,019 |
| Pakistan | 500 | 5,725 | 12,371 |

The countries in this list are ordered by measures of their capital per man with Canada being the most abundant in capital and Pakistan the least abundant. The capital per man in exports is compared with the capital per man in imports in the next two columns. It should be noted that the U.S. data displays the "Leontief paradox" that imports are more capital intensive than exports. But this is not true for the other countries at the top of the list. Hufbauer reports that the capital per man (first column) has a correlation of .625 with the capital per man in exports (second column) and a correlation of -.353 with capital per man in imports (third column). This is regarded to be confirmatory of the H-O model: capital abundant countries tend to have capital intensive exports and labor intensive imports.

There are four comments that can be made about this study.

(1) The study uses measures of all three concepts: factor supplies, trade and technological input intensities. As I have already mentioned, a full test of the H-O model must surely make reference to all of these.

(2) Hufbauer's analysis does not refer explicitly to any model. It separates imports from exports, which got Leontief in trouble.

(3) I find it curious that the capital per man in exports varies greatly across countries in contrast to the capital per man in imports. I would not have expected this result based on my understanding of the H-O model. What might account for it? Perhaps the model with more goods than factors can help. In the H-O model with many goods and two inputs, countries concentrate production on just two of the goods and import all the rest. The two produced goods have similar capital intensities. In words, countries have a diversified import structure but a concentrated export structure.

(4) Competing models and/or factors that might explain trade are "tested" by comparing the size of the correlations that they produce. The list of theories is noticeably inclusive: factor proportions, human skills, scale economies, stage of production, technological gap, product cycle and preference similarity.

Bowen, Leamer and Sveikauskus(1987) also use measurements of all three concepts and link their work to a carefully formulated model, namely the H-O-V model as captured by equation (7) which determines the adjusted factor content of trade as a function of resource supplies. Recognizing the impossibility of testing a theory without an alternative, these authors generalize the H-O-V model to allow (a) non-homothetic tastes characterized by linear Engel curves, (b) technological differences among countries that affect all technological coefficients proportionately and (c) various kinds of measurement errors. In the words Bowen, Leamer and Sveikauskus (1987,p.805) "The data suggest errors in measurement in both trade and national factor supplies, and favor the hypothesis of neutral technological differences across countries. However, the form of the technological differences

favored by the data involves a number of implausible estimates, including some in which factors yield strictly negative outputs. Thus,... The Heckscher-Ohlin model does poorly, but we do not have anything that does better." OK, but what are the issues? How does this help us design trade interventions?

4.5 Comment on the Studies of the Ricardian Model

I have argued above that the studies of the Ricardian model that regress measures of relative export performance on measures of relative labor productivities are only loosely connected with the Ricardian model. We can also ask if the Ricardian regressions make sense from the standpoint of the H-O model. Clearly, the answer is no if the even H-O model is used since it implies that labor productivities are the same in all countries. Cross-commodity empirical studies suggested by the Heckscher-Ohlin model thus usually proceed as if this were so and use the input intensities from one country as explanatory variables rather than ratios of intensities from different countries. The Heckscher-Ohlin regressions take the form $X_{ik}/X_{i'k} = f(q_{ik})$ where $X_{ik}$ equal exports of commodity i by country k and $q_{ik}$ is the corresponding labor productivity measured in one of the countries. This contrasts with the Ricardian equation which uses the relative labor productivities: $X_{ik}/X_{i'k} = f(q_{ik}/q_{i'k})$.

In a search to give the Ricardian regression some meaning from the standpoint of this version of the H-O model, it seems natural to consider aggregation over goods or over factors. Aggregation is something that needs always to be considered since real data on commodities and factors necessarily refer to aggregates. In particular, differences in labor productivities may be a consequence of aggregation

even though at the level of the commodity labor productivities are equal. The labor productivity within a commodity aggregate is equal to $\sum p_k Q_k / \sum L_k = \sum (p_k Q_k / L_k) L_k / \sum L_k$ which is a weighted average of labor productivities with weights equal to the labor used in sector k. This aggregate labor productivity will thus be relatively high in countries with labor allocations concentrating on those industries within the aggregate with relatively high labor productivities. If the summation in this expression extends over all commodities, then the labor productivity is GNP per man which is an increasing function of the relative supplies of the nonlabor factors. Those countries with relatively high GNP per man will therefore have relatively large allocation of labor to the industries with relatively high productivities. This can make the measured productivities for commodity aggregates different in the two countries even though they are identical at the level of individual commodities. Aggregates composed of commodities with uniform values of output per man will of course have the same level of aggregate output per man in both countries since the weights do not matter. But aggregates composed of commodities with variable labor productivities will tend to have high measured labor productivities in the capital abundant country because it allocates relatively large shares of its work force to the components with relatively high labor productivities.

