Does Trade Cause Growth?

By JEFFREY A. FRANKEL AND DAVID ROMER*

Examining the correlation between trade and income cannot identify the direction of causation between the two. Countries' geographic characteristics, however, have important effects on trade, and are plausibly uncorrelated with other determinants of income. This paper therefore constructs measures of the geographic component of countries' trade, and uses those measures to obtain instrumental variables estimates of the effect of trade on income. The results provide no evidence that ordinary least-squares estimates overstate the effects of trade. Further, they suggest that trade has a quantitatively large and robust, though only moderately statistically significant, positive effect on income. (*JEL* F43, O40)

This paper is an empirical investigation of the impact of international trade on standards of living. From Adam Smith's discussion of specialization and the extent of the market, to the debates about import substitution versus export-led growth, to recent work on increasing returns and endogenous technological progress, economists interested in the determination of standards of living have also been interested in trade. But despite the great effort that has been devoted to studying the issue, there is little persuasive evidence concerning the effect of trade on income.

To see the basic difficulty in trying to estimate trade's impact on income, consider a cross-country regression of income per person on the ratio of exports or imports to GDP (and other variables). Such regressions typically find a moderate positive relationship.[1] But this relationship may not reflect an effect of trade on income. The problem is that the trade share may be endogenous: as Elhanan Helpman (1988), Colin Bradford, Jr. and Naomi Chakwin (1993), Rodrik (1995a), and many others observe, countries whose incomes are high for reasons other than trade may trade more.

Using measures of countries' trade policies in place of (or as an instrument for) the trade share in the regression does not solve the problem.[2] For example, countries that adopt free-market trade policies may also adopt free-market domestic policies and stable fiscal and monetary policies. Since these policies are also likely to affect income, countries' trade policies are likely to be correlated with factors that are omitted from the income equation. Thus they cannot be used to identify the impact of trade (Xavier Sala-i-Martin, 1991).

This paper proposes an alternative instrument for trade. As the literature on the gravity model of trade demonstrates, geography is a powerful determinant of bilateral trade (see, for example, Hans Linneman, 1966, Frankel et al., 1995, and Frankel, 1997). And as we show in this paper, the same is true for countries' overall trade: simply knowing how far a country is from other countries provides considerable information

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[1] See, for example, Michael Michaely (1977), Gershon Feder (1983), Roger C. Kormendi and Philip G. Meguire (1985), Stanley Fischer (1991, 1993), David Dollar (1992), Ross Levine and David Renelt (1992), Sebastian Edwards (1993), Ann Harrison (1996), and the ordinary least-squares regressions in Section II of this paper. Edwards (1995) and Dani Rodrik (1995b) survey the literature.

[2] For examples of this approach, see J. Bradford De Long and Lawrence H. Summers (1991), Fischer (1991, 1993), Dollar (1992), William Easterly (1993), Edwards (1993), Jong-Wha Lee (1993), Jeffrey D. Sachs and Andrew Warner (1995), and Harrison (1996).

about the amount that it trades. For example, the fact that New Zealand is far from most other countries reduces its trade; the fact that Belgium is close to many of the world's most populous countries increases its trade.

Equally important, countries' geographic characteristics are not affected by their incomes, or by government policies and other factors that influence income. More generally, it is difficult to think of reasons that a country's geographic characteristics could have important effects on its income except through their impact on trade. Thus, countries' geographic characteristics can be used to obtain instrumental variables estimates of trade's impact on income. That is the goal of this paper.[3]

The remainder of the paper contains two main sections. The first describes the impact of geographic characteristics on trade in more detail and uses geographic variables to construct an instrument for international trade. As we discuss there, there is one important complication to our basic argument that geographic variables can be used to construct an instrument for international trade in a cross-country income regression. Just as a country's income may be influenced by the amount its residents trade with foreigners, it may also be influenced by the amount its residents trade with one another. And just as geography is an important determinant of international trade, it is also an important determinant of within-country trade. In particular, residents of larger countries tend to engage in more trade with their fellow citizens simply because there are more fellow citizens to trade with. For example, Germans almost surely trade more with Germans than Belgians do with Belgians. This suggests a second geography-based test of the impact of trade on income: we can test whether within-country trade raises income by asking whether larger countries have higher incomes.

The reason that this issue complicates our test of the impact of international trade is that country size and proximity to other countries are negatively correlated. Because Germany is larger than Belgium, the average German is farther from other countries than the average Belgian is. Thus in using proximity to estimate international trade's effect on income, it is necessary to control for country size. Similarly, in using country size to test whether within-country trade raises income, it is necessary to control for international trade.

To construct the instrument for international trade, we first estimate a bilateral trade equation and then aggregate the fitted values of the equation to estimate a geographic component of countries' overall trade. In contrast to conventional gravity equations for bilateral trade, our trade equation includes only geographic characteristics: countries' sizes, their distances from one another, whether they share a border, and whether they are landlocked. This ensures that the instrument depends only on countries' geographic characteristics, not on their incomes or actual trading patterns. We find that these geographic characteristics are important determinants of countries' overall trade.

The second main section of the paper employs the instrument to investigate the impact of trade on income. We estimate cross-country regressions of income per person on international trade and country size by instrumental variables (IV), and compare the results with ordinary least-squares (OLS) estimates of the same equations. There are five main findings.

First, we find no evidence that the positive association between international trade and income arises because countries whose incomes are high for other reasons engage in more trade. On the contrary, in every specification we consider, the IV estimate of the effect of trade is larger than the OLS estimate, often by a considerable margin. Section II, subsection E, investigates possible reasons that the IV estimate exceeds the OLS one.

