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DOES TRADE RAISE INCOME?
EVIDENCE FROM THE TWENTIETH CENTURY

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ABSTRACT

Efforts to estimate the effects of international trade on a country's real income have been hampered by the failure to account for the endogeneity of trade. Frankel and Romer recently use a country's geographic attributes - notably its distance from potential trading partners - as an instrument to identify the effects of trade on income in 1985. Using data from the pre- World War I, the interwar, and the post-war periods, this paper finds that the Frankel-Romer result is robust to different time periods, i.e., that instrumenting for trade with geographic characteristics raises the estimated positive effect of trade on income by a substantial margin and, in most of our cases, the precision of those estimates. These results suggest that the downward bias of OLS estimates is systematic and may be due to measurement error, a potential source of which is that trade is an imperfect proxy for a host of economically beneficial interactions between countries. However, the results are not robust to the inclusion of another geographic variable, latitude (distance from the equator).

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Does Trade Raise Income? Evidence from the Twentieth Century

1. Introduction

Perhaps the most fundamental proposition of international trade theory is that trade allows a country to achieve a higher real income than would otherwise be possible.[1] Yet this basic axiom has eluded satisfactory empirical treatment in part because simple ordinary least squares (OLS) regressions of income on trade neglect the endogeneity of trade and therefore fail to identify the true effect of trade. Thus, the positive correlation between trade and income could mean that countries with higher incomes engage in more trade rather than the other way around.

Frankel and Romer (1999) recently overcame this simultaneity problem by constructing an instrument using a country's geographic attributes — notably its distance from other trade partners — that are correlated with trade but uncorrelated with income. Using a cross-section of countries in 1985, they find that the effect of trade on income is considerably higher (although imprecisely estimated) with instrumental variables (IV) estimation, suggesting that the positive association of trade and income is not entirely due to high income countries trading more extensively than others.

This paper demonstrates that the Frankel-Romer finding is not unique to 1985 or even to the postwar period. Rather, in various periods during the twentieth century, instrumenting for trade with geographic characteristics raises the estimated positive effect of trade on income by a substantial margin and also, in most cases, the precision of those estimates. This brings into question Frankel and Romer's (1999, p. 394) conclusion "that the most plausible explanation of

[1] Samuelson's (1939, 1962) two papers are among the classic articles on the gains from trade, but even Adam Smith discussed how a country open to international trade would increase "the exchangeable value of its annual produce."

the bulk of the gap between the IV and OLS estimates is simply sampling error." Instead, the systematic understatement of trade's positive effect on income by OLS regressions is more akin to an empirical regularity. Our results lend support to the conclusion that the downward bias of the OLS estimate is not due to sampling error, but rather to measurement error. One possible source of measurement error, and one that is economically interesting, is that trade imperfectly captures a variety of economically beneficial interactions between countries. However, like Rodriguez and Rodrik (2000), we also find that the Frankel-Romer results are not robust to the inclusion of another geographic variable, latitude (distance from the equator). Latitude negates the impact of trade on income in the OLS regressions except in 1985 and 1990, but negates the impact of trade in the 2SLS regressions in all years, raising the issue of measurement error once again.

This paper first sets out the empirical framework for evaluating the effects of trade on income. The paper then examines the difference between OLS and two-stage least squares (2SLS) estimation of trade on income using data from several periods: the pre-World War I period (1913), the interwar period (1928), the Great Depression (1938), the early postwar period (1954), and several years in the later post-war period (1964, 1975, 1985, 1990). These data span various episodes in which the relationship between trade and income could potentially differ from that in the late postwar period. During the interwar period and immediately following World War II, for example, many economic observers were skeptical about the benefits of openness to trade.[2]

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[2] In the midst of the Great Depression, some economic observers suspected that countries insulated from trade did not experience the full brunt of the depression, as when John Maynard Keynes (1971, p. 235) expressed his sympathy "with those who would minimize, rather than with those would maximize, economic entanglements between nations." Later, in the early postwar period, development economists such as Raúl Prebisch (1984, p. 179) "strongly

Based on a variant of the Frankel-Romer methodology, our findings are broadly consistent with but somewhat stronger than those of Frankel and Romer. In seven of eight periods, the 2SLS estimate of trade's impact on income is larger than the OLS estimate. In six of eight periods, the 2SLS estimate is statistically significant at the 5 percent level, in contrast to the marginal statistical significance of Frankel and Romer's IV estimate. In five of eight periods, we are able to reject the hypothesis that the OLS and 2SLS coefficients are equal. These results reinforce their finding that trade serves to increase a country's real income and that OLS estimates understate the true effect of trade on income.

While the Frankel-Romer methodology appears to yield consistent results across different sample periods, the results are not robust to the inclusion of latitude in the income regression. We discuss this finding below and, given that the economic interpretation of latitude (unlike distance) is not immediately evident, the extent to which it calls into question the trade and income link uncovered using the Frankel-Romer approach.