This discussion of aggregation does not give a very sensible foundation to the Ricardian regressions since the proposition that the U.S. has a comparative advantage in those commodities with relatively high labor productivities amounts to the odd claim that the U.S. has a

comparative advantage in the commodity aggregates composed of variable labor productivities.

As argued by Deardorff(1984), the uneven H-O model can give greater content to the Ricardian regressions because factor prices are not necessarily equalized and labor productivities may differ across countries. The supply price of commodity k in country i depends on the factor return vector and the input vector by the zero profit condition (2): $p_{ik} = \sum_f w_{if} A_{ifk}$. The relative supply price of two different countries is thus $p_{ik}/p_{i'k} = \sum_f w_{if} A_{ifk} / \sum_f w_{i'f} A_{i'fk}$ which depends on factor intensity differences and also factor return differences. This isn't exactly expressible simply in terms of relative labor productivities even if there are only two factors, since other factors matter, but differences in labor productivities do account for part of the differences in relative prices.[4]

## 5. EMPIRICAL STUDIES OF INTRA-INDUSTRY TRADE AND INCREASING RETURNS TO SCALE

The puzzling phenomenon of intraindustry trade has sparked a large theoretical literature dealing with differentiated goods produced with increasing returns to scale. Accompanying these theoretical pieces are a number of empirical studies of the determinants of intraindustry

[4] Deardorff's(1984) (loose) argument is based on the observation that countries with relatively high labor costs will use more capital intensive techniques and have higher labor productivities. He notes that the responsiveness of labor productivities in the two-factor model satisfies , $d\log(Q/L)/d\log(w/r) = \sigma\,\theta$ where $\sigma$ is the elasticity of substitution between capital and labor, and $\theta$ is the capital share. Thus if the elasticity of substitution were the same in all industries, differences in factor prices would cause the greatest differences in labor productivities in industries with the greatest share of capital. The looseness in the argument is the statement "Now suppose that the more capital abundant country has a comparative advantage in more capital intensive goods, as the Heckscher-Ohlin model predicts..." I am not sure that the H-O model does make this prediction.

trade. This area of research has a special difficulty forming interesting empirical questions because the linkage of the theory and the data analyses of necessity is often casual. Here are some of the problems:

1. It is often difficult to find any variable that closely measures the hypothetical construct stipulated by the theory. For example, Loertscher and Wolter(1980), measure "the potential for large scale production" by value added per establishment. But it isn't clear what this has to do with the fixed costs and differentiated products that are the bases for models of intra-industry trade. An industry that comprises much of GNP may be supplied by many very large establishments each producing at the efficient scale, and nonetheless have industry output exhibit constant returns to scale. A better variable might be value added per establishment relative to some measure of the total market of the good.
2. The theory consists of a set of separate models, each intended to capture one feature. There is ordinarily little attempt made theoretically to combine these models into one composite. They are combined empirically merely by inclusion of separate variables representing each model in a single linear regression equation. But absent a clear combined theory, it may make more sense to look at simple correlations, rather than partial correlations, since there is no assurance that the other influences are properly controlled merely by dumping them into a regression.
3. Studies that combine data from many industries are especially suspect, since the theoretical underpinnings of these studies are often weak. Economists would distrust estimates of a price

elasticity of demand based on observations of price and quantity collected from many industries. Some of this distrust should carry over to all cross-industry studies.

4. Null and alternative hypotheses are not often stated and are usually quite difficult to form. Hypotheses refer either to the opinion of economists or the uses to which the theory might be put. But because of measurement problems and theoretical doubtfulness, economists cannot have much in the way of well-formed opinions about the signs or sizes of the coefficients in these regressions. And the uses of these models seem pretty distant and unclear.
5. The counterfactuals that are implicit in the estimated regressions are often unclear. What exactly, for example, is meant by a change in value added per establishment?. If you cannot answer that question, then how does this tell us anything about the role of scale economies in international trade?
6. It is sometimes difficult to determine whether a projected empirical regularity is due to the existence of economies of scale or more importantly to the assumption about the nature of tastes.