Second, the point estimates suggest that the impact of trade is substantial. In a typical specification, the estimates imply that increasing the ratio of trade to GDP by one percentage point

[3] Lee (1993) also uses information on countries' distances from one another to construct a measure of their propensity to trade. His approach differs from ours in two major respects. First, his measure is based not only on countries' geographic characteristics, but also on their actual trade patterns; thus it is potentially correlated with other determinants of income. Second, he does not investigate the relationship between income and his measure of the propensity to trade, but only the relationship between income and the interaction of his measure with indicators of distortionary trade policies.

raises income per person by between one-half and two percent.

Third, the estimates also imply that increased size raises income. This supports the hypothesis that greater within-country trade raises income.

Fourth, the large estimated positive effects of trade and size are robust to changes in specification, sample, and construction of the instrument.

Fifth, the impacts of trade and size are not estimated very precisely. The null hypothesis that these variables have no effect is typically only marginally rejected at conventional levels. As a result, the estimates still leave considerable uncertainty about the magnitudes of their effects.

I. Constructing the Instrument

A. *Background*

Our basic ideas can be described using a simple three-equation model. First, average income in country i is a function of economic interactions with other countries ("international trade" for short), economic interactions within the country ("within-country trade"), and other factors:

$$\ln Y_i = \alpha + \beta T_i + \gamma W_i + \varepsilon_i. \tag{1}$$

Here Y_i is income per person, T_i is international trade, W_i is within-country trade, and ε_i reflects other influences on income. As the vast literature on trade describes, there are many channels through which trade can affect income—notably specialization according to comparative advantage, exploitation of increasing returns from larger markets, exchange of ideas through communication and travel, and spread of technology through investment and exposure to new goods. Because proximity promotes all of these types of interactions, our approach cannot identify the specific mechanisms through which trade affects income.

The other two equations concern the determinants of international and within-country trade. International trade is a function of a country's proximity to other countries, P_i, and other factors:

$$T_i = \psi + \phi P_i + \delta_i. \tag{2}$$

Similarly, within-country trade is a function of the country's size, S_i, and other factors:

$$W_i = \eta + \lambda S_i + \nu_i. \tag{3}$$

The residuals in these three equations, ε_i, δ_i, and ν_i, are likely to be correlated. For example, countries with good transportation systems, or with government policies that promote competition and reliance on markets to allocate resources, are likely to have high international and within-country trade given their geographic characteristics, and high incomes given their trade.

The key identifying assumption of our analysis is that countries' geographic characteristics (their P_i's and S_i's) are uncorrelated with the residuals in equations (1) and (3). Proximity and size are not affected by income or by other factors, such as government policies, that affect income. And as we observe in the introduction, it is difficult to think of important ways that proximity and size might affect income other than through their impact on how much a country's residents interact with foreigners and with one another.

Given the assumption that P and S are uncorrelated with ε, data on Y, T, W, P, and S would allow us to estimate equation (1) by instrumental variables: P and S are correlated with T and W [by (2) and (3)], and are uncorrelated with ε (by our identifying assumption). Unfortunately, however, there are no data on within-country trade. Ideally, we would want data comparable to measures of international trade. That is, we would want a measure of the value of the exchange of all goods and services among individuals within a country, both across and within firms. This measure would probably be many times GDP for most countries. But no such measure exists.

To address this problem, we substitute (3) into (1) to obtain

$$\begin{aligned} \ln Y_i &= \alpha + \beta T_i + \gamma(\eta + \lambda S_i + \nu_i) + \varepsilon_i \\ &= (\alpha + \gamma\eta) + \beta T_i + \gamma\lambda S_i \\ &\quad + (\gamma\nu_i + \varepsilon_i). \end{aligned} \tag{4}$$

Our identifying assumption implies that P_i and S_i are uncorrelated with the composite residual, $\gamma\nu_i + \varepsilon_i$. Thus (4) can be estimated by instrumental variables, with P_i and S_i (and the constant) as the instruments.

Note that estimation of (4) yields an estimate not only of β, international trade's impact on income, but also of $\gamma\lambda$, country size's impact on income. Since the two components of this coefficient are not identified separately, one cannot obtain an estimate of γ, the effect of within-country trade on income. But as long as λ is positive—that is, as long as larger countries have more within-country trade—the sign of γ is the same as the sign of $\gamma\lambda$. Thus, although we cannot estimate the magnitude of the impact of within-country trade on income, we can obtain evidence about its sign.

As we argue in the introduction (and verify below), P_i and S_i are negatively correlated: the larger a country is, the farther its typical resident is from other countries. Thus if we do not control for size in (4), P_i will be negatively correlated with the residual, and thus will not be a valid instrument. Intuitively, smaller countries may engage in more trade with other countries simply because they engage in less within-country trade. This portion of the geographic variation in international trade cannot be used to identify trade's impact on income. Similarly, if we fail to control for T_i in (4), S_i will be negatively correlated with the residual. Thus, our approach requires us to examine the impacts of both international trade and country size.

To estimate (4), we need data on four variables: income (Y), international trade (T), size (S), and proximity (P). We measure the first three in the usual ways. Our measure of income is real income per person. Following standard practice, we measure international trade as imports plus exports divided by GDP. This is clearly an imperfect measure of economic interactions with other countries, an issue we return to in Section II, subsection E.[4] Finally, theory provides little guidance about the best measure of size. We therefore use two natural measures, population and area, both in logs. In interpreting the results concerning size, we focus on the sum of the coefficients on log population and log area. Thus we consider the impact of an increase in population and area together, with no change in population density. Such a change clearly increases the scope for within-country trade.[5]

To measure proximity, we need an appropriate weighted average of distance or ease of exchange between a given country and every other country in the world. To choose the weights to