2. Empirical Framework

The key insight of the Frankel-Romer paper is that a country's geographic attributes, especially distance from one's trading partners, yield important information on the "natural" or "expected" volume of its trade with other countries. While highly correlated with trade, these attributes are not important determinants of country's income (except as they operate through trade), nor does a country's income affect these attributes. Therefore, these geographic factors can be used as exogenous instruments for identifying the impact of trade on income.

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criticized . . . the outworn idea of the international division of labor" and proposed more "inward-oriented" development strategies.

These geographic characteristics, however, are also important determinants of within country trade. That is, countries with a relative large population or area, such as the United States, tend to have smaller trade to GDP ratios than smaller countries, such as Finland, because there are more opportunities for within-country trade in larger countries. Therefore, country size must also be taken into account when evaluating the effect of trade on income. Opportunities for within country transactions cannot be measured using the same monetary metric as international trade, but they can be proxied by population and area.

We adopt the empirical procedure devised by Frankel and Romer with some minor alterations. First, a geographical prediction of countries' bilateral trade flows is estimated using a variant of the standard gravity equation (omitting the income variables). More formally, we estimate:

$$(1) \qquad \tau_{ij} \equiv \log(t_{ij}/GDP_i) =$$
$$a_0 + a_1 \log(distance_{ij}) + a_2 \log(pop_i) + a_3 \log(area_i) + a_4 \log(pop_j) + a_5 \log(area_j) + a_6 (border_{ij}) + a_7 (landlocked_i + landlocked_j) + e_{ij}.$$

This equation states that the value of bilateral trade between country i and country j (denoted t_{ij}) relative to country i's GDP is related to the distance between them, their respective areas and populations, whether or not they share a common border, and whether one or both of the countries is landlocked.[3]

An important data issue at this point is the reliability of reported bilateral trade between small and distant countries. If trade between some pair of countries is very small then it is often recorded as zero. However, a predicted τ_{ij} between such countries can be calculated on the

[3] Unlike Frankel and Romer, we do not include interaction terms with the border dummy as this simply introduces six new statistically insignificant variables and does not improve the equation's fit.

basis of the coefficients estimated in equation (1), thereby yielding n(n-1) predicted values of τ_{ij}, where n is the number of countries in the sample. In other words, the geographically predicted trade shares are also obtained for country pairs whose recorded trade is zero.[4]

Frankel and Romer take a slightly different approach. They calculate the predicted values of τ_{ij} not only for all pairs in the 62 sample countries, but also for another 88 countries for which data on bilateral trade is not available. This extended sample comprises essentially the whole world, so their aggregated trade share variable is a country's total trade with the outside world divided by its GDP. In the income regressions, they then use these predicted trade shares to instrument for the trade share variable from Penn World Tables. (This procedure could bias the coefficients of interest, if the gravity relation is systematically different for countries in the sample than for those that are added in the prediction stage.)

We do not follow this procedure because the binding constraint in our earlier sample periods is GDP data rather than trade data. Therefore we cannot extend our sample to other countries for which we lack bilateral trade data. Thus we do not use the overall openness variable (aggregate trade over GDP) but concentrate only on trade within our sample of countries. In any case, it should be remembered that we are only studying trade and income among sample countries and do not claim to say anything about the relationship between trade and income with nonsample countries, which includes many small, less developed countries. (Sample countries for each year are listed in the appendix.)

The instrument is constructed by aggregating the predicted values of country i's trade

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[4] For almost all countries in our samples, most bilateral trades with other sample countries are nonzero. The predicted trade shares for countries with zero recorded trade are very small as they are typically small countries that are quite distant from each other. The main exception is Egypt and Israel.

with countries j ($\hat{\tau}_{ij}$) over all j, yielding a predicted value of a country i's trade share with all sample countries j:[5]

(2) $\hat{T}_i = \sum_{i \neq j} \exp(\hat{\tau}_{ij})$.

The simplest way to evaluate the impact of trade on income is to run the OLS regression that explains a country's per capita income as dependent upon the trade ratio and the country's area and population:

(3) $\log(GDP_i/pop_i) = b_0 + b_1 (T_i) + b_2 \log(area_i) + b_3 \log(pop_i) + u_i$,

where T_i is the actual trade to GDP ratio (defined as $[1/GDP_i]\sum_{i \neq j} t_{ij}$) and where b_1 captures the contribution of trade on per capita income. However, there are reasons to suspect that the coefficient estimate of b_1 is biased. High income countries are better able to afford infrastructure that is conducive to trade (such as better ports and airports) and have more resources to overcome the informational search costs associated with trade. Open trade policies might also be correlated with other successful domestic economic policies that raise income, or alternatively poorer countries may be more likely to imposes taxes that discourage trade but are relatively simple to administer. In these cases, the estimated b_1 would be biased upward because of a positive correlation between u_i and T_i and IV would reduce the estimated effect of trade on income.