With all of these difficulties, it is not surprising that the impact that these empirical findings might have on our understanding of the role of economies of scale seems not much beyond a simple measurement of the amount of intraindustry trade for various commodity groups and countries: There seems like a lot of intraindustry trade, and the Heckscher-Ohlin model doesn't seem capable of offering a very satisfying explanation of it.

An example of the kind of empirical work that has accompanied models of intraindustry trade is a study by Loertscher and Wolter(1980) who report:

> *Drawing* on the literature quoted above the following hypotheses *seem warranted*: (my italics)
> Intra-industry trade among countries is intense if
> (a) the average of their levels of development is high.
> (b) the difference in their levels of development is relatively small.
> (c) the average of their market sizes is large.
> (d) the difference in their sizes is small.
> (e) barriers to trade are low.
> Intra-industry trade in an industry is intense if
> (f) the potential for product differentiation is high and market entry in narrow product lines is impeded by significant barriers.
> (g) transaction costs are low.
> (h) the definition of an industry is comprehensive.

I have added the italics in this quotation to emphasize the casual link between the theory and the empirical work. The regression that Loertscher and Wolter compute explains a measure of intra-industry trade indexed by importer, exporter and commodity in terms of a set of variables selected to represent the various hypothetical determinants of intraindustry trade. One of their results is reported in Table 5.

Table 5

Country- and Industry-Specific Determinants of Intra-Industry Trade
OECD-Countries, Cross Section 1972/73
Source: Loertscher and Wolter(1980)

| | Estimate | $t^2$-value |
|---|---|---|
| Country-specific variables | | |
| Development stage differential | $-0.106\ 10^{0}$ | 47.95 |
| Average development stage | $0.259\ 10^{-1}$ | 1.68 |
| Market size differential | $-0.146\ 10^{-5}$ | 82.71 |
| Average market size | $0.296\ 10^{-5}$ | 108.17 |
| Distance | $-0.485\ 10^{-4}$ | 44.52 |
| Customs unions dummy | $0.382\ 10^{0}$ | 64.89 |
| Language group dummy | $0.171\ 10^{0}$ | 6.43 |
| Border trade dummy | $0.268\ 10^{0}$ | 20.41 |
| Cultural group dummy | $-0.423\ 10^{-2}$ | 0.01 |
| Industry Specific variables | | |
| Product differentiation | $0.733\ 10^{-3}$ | 0.45 |
| Scale economies | $-0.311\ 10^{-1}$ | 91.23 |
| Transactions costs | $-0.225\ 10^{-3}$ | 3.71 |
| Level of aggregation | $0.137\ 10^{-1}$ | 3.05 |
| Product group | $0.112\ 10^{0}$ | 5.56 |

Adjusted $R^2$ = .070, degrees of freedom = 6975.

From my perspective it is difficult to know what to make of a regression of this type. Most of the coefficients are very "statistically significant", as might be expected with so large a sample size. The precision with which these coefficients are estimated is misleading, however, since the fit as measured by the $R^2$ is very low. One thing the low $R^2$ means is that the signs of the estimated coefficients are not resistant to measurement-error adjustments. In fact, I am pretty sure that any sign pattern of estimated coefficients would be possible if you assumed a little measurement error in a few of the variables. For the technical reasons, consult Klepper and Leamer(1984). Actually, the authors' reaction (p. 287) to the "wrong" sign on the scale economies variable is indeed mismeasurement.

But the real difficulties of interpreting a regression of this type come from the very fuzzy link between the theory and the

regression. Models do suggest that intraindustry trade is positively associated with scale economies. But no composite model has been presented which suggests that, controlling for all these other variables, the scale effect is positive. Absent a theory that tells what other things I should control, it is difficult to interpret a partial correlation which controls for a haphazardly selected group of other variables. Then it may make sense to look at the simple correlations.

Helpman's(1987) study of the effect of size dispersion on the amount of trade and the amount of intraindustry trade is noteworthy in its attempt to link more closely the theory and the empirical study. Neglecting Helpman's correction for trade imbalances, his size similarity index of a group I of industrial countries is defined to be a negative function of the variance of GNP shares:[5]

$$SIM = 1 - \sum (s_j)^2 = 1 - (1/n) \cdot Var(s_j)$$

where $s_j$ is the GNP share of country i in total GNP of group I. Total intra-group trade is

$$V_I = \sum_{i \neq j} X_{ij}$$

where $X_{ij}$ is the value of exports from i to j. Helpman's model implies that the total trade increases with similarity:

$$V_I/GNP_I = (GNP_I/GNP_W) \times SIM$$

Helpman finds that for a group of fourteen of the most industrialized countries both GNP similarity and trade intensity have increased more-or-less constantly from 1956 to 1981, giving the appearance that the model is supported.