Alternatively, recorded trade could be subject to measurement error, in which case the

[5] Aggregation would appear to bias the level of estimated T_i because the error term is aggregated through the exponential function, so $E[\exp(e_{ij})] > 0$. But under a homoskedasticity assumption this just multiplies the expectation of the predicted shares by a constant. This is not a problem here because the purpose is simply to obtain an instrument that is correlated with the underlying "natural" trade share of a country. The mean value of the instrument does not matter for this purpose, and a casual comparison of predicted and actual trade shares would be misleading.

IV estimate would raise the estimated effect of trade on income. One type of measurement error is that trade serves as an imperfectly measured proxy for a host of beneficial transactions between countries, such as productivity-enhancing knowledge and R&D spillovers that are thought to be transmitted via trade flows as described by Helpman (1997). If these income-enhancing transactions are only partially captured by recorded trade flows, this measurement error would produce a downward biased estimate of b_1. It is impossible to distinguish between these two cases just from OLS results.

To overcome the endogeneity problem of trade, the above OLS estimation is replaced by an IV estimation, two stage least squares (2SLS) to be exact, where the geographical component of a country's trade (equation 2) is used to instrument for a country's actual trade share. A country's geographical features are not correlated with u_i and therefore (equation 2) gives an appropriate instrument for trade. In this case, the first stage regression is:

(4a) $T_i = c_0 + c_1 \, (\hat{T}_i) + c_2 \log(\text{area}_i) + c_3 \log(\text{pop}_i) + v_i,$

The predicted values from this equation, $\tilde{T}_i$, are then used to estimate the second stage regression:

(4b) $\log(\text{GDP}_i/\text{pop}_i) = d_0 + d_1 \, (\tilde{T}_i) + d_2 \log(\text{area}_i) + d_3 \log(\text{pop}_i) + w_i,$

The coefficient estimate of d_1 can then be compared to the b_1 estimate to obtain information about the direction and magnitude of the possible OLS bias. Other independent determinants of per capita income are not likely to be correlated with the instrument and are thus relegated to the error term.

3. Data and Empirical Results

The key data requirements are the values of bilateral trade and a measure of real income. Frankel and Romer use data from the International Monetary Fund's Direction of Trade Statistics for bilateral trade, a source which we also use for the years 1954, 1964, 1975, 1985, and 1990.[6] Bilateral trade flows for 1913 were compiled from various national sources similar to those used by Estevadeordal (1997) and for 1928 and 1938 are from the League of Nations's publication Network of World Trade (1942) as used by Eichengreen and Irwin (1995) for analyzing interwar trade flows. Frankel and Romer use the Penn World Tables for data on real GDP and population, which we also use for the years 1954, 1964, 1975, 1985, and 1990.[7] Maddison (1995) presents real GDP and population for a smaller sample of countries for the years 1913, 1928, and 1938. Pre-World War II area is available in the League of Nations (1927). The distance data for the years 1913, 1928, and 1938 is based on Linneman (1966), as was used in Eichengreen and Irwin (1995), and for the postwar years is the same data used by Frankel and Romer.

Table 1 reports the estimated results from equation 1. The findings are unsurprising: the bilateral trade share is decreasing in the distance between the two countries, in the size of the home country's population, and is increasing in the population of the other country (as there are more consumers for the home country's products). Beyond this, sharing a common border has a positive effect on trade and being landlocked has a negative effect on trade, although these

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6 Specifically, bilateral trade between countries i and j is measured as $M_{ij} + M_{ji}$, i.e. the sum of bilateral imports.

7 For a description of the data, see Summers and Heston (1991). We use the Mark 5.6 version of the data available from the NBER website. Frankel and Romer's population variable is actually the working population constructed from Penn World Tables. We use population, although when we used working population it made virtually no difference to the results.

effects are significant only half the time. These results are generally consistent across all years. Geographic characteristics alone explain about 30 to 40 percent of the variance of the log of the bilateral trade share.

We report two regressions for 1985, one using the full 62 country sample and another using a smaller 40 country sample that matches our samples for 1913, 1928, and 1938 as closely as possible. The results from the 62 country sample are almost identical to those reported by Frankel and Romer, despite the slight differences in estimation technique and in the variables. Table 2 reports the first stage regressions (equation 4a) that can be used to determine whether the geographic determinants of trade help explain the actual trade share beyond the information in country size and population. The message here is that the fitted trade share does provide information about the actual trade share even after controlling for population and area, although the predicted trade share in 1928 is not significant.

Table 3 presents the results of the income regressions (equation 3). Panel A presents the OLS results and panel B displays the 2SLS results. With the exception of 1938, the 2SLS estimates of the effect of trade on income are larger than the OLS estimates.[8] Frankel and Romer find that the IV coefficient is 2.3 times greater than the OLS estimate. We find that,

[8] As Frankel and Romer note in their footnote 15, the standard errors of the estimated coefficients in the gravity equations should also be accounted for in the 2SLS standard errors because the instrument is constructed from the aggregated fitted values from the gravity equations. This is done by using the delta-method and adding $\hat{V} = \hat{G}\hat{S}\hat{G}'$ to the usual 2SLS covariance matrix $\hat{Z}$, where $\hat{S}$ is the estimated covariance matrix of **a**, the estimated vector of coefficients of the gravity equation, and $\hat{G}$ is the gradient matrix $(d\hat{a} / d\hat{b})$. The revised standard errors are then obtained as the square roots of the diagonal elements of the matrix sum $\hat{Z} + \hat{V}$. This is best done numerically because the instrument is a nonlinear function of these coefficients. As in Frankel and Romer, this added error never increases the standard errors by more than 10 percent.

averaging all cases, the 2SLS estimate exceeds the OLS estimate by a factor of 3.0. As the Hausman test (regression version) indicates, we can reject the hypothesis that the 2SLS and OLS estimates are equal in every year except 1928, 1938, and 1990.

Frankel and Romer find that the OLS coefficient on the trade share is statistically significant but the IV coefficient is marginally significant for 1985. We also find that the OLS estimate is generally significant (in six of the eight years), but also that the 2SLS estimate is significant at the 5 percent level in five of the eight years . These are not the same cases; in 1913 the OLS coefficient on trade is insignificant while the 2SLS coefficient is significant, whereas in 1928 the OLS coefficient is significant while the 2SLS coefficient is not. In fact, the 2SLS coefficients are significant in all but the interwar years of 1928 and 1938.

Why are the interwar results so different from the pre-World War I and the post-World War II results? Possible explanations for 1938 are the easiest to offer. At that time countries were still recovering from the depths of the Great Depression, so the measures of real income in that year are (to widely varying degrees) well below their potential, full employment level. The disparity between actual and potential income in 1938 thus reflects the lingering effects of the severe business cycle, not international trade, but will confound attempts to identify the impact of trade on income in that year. Another interpretation, advanced by Eichengreen and Irwin (1995), is that tariff and especially non-tariff measures (quotas, import licensing etc.) which proliferated in the early 1930s attenuated the link between trade and income in that decade. These explanations, however, fail to account for the statistical insignificance of the 2SLS estimate in 1928. In this year the point 2SLS estimate, while imprecisely measured, is at least much higher than the OLS estimate, as in every other case except 1938.

As previously noted, there are several reasons why the 2SLS estimates of trade's impact on income could be expected to be lower than the OLS estimates. Yet the 2SLS coefficients are in fact higher in almost every year, and significantly so for most years. This consistency over time lends credibility to the Frankel-Romer finding. But because their IV result was on the margin of statistical significance, they concluded that sampling variation (by which they mean a chance positive correlation between the instrument and the residual) is "the most plausible explanation" for the result.

The uniformity and the greater precision of our results suggest that it is not chance sampling error at work, but perhaps their alternative explanation: that OLS is biased down because trade is subject to measurement error. The higher 2SLS coefficient could simply indicate that trade is consistently measured with error (without it being a proxy for other interactions) or that countries with poorly measured income also have poorly measure trade. An economically relevant, special case of measurement error is that trade is an imperfect proxy for a host of income-enhancing interactions with other countries. These interactions include trade in ideas and other technological spillovers that Helpman (1997) and others have identified as important.

This raises questions about the difference between our results for 1985 and Frankel and Romer's results. They found using samples of 150 and 98 countries that the IV estimate was marginally significant at conventional levels, whereas we found using samples of 62 and 40 countries that the 2SLS estimate was significant at conventional levels. The main difference between the samples is that Frankel and Romer used the gravity estimates to predicted values of trade for countries (presumably smaller countries) for which bilateral trade data was not available. This enabled them to expand their sample greatly, but including smaller countries with possibly noisier data appears to have reduced the precision of the estimates or diluted the effect of countries with more extensive bilateral trading partners. In contrast, our sample has a core group for which data on all variables exists even for periods early this century. In this sample group, the 2SLS estimates are quite precise. This group does not include oil exporting countries or small open economies such as Luxembourg, Hong Kong, or Singapore that might be idiosyncratic outliers and contribute disproportionately to the results.

One benefit to the 1985 data employed by Frankel and Romer is the ease of interpreting the coefficients, as both GDP per capita and the trade share are measured in 1985 dollars. Their estimates suggest a one percentage point increase in the trade share increases per capita income by 2 percent. In our case, we are unable to make such an informative statement because the trade shares are defined as the nominal value of trade within sample countries divided by real GDP in either 1992 dollars (in the Maddison data) or in 1985 dollars (in the Penn World Tables data).[9]

Frankel and Romer caution that these results are not immediately applicable to trade policy. Standard trade theory suggests that deviations from free trade could increase real income if optimal tariffs are imposed to improve the terms of trade or offset domestic distortions. In addition, as they note (p. 395), “differences in trade resulting from policy may not affect income in precisely the same way as differences resulting from geography.” Yet we share their view that these results are suggestive about policy, in particular that general policy measures that restrict international trade without having these corresponding benefits could result in significant losses to real income.

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[9] Note that if the predicted trade share with respect to sample countries was used to instrument for total trade share with the world, this would create serious bias, as countries whose natural trading partners are missing from the sample would appear to have abnormally high actual trade shares.