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[5] Helpman calls this a "dispersion" index though it is a measure of how similar are the sizes of different countries.

One of the basic questions now arises: "What is the theoretical basis for this empirical work?" To express it differently: What is being tested? Economies of scale appear to be central but, in fact, the result just described comes from the consumption side of the model, and makes no serious reference to the production side. In particular, suppose that we make the "Armington" assumption that products are distinguished by location of production and make the further assumptions that tastes are identical and homothetic and that trade is balanced. Then purchases by country i of country j's product are equal to $s_i \ GDP_j - s_i \ s_j \ / \ GDP_W$, where $s_i$ is country i's GDP share. Summing this over all importers and exporters produces the result:

$$V_I = \sum_{i \neq j} s_i \ s_j \ / \ GDP_W = (1 - \sum_i s_i^2)/GDP_W \ ,$$

which is just the model that Helpman studies. If this model fits poorly, it is due to a failure of the proposition that all individuals consume the same share of total output of U.S. wine, French wine, German automobiles, ... It need not have anything to do with the method of production or the nature of competition. The same result can be obtained by appending a different model of consumption onto the basic Heckscher-Ohlin model of production.

Helpman also reports that his theory "suggests" that : "The share of intraindustry trade in bilateral trade flows should be larger for countries with similar incomes per capita. ... In order to examine the consistency of this hypothesis with the data," Helpman calculates bilateral intraindustry trade as

$$S_{ij} = 2 \sum_k \min(X_{ijk}, \ X_{jik}) / \sum_i ( \ X_{ijk} + X_{jik})$$

where k indexes commodities. For each of twelve different years this measure of intra-industry trade is explained in terms of three variables:

$X_1 = \log|(GDP_i/POP_i) - (GDP_j/POP_j)|$

$X_2 = \min(\log(GDP_i), \log(GDP_j))$

$X_3 = \max(\log(GDP_i), \log(GDP_j))$

The results for the extreme years were:

Table 6
Regressions for Intra-industry Trade
Source:Helpman(1987)

| | $X_1$ | $X_2$ | $X_3$ | $R^2$ |
|---|---|---|---|---|
| 1970: | -0.044 | 0.055 | -0.014 | 0.266 |
| | (-3.141) | (4.153) | (-1.105) | |
| 1981: | -0.006 | 0.027 | -0.020 | 0.039 |
| | (-0.370) | (1.686) | (-1.283) | |

Note: t-values in parentheses, n = 14*13/2 = 91.

From these regressions we may conclude that intraindustry trade is more intense between countries that are similar either in terms of per capita GDP's or in terms of GDP itself, though this is more difficult to detect in the latter period. The conclusion regarding the effect of similarity in the levels of GDP's refers to the fact that the coefficient on the minimum GDP is positive and the coefficient on maximum GDP is negative and approximately the same absolute size. It also appears that the coefficient on the minimum GDP is the larger in absolute value, suggesting that country size as well as similarity contributes to intra-industry trade. A second regression reported by Helpman separates GDP size from GDP similarity and confirms that both seem to contribute positively to intraindustry trade.

This work has unearthed several interesting empirical regularities but there remains a great deal that could be done on the role of economies of scale in international relationships. For example, among the unanswered questions are: How much of total trade is due to economies of scale? How much of the gains from trade are due to economies of scale? What role do tastes have in determining the result? Why does the regression fit so poorly? Which industries are best described by models of imperfect competition? ...

6. EMPIRICAL STUDIES OF THE EFFECT OF DEMAND

Trade is the difference between production and consumption. Most of the theoretical literature in international economics concentrates on the production side and often uses assumptions that neutralize demand as a determinant of the composition of trade. An early and notable exception is Linder(1961) who argues that differences in tastes is a deterrent to trade because of the costs of tailoring a product to fit local conditions. This is usually interpreted to mean that the intensity of bilateral trade decreases with differences in per capita income. The Heckscher-Ohlin model, on the other hand, "suggests" the reverse association because countries with substantially different per capita incomes are "likely" to have different resource endowments, offer different baskets of goods for trade and therefore become trading partners.