While the Frankel-Romer approach yields results that appear to be robust across time, another question is whether the results are robust to the inclusion of other geographic variables that could potentially enter into the income regression. Rodriguez and Rodrik (2000) have suggested that the Frankel-Romer results are sensitive to the inclusion of a country's latitude (distance from the equator). Table 4 shows that when included in the income regressions, latitude negates the significance of trade on income in the OLS regressions except in 1985 and 1990, but negates the significance of trade in the 2SLS regressions in all years.[10] The inclusion of latitude also makes the ratio of the OLS/2SLS estimates behave quite randomly, without a general tendency for the 2SLS estimate to exceed the OLS estimate.

One possible conclusion from these results is that, for much of the twentieth century, geography was destiny for most countries in that latitude largely determined income (see Theil and Galvez 1995). By the late twentieth century (i.e., from the 1980s), however, trade also became an important determinant of income in the OLS regressions, but the lower and statistically insignificant 2SLS coefficient on trade raises the issue of measurement error once again. However, using somewhat different control factors, Hall and Jones (1999) and Frankel and Rose (2000) find that the geographic-determined portion of trade remains a significant determinant of income in 1985 even after controlling for latitude. If one believes that latitude should be included in the income regression, the issue of how latitude and trade affect income may depend upon the period considered and other control factors.

Another view is that because geographic variables such as distance from trading partners have a natural economic interpretation (e.g., in terms of trading costs) in terms of explaining trade, these geographic factors belong in the trade equation but not in the income regression,

[10] The data for latitude are available at Chad Jones's web site: http://www.stanford.edu/~chadj

where they lack a clear economic interpretation. When latitude is excluded from the income regressions but included in the trade equations, we find (in results that we do not report) that latitude is statistically significant but does not ultimately affect the favorable impact of trade on income. Hall and Jones justify using distance from the equator in an income regression by arguing that it is a proxy for Western influence, but this rationale is not obvious. (Britain, for example, colonized many countries close to the equator, such as India and Kenya, as well as many countries more distant from the equator, such as Canada and Australia.)

Latitude is more plausibly a proxy for tropical conditions that imply something about factor endowments (such as arable land) or climatic conditions susceptible to diseases such as malaria, which some suggest have significant negative effects on economic growth. McCarthy, Wolf, and Wu (2000) find that malaria-prone countries suffer adverse growth effects, but that climate is not destiny: conditional on climate, investment in public health infrastructure can mitigate the consequences of malaria. If trade helps raise income, then it adds to the resources which can help overcome the adverse effects of climate. In any event, the precise role of latitude in determining economic performance is an issue for further research.

The question of whether trade raises a country's income should be distinguished from two distinct but related questions. The first concerns the relationship between trade and economic growth. The results here say nothing about the growth effects of trade. An increase in trade that raises the level of income could do so immediately without any growth effects or could result in transitional growth effects, whose measurement could prove difficult. Although there does not exist a strong theoretical presumption regarding trade and growth, an empirical literature has examined the links between the two. Levine and Renalt (1992) and Xavier Sala-i-Martin (1997) find that trade (usually measured by the trade to GDP ratio) is indirectly related to growth, although the endogeneity of trade is typically not controlled for. Levine and Renalt find that trade is robustly related to the investment share that in turn is related to growth (and finding that is consistent with Frankel and Romer's additional results), while Sala-i-Martin finds that trade is not related to growth directly, but is related to the Sachs and Warner (1985) measure of the number of years a country is considered an "open economy."

The second concerns the relationship between trade policy and economic growth. Identifying the impact of trade policy on growth is hampered by the problem of measuring trade policy (particularly reducing it to a single metric) and by the correlation of trade policy with other economic policies. Several studies, notably Sachs and Warner (1995), have used cross-country growth regressions and have found a positive relationship between lower trade barriers and more rapid economic growth in the postwar period. Rodriguez and Rodrik (2000) have raised questions about the robustness of these results and whether trade policy per se (as opposed to state export monopolies or region-specific factors) is responsible for them. Jones (2000), however, focused just on trade policy variables (such as the Sachs-Warner openness variable and the average tariff rate) and concluded that trade restrictions are almost invariably harmful to long-run growth, but that the magnitude of this effect is uncertain.

4. Conclusions

This paper has extended the Frankel and Romer (1999) methodology of assessing the impact of trade on income to a wide set of periods during the twentieth century. Our results support their main finding, that instrumenting for trade with a country's geographic characteristics raises the estimated positive impact of trade on income by a large magnitude. In contrast to their findings, our instrumented estimates are statistically significant, perhaps because we do not extend the gravity estimates to countries for which data on bilateral trade flows are lacking. However, our results are not robust to the inclusion of latitude in the income regression, although the economic interpretation of this geographic factor is unclear.

Table 1: Geographic Component of Bilateral Trade

Dependent Variable: Log of Bilateral Trade Share (t_{ij}/GDP_i)

	1913	1928	1938	1954	1964	1975	1985-A	1985-B	1990
Constant	-6.73* (1.01)	-4.04* (0.56)	-2.65* (0.53)	-12.30* (0.69)	-9.43* (0.48)	-5.91* (0.64)	-6.22* (0.41)	-4.58* (0.54)	-5.75* (0.57)
Log of Distance	-0.88* (0.09)	-0.59* (0.05)	-0.59* (0.05)	-0.75* (0.06)	-0.79* (0.04)	-1.04* (0.05)	-0.85* (0.04)	-0.87* (0.05)	-0.97* (0.05)
Log of Population i	-0.09 (0.06)	-0.42* (0.04)	-0.50* (0.04)	-0.19* (0.04)	-0.32* (0.03)	-0.24* (0.04)	-0.25* (0.03)	-0.28* (0.04)	-0.24* (0.04)
Log of Area i	-0.13* (0.05)	0.04 (0.03)	0.04 (0.03)	-0.07 (0.04)	-0.09* (0.02)	-0.14* (0.03)	-0.12* (0.02)	-0.13* (0.02)	-0.21* (0.03)
Log of Population j	0.82* (0.06)	0.47* (0.04)	0.37* (0.04)	0.58* (0.04)	0.56* (0.03)	0.54* (0.04)	0.60* (0.03)	0.39* (0.04)	0.55* (0.04)
Log of Area j	-0.20* (0.05)	-0.05 (0.03)	-0.03 (0.03)	0.02 (0.03)	-0.08* (0.02)	-0.13* (0.03)	-0.19* (0.02)	-0.08* (0.02)	-0.24* (0.03)
Border	0.70* (0.32)	0.45* (0.16)	0.06 (0.15)	0.29 (0.25)	0.32 (0.19)	0.32 (0.25)	0.76* (0.17)	0.75* (0.21)	0.77* (0.23)
Landlocked	--	-0.43* (0.10)	-0.46* (0.09)	-0.03 (0.13)	0.02 (0.08)	-0.10 (0.13)	-0.32* (0.08)	-0.54* (0.11)	-0.63* (0.09)
No. of Countries	36	41	41	42	63	52	62	40	52
N ($t_{ij} > 0$)	996	1131	1111	1356	2752	2502	3220	1520	2562
N ($t_{ij} = 0$)	264	509	529	408	1154	360	562	40	90
R^2	0.31	0.40	0.40	0.29	0.31	0.27	0.36	0.40	0.32
MSE	2.18	1.32	1.21	1.64	1.74	2.02	1.65	1.52	1.93

* indicates significant at the 5 percent level.

Table 2: Relationship Between Actual and Constructed Trade Share

Dependent Variable: Trade to GDP Ratio

	1913	1928	1938	1954	1964	1975	1985 - A	1985 - B	1990
Constant	0.51*	0.42	-0.14	2.49	5.12	22.53*	0.57	0.74	0.67*
	(0.20)	(0.53)	(0.20)	(1.68)	(3.04)	(6.96)	(0.39)	(0.40)	(0.22)
Constructed Trade Share	1.37*	1.65	2.24*	1.39*	1.70*	1.42*	2.09*	1.58*	1.57*
	(0.39)	(0.94)	(0.46)	(0.41)	(0.53)	(0.38)	(0.59)	(0.47)	(0.34)
Log of Population	-0.07*	-0.06	0.00	-0.51*	-0.10	-0.61	0.00	-0.02	-0.01
	(0.02)	(0.04)	(0.02)	(0.10)	(0.18)	(0.59)	(0.02)	(0.03)	(0.02)
Log of Area	0.04	0.03	0.01	0.25	-0.28	1.00*	-0.05	-0.04	-0.04*
	(0.03)	(0.03)	(0.01)	(0.11)	(0.18)	(0.45)	(0.03)	(0.03)	(0.02)
N	36	41	41	41	63	52	62	40	52
F-test	12.64*	3.06	24.40*	11.49*	10.28*	13.87*	12.15*	11.27*	21.43*
(p-value)	(0.001)	(0.09)	(0.00)	(0.002)	(0.002)	(0.00)	(0.00)	(0.002)	(0.00)
R^2	0.48	0.37	0.66	0.61	0.55	0.64	0.59	0.66	0.73
MSE	0.16	0.20	0.07	0.63	1.49	3.93	0.21	0.21	0.13

* indicates significance at the 5 percent level.

Note: The F statistic tests whether the constructed trade share coefficient is significantly different from zero.