Most of the theoretical work deals with the commodity composition of trade, not the partner composition. The Linder hypothesis has traditionally been interpreted in terms of its implications for partner composition by including a measure of similarity of per capita GNP's in "gravity equations" that explain bilateral trade. The Linder

hypothesis as it relates to the commodity composition has been studied recently by Hunter and Markusen(1988) who estimate a system of demand equations and study its implications for total trade. An interesting example of the gravity models is reported by Hoftyzer(1984) who presents a model that explains the bilateral trade of each of eleven importers using data for fifty eight exporters. His results for three of the importers are reported in Table 7.

Table 7

Gravity Equations: 1970 Trade

Source: Hoftyzer(1984)

| Country | $DIFF_Y$ | DIST | $DIFF_L$ | ASSOC | COMMON | BORDER | $R^2$ | $\lambda$ |
|---|---|---|---|---|---|---|---|---|
| West Germany | .01 | -4.65 | -.29 | -.22 | | .56 | .55 | .001 |
| | (.01) | (5.00) | (.06) | (.55) | | (1.45) | | |
| Japan | -2.18 | -11.52 | -1.40 | | | | .13 | .3 |
| | (.08) | (2.70) | (.12) | | | | | |
| U.S.A. | 3.20 | -8.22 | -2.40 | | | .28 | .27 | .1 |
| | (1.15) | (3.33) | (.31) | | | (.40) | | |

t-statistics in parentheses
$DIFF_Y$ = Absolute difference in income per capita
DIST = Great circle difference between economic centers
$DIFF_L$ = Absolute difference in land per capita
ASSOC = Common membership in free trade associations
COMMON= Commonwealth country
BORDER= Common border
$\lambda$ = Box-Cox parameter

The Linder Hypotheses is interpreted to mean that the disimilarity of countries as measured by the difference in per capita incomes will lower the intensity of trade. In other words the coefficient on $DIFF_Y$ should be negative. Hoftyzer(1984) finds otherwise in the sense that for a few countries the estimated coefficient is negative, but for most

it is positive. This contrasts with some more positive results by other authors, which Hoftyzer argues are due to their failure to control for border effects and membership in free trade associations and their failure to consider other functional forms which he does through the Box-Cox analysis. But I note that this finding is in conflict with Helpman's (1987) time series finding that the rapid growth of trade over the last three decades was associated with a convergence of per capita incomes.

It's time again to trot out our two questions for the last time. Does this work have a solid base in economic theory? No, not really. Are the hypotheses phrased in a way that puts intellectual capital at risk? Actually, I think so. Even though the theoretical foundation is murky, the finding of Hoftyzer(1984) seems to me to be unsettling to the Linder viewpoint. According to Hoftyzer(1984) trade may seem intense between similar countries, but that can be explained first by the fact that they are neighbors and/or members of free trade associations and secondly that, whatever relationship exists, it is not log-linear. This for me is a memorable result, affecting my understanding of the role of demand as a determinant of trade patterns.

Still we need to ask what exactly is being studied here? What is the counterfactual? Is the question as simple as determining the sign of the difference variable after accounting for a list of randomly selected other variables? That is an interesting question, but it seems like a very limited question. Maybe the deeper issues have to do with the gains from trade and the effects of trade policy on welfare. Are the gains from trade less if the trade is Linder trade as opposed to Heckscher-Ohlin trade?

## 7 GROWTH AND OPENNESS

The phenomenal difference between the growth rates of the East Asian economies and the Latin American economies over the last several decades has stimulated a renewed interest in the determinants of economic growth. A prominent and important hypothesis is that these differences in growth rates can be explained by differences in the degree of openness to international commerce. Many suppose that the successful East Asian economies are open, and the unsuccessful Latin American economies are closed. But clear empirical support for this proposition is not easy to come by.

### 7.1 Cross-country Comparisons

Studies by Tyler(1981), Feder(1983), Kavoussi(1984), Balassa(1985) and Ram(1985) have examined the relationship between trade and growth in a cross section of countries by regressing the rate of growth of GNP on the rate of growth of trade and the rate of growth of certain measurable inputs. Generally, the coefficient on the growth of trade is positive and "statistically significant." For example, Ram(1985) reports the following regression (t-values is parentheses):

$$\hat{Y} = \underset{(-1.15)}{-1.034} + \underset{(3.41)}{1.071}\,\hat{L} + \underset{(3\ 75)}{.130}\,IY + \underset{(4.01)}{.124}\,\hat{X} \quad , R^2 = .46,\ n = 73.$$

where the $\hat{}$ indicates the average annual rate of growth, Y is GDP, L is labor force, X is exports and IY is the ratio of investment to GDP.

This kind of regression is based on the assumption that aggregate output is a log-linear function of capital, labor and technology, and that technology is a log-linear function of the level of exports. Criticisms of these findings abound.

First one might question whether the growth of GNP should be explained by the degree of openness or the growth thereof. In the regression just described, is it better to include the growth of exports or the initial ratio of exports to GNP? To seek a proper answer to this question it seems wise to model more carefully the dynamic relationship between GNP and openness. Traditional models of comparative advantage allow a one-time effect of liberalization, not a continuing effect on the growth rate. More recently, partly in response to the great differences in growth experiences of different countries, there has been an attempt to form models that do link the rate of growth with the degree of openness, not the change thereof. These models draw their dynamics from assumptions regarding economies of scale or the nature of technological change. But I am left with the impression that theory will not decisively answer this modelling question. I also suspect that the evidence embodied in aggregate data sets is unlikely to be decisive about the dynamic relationship between growth and openness.

Regardless of what model is adopted, the level of exports seems not a very good indicator of openness because much of the cross country variation in the ratio of exports to GNP is due to differences in comparative advantage, not to government intervention in international transactions. This is more than just a measurement error problem because these comparative-advantage components of the variation in export growth are unlikely to be exogenous to the process that determines GNP. For example, the discovery of gold in California precipitated an economic boom affecting both the level of exports and the level of Californian GNP. This coincident boom in both GNP and exports obviously tells us nothing about the effect of openness on

growth. A second problem with exports as a measure of openness is that exports may be high because of export promotion, rather than noninterference.

To explore and to combat these criticisms Edwards(1989,1991) and Pritchett(1991) have experimented with many other measures of openness, including some from Leamer(1987) that attempt to control for differences in comparative advantage. The variety of ways that one can measure openness using trade data leaves the impression of an annoying degree of fragility in the inferences based on them.

The choice of measures of openness creates one set of concerns. The choice of economic model of aggregate output creates another. It seems appropriate at a minimum to consider at least two alternatives to the log-linear aggregate production function. One alternative is implied by the multi-product Heckscher-Ohlin model with constant returns to scale at the level of the industry. This model implies that GNP is a linear function of the inputs with coefficients that depend on the product mix (the cone of specialization) and the level of technology: $GNP = \sum_i V_i w_i(V,t)$ where $V_i$ is the supply of factor i, and $w_i$ is the corresponding return which depends on factor supplies and technology. Stripped to its essentials this model produces just another aggregate production function and openness is a source of growth if it somehow enhances the rate of dispersion or creation of technology. A more substantial conceptual difference occurs if the disaggregated model has some sectors with increasing returns to scale. Then openness can encourage growth because it creates a larger market.

These cross-country comparisons of growth performance have certainly increased our understanding of the determinants of growth and

have added to the impression that growth is enhanced by openness, but much of the cross country variation of aggregate growth rates remains unaccounted for. It appears that the processing of these kinds of cross-country aggregate data sets is suffering from diminishing returns. Perhaps much could be gained from the study of data disaggregated by industry, and by attempting to be explicit about estimating the rate of technological dispersion and the attainment of economies of scale.

7.2 Granger Causal Orderings

A number of studies have "tested" to see if exports "Granger cause" GNP growth. Readers of this work need to proceed with caution. It is an abuse of the language to refer to temporal orderings in terms of causation. For example, we know that weather forecasts precede the weather but few of us take this to be evidence that weathermen cause the weather. Studies of "Granger Causal Orderings" of exports and GNP are not identifying causal directions but are asking only the question whether movements in exports tend to precede or follow movements in GNP. Studies of the temporal orderings of exports and GNP seem interesting on their face, but leave one wondering exactly how they relate to the growth and openness debate. Lal and Rajapatirana(1987) offer detailed criticisms and argue "if a small country is developing efficiently in line with its comparative advantage, it will specialize and hence be compelled to turn to foreign markets for exports of goods that use its most abundant factor of production most intensively." This may make it appear that GNP "Granger causes" exports, but if the economy were closed the internal growth spurt would have been choked off due to lack of markets. This is just a version of the weather and weatherman story.