Table 3: Determinants of Real National Income

Dependent Variable: Log of Per Capita GDP

A. OLS

	1913	1928	1938	1954	1964	1975	1985 - A	1985 - B	1990
Constant	8.44*	1.39*	1.23	6.82*	9.13*	8.04*	9.57*	10.70*	7.73*
	(0.97)	(0.85)	(0.96)	(1.87)	(1.43)	(1.66)	(1.04)	(1.00)	(1.32)
Trade Share	0.72	1.11*	2.43*	0.46*	0.08	0.07*	1.16*	0.72	2.89*
	(0.68)	(0.45)	(0.95)	(0.19)	(0.08)	(0.03)	(0.43)	(0.41)	(0.07)
Log of Population	-0.07	-0.05	-0.06	0.01	-0.16	-0.16	-0.15	-0.31*	-0.19
	(0.10)	(0.09)	(0.10)	(0.17)	(0.11)	(0.14)	(0.08)	(0.09)	(0.10)
Log of Area	-0.06	-0.05	-0.02	0.07	0.08	0.17	0.07	0.13	0.24
	(0.08)	(0.06)	(0.06)	(0.10)	(0.10)	(0.10)	(0.08)	(0.07)	(0.10)
N	36	41	41	42	63	52	62	40	52
R^2	0.15	0.30	0.32	0.20	0.08	0.17	0.23	0.36	0.36
MSE	0.65	0.56	0.54	0.85	0.97	0.92	0.78	0.59	0.76

Note: The coefficients on trade share are not comparable across years.

* indicates significant at the 5 percent level.

B. 2SLS

	1913	1928	1938	1954	1964	1975	1985 - A	1985 - B	1990
Constant	6.35* (1.60)	-0.11 (5.00)	1.47 (1.33)	1.39 (3.81)	2.59 (3.84)	3.51 (3.25)	6.01* (2.36)	8.45* (1.88)	5.78* (2.15)
Trade Share	3.15* (1.53)	2.13 (3.94)	2.10 (1.55)	1.21* (0.49)	0.55* (0.26)	0.18* (0.07)	3.20* (1.27)	1.96* (0.94)	4.22* (1.34)
Log of Population	0.04 (0.13)	0.06 (0.44)	-0.08 (0.12)	0.42 (0.31)	0.01 (0.16)	-0.06 (0.15)	-0.13 (0.10)	-0.26* (0.10)	-0.18 (0.14)
Log of Area	0.01 (0.10)	-0.06 (0.10)	-0.02 (0.06)	0.09 (0.12)	0.39 (0.21)	0.39* (0.17)	0.30 (0.16)	0.26* (0.12)	0.36* (0.14)
N	36	41	41	41	63	52	62	40	52
Hausman F (p-value)	5.70* (0.02)	0.43 (0.51)	0.07 (0.79)	5.11* (0.03)	7.10* (0.01)	4.02* (0.05)	4.83* (0.03)	2.93 (0.10)	1.63 (0.21)
MSE	0.77	0.60	0.54	1.00	1.23	1.03	0.91	0.91	0.79
Ratio of 2SLS/OLS Coefficients on Trade	4.0	1.9	0.9	2.6	7.3	2.5	2.8	2.7	1.5

Note: The coefficients on trade share are not comparable across years.

* indicates significant at the 5 percent level.

Table 4: Determinants of Real National Income

Dependent Variable: Log of Per Capita GDP

A. OLS

	1913	1928	1938	1954	1964	1975	1985 - A	1985 - B	1990
Constant	7.74* (0.86)	1.13 (0.81)	1.31 (0.89)	6.46* (1.49)	8.60* (1.62)	7.70* (1.33)	8.02* (0.87)	8.97* (0.93)	7.18* (1.06)
Trade Share	0.12 (0.61)	0.70 (0.46)	1.05 (1.02)	0.12 (0.17)	-0.02 (0.07)	0.04 (0.03)	1.16* (0.35)	0.99* (0.34)	2.07* (0.58)
Log of Population	-0.13 (0.09)	-0.12 (0.09)	-0.16 (0.10)	-0.20 (0.14)	-0.22 (0.09)	-0.12 (0.11)	-0.11 (0.07)	-0.21* (0.08)	-0.09 (0.09)
Log of Area	0.03 (0.07)	0.02 (0.07)	0.06 (0.06)	0.21* (0.08)	0.10 (0.08)	0.10 (0.08)	0.09 (0.06)	0.13* (0.06)	0.14 (0.08)
Distance from the Equator	0.024* (0.007)	0.015* (0.007)	0.018* (0.007)	0.035* (0.007)	0.035* (0.006)	0.033* (0.006)	0.028* (0.005)	0.021* (0.005)	0.028* (0.005)
N	36	41	41	41	63	52	62	40	52
R^2	0.39	0.39	0.43	0.51	0.41	0.48	0.52	0.58	0.60
MSE	0.56	0.53	0.50	0.67	0.79	0.74	0.62	0.48	0.61

Note: The coefficients on trade share are not comparable across years.

* indicates significant at the 5 percent level.