7.3 Cross-industry comparisons

Most of the cross-industry studies have adopted the same casual conceptual framework as the aggregated studies. Some of these explain growth in output in terms of growth in inputs plus some measure of trade. Others study Granger temporal orderings. The one new wrinkle that is common in these disaggregated studies is the use of earnings shares to compute "total factor productivities." The logic for this computation is as follows. A typical production function can be written as $Q = f(K,L,t)$ where K is capital, L is labor and t is "technology." Differentiation of this function produces the relationship

$$dQ/Q = (f_K K/Q)\ dK/K + (f_L L/Q)\ dL/L + (f_t/Q)\ dt$$

where $f_i$ refer to the marginal products and where the last term represents the effect of technological change on output. If the goods and factor markets were competitive and if the levels of the inputs of capital and labor were costlessly adjustable by the firms, then these inputs would be paid their marginal products and the expressions like $(f_L L/Q)$ would represent the input shares. Then we could write the growth in technology in terms of the growth in "total factor productivity (TFP)":

$$dTFP/TFP = (f_t/Q)\ dt = dQ/Q - (f_K K/Q)\ dK/K - (f_L L/Q)\ dL/L$$
$$= dQ/Q - \theta_K\ dK/K - \theta_L\ dL/L.$$

In words, the growth in total factor productivity is measured as that part of the growth in output that is not accounted for by growth in inputs.

The tradition in this literature has been to explain the growth in total factor productivity in terms of measures of openness, often imports and exports. The same concerns that were raised above about

these measures of openness apply here as well. The substantial difference between these studies and the cross country comparisons reviewed above is that these latter studies presume that input shares reveal marginal products.

I hope that you detected the liberal use of the subjunctive in the preceding paragraphs: there are lots of "would's" and "were's" that reflect a high degree of scepticism about the computation of total factor productivities. Nelson(1981) is one source of criticisms of these kinds of calculations. Here are some of my own. First of all there is the assumption that inputs are paid their marginal products. If you think that inputs are paid their marginal products, then it seems to me that you are obligated to tell us the time frame to which your thoughts apply. Is it minute by minute? Probably not. But the same reservations that make you doubt the relationship between compensation and marginal products minute by minute apply also to quarterly data and even to annual data. Secondly, what about technological change? Is it really only an accident or is technological improvement a consequence of an investment, probably in the form of a salary to some employees whose job it is to put in place the new technique. If it is the latter, then the growth accounting outlined above is conceptually incorrect. Another problem is that rapid development can effect substantial changes in the input shares. The formula above applies only to infinitesimal changes in the input mix unless the earnings shares are technologically fixed as in a Cobb-Douglas production function. There are also serious questions concerning the disaggregation of factors, particularly labor which surely embodies an amount of human capital that increases with development.

Any empirical exercise is subject to a barrage of criticisms that can be incapacitating to all but the most stout-hearted. The foregoing comments are not intended to produce this response. But wariness is suggested.

7.4 Studies of major liberalizations

One of the most influential arguments that link growth and openness has been the NBER multicountry project on "Trade Regimes and Economic Development". Krueger and Bhagwati(1978) coordinated studies of ten countries that had undergone major liberalizations. These studies gain credibility from the degree and kind of institutional knowledge that they reveal. Their ultimate appeal, however, probably comes from the readers' feeling that these observations come about as close to a controlled experiment as we are likely to get. Another study in the same genre is Edwards and Edwards(1987) which deals informatively with the Chilean liberalization from 1973 to 1983.

7. CONCLUSION

I want data to have a major effect on the way that international economists think. I am disappointed, but I am not defeated. I think we can do better. My principle piece of advice is: balance. We need to have balance between the three layers of economic reasoning - issues, theory and data. The theory layer has become so large that it sometimes appears as though there are no issues and no data. The data layer, when it is present, sometimes dangles without theory, and sometimes it takes the theory too seriously, and dangles without the issues. An influential piece of empirical work would take the theory just seriously enough and would make a clear reference to the issues. We can do it.

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# Guide to Bibliography

This bibliography is arranged to correspond to the sections in the paper as indicated in the following outline. Some papers contain empirical analyses that overlap subjects in the text and their location in this bibliography is somewhat arbitrary. Surveys make up the first part of the bibliography and are associated with no special place in the text.

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