B. 2SLS

	1913	1928	1938	1954	1964	1975	1985 - A	1985 - B	1990
Constant	6.42	8.30	2.84	4.88*	9.65*	8.30*	9.00*	9.37*	7.91*
	(1.33)	(27.84)	(1.52)	(3.30)	(2.91)	(2.59)	(1.42)	(1.27)	(1.70)
Trade Share	2.00	-6.78	-1.59	0.40	-0.10	0.02	0.60	0.79	1.52
	(1.46)	(28.98)	(2.30)	(0.53)	(0.22)	(0.06)	(0.73)	(0.56)	(1.16)
Log of Population	-0.04	-0.99	-0.32	-0.05	-0.25*	-0.13	-0.11	-0.22*	-0.09
	(0.12)	(3.36)	(0.17)	(0.32)	(0.12)	(0.11)	(0.07)	(0.08)	(0.08)
Log of Area	0.05	0.27	0.08	0.20*	0.05	0.07	0.03	0.11	0.09
	(0.08)	(1.00)	(0.07)	(0.09)	(0.15)	(0.14)	(0.10)	(0.07)	(0.12)
Distance from Equator	0.018*	0.056	0.026*	0.030*	0.037*	0.034*	0.027*	0.021*	0.003*
	(0.009)	(0.162)	(0.010)	(0.010)	(0.008)	(0.008)	(0.005)	(0.005)	(0.006)
N	36	41	41	41	63	52	62	40	52
Hausman F	2.97	0.55	2.09	0.30	0.16	0.07	0.83	0.21	0.31
(p-value)	(0.10)	(0.46)	(0.16)	(0.58)	(0.69)	(0.24)	(0.36)	(0.64)	(0.58)
MSE	0.64	1.53	0.55	0.69	0.79	0.74	0.63	0.91	0.62
Ratio of 2SLS/OLS Coefficients on Trade	16.4	-9.7	-1.5	3.2	5.2	0.5	0.5	0.8	0.7

Note: The coefficients on trade share are not comparable across years.

* indicates significant at the 5 percent level.

Appendix: Sample Countries

	1913	1928	1938	1954	1964	1975	1985a	1985b	1990
ALGERIA							x		
ARGENTINA	x	x	x	x	x	x	x	x	x
AUSTRALIA	x	x	x	x	x	x	x	x	x
AUSTRIA	x	x	x	x	x	x	x	x	x
BELGIUM	x	x	x	x	x	x	x	x	x
BOLIVIA					x		x		
BRAZIL	x	x	x	x	x	x	x	x	x
BULGARIA	x	x	x						
CANADA	x	x	x	x	x	x	x	x	x
CHILE	x	x	x	x	x	x	x	x	x
CHINA	x	x	x		x		x	x	x
COLOMBIA	x	x	x	x	x	x	x	x	x
COSTA RICA					x				
CZECH-OSLOVAKIA		x	x		x				x
DENMARK	x	x	x	x	x	x	x	x	x
ECUADOR					x		x		
EGYPT	x			x	x	x	x		x
ETHIOPIA				x	x	x	x		
FINLAND	x	x	x	x	x	x	x	x	x
FRANCE	x	x	x	x	x	x	x	x	x
GERMANY*	x	x	x	x	x	x	x	x	x
GHANA					x	x	x		x
GREECE	x	x	x	x	x	x	x	x	x
HONG KONG					x	x	x	x	x
HUNGARY		x	x				x	x	
ICELAND					x		x		
INDIA	x	x	x	x	x	x	x	x	x
INDONESIA	x	x	x		x	x	x	x	x
IRAN					x	x	x		x
IRELAND		x	x	x	x	x	x		x
ISRAEL				x	x	x	x		x
ITALY	x	x	x	x	x	x	x	x	x
JAMAICA					x				
JAPAN	x	x	x	x	x	x	x	x	x
KENYA				x	x	x	x		x
KUWAIT							x		
MALAYSIA							x	x	
MEXICO	x	x	x	x	x	x	x	x	x
MOROCCO					x		x		
NETHERLANDS	x	x	x	x	x	x	x	x	x

NEW ZEALAND	x	x	x	x	x	x	x	x	x
NIGERIA				x	x	x	x		x
NORWAY	x	x	x	x	x	x	x	x	x
PAKISTAN				x	x	x	x		x
PANAMA					x				
PARAGUAY					x		x		
PERU	x	x	x	x	x	x	x		x
PHILIPPINES	x	x	x	x	x	x	x	x	x
POLAND		x	x				x	x	
PORTUGAL	x	x	x	x	x	x	x	x	x
ROMANIA		x	x		x	x			
SAUDI-ARABIA					x		x		
SINGAPORE						x	x	x	x
SOUTH AFRICA	x			x	x	x	x	x	x
SOUTH KOREA					x	x	x	x	x
SPAIN	x	x	x	x	x	x	x	x	x
SUDAN							x		
SWEDEN	x	x	x	x	x	x	x	x	x
SWITZERLAND	x	x	x	x	x	x	x	x	x
TAIWAN				x	x	x	x	x	x
TANZANIA					x	x			
THAILAND	x	x	x		x	x	x	x	x
TUNISIA					x	x	x		x
TURKEY	x	x	x	x	x	x	x	x	x
UGANDA				x	x	x			x
UK	x	x	x	x	x	x	x	x	x
URUGUAY				x	x	x	x		x
USA	x	x	x	x	x	x	x	x	x
USSR/RUSSIA	x	x	x						
VENEZUELA		x	x	x	x	x	x		x
YUGOSLAVIA		x	x		x	x	x	x	x
ZIMBABWE					x				x

* West Germany in post-war samples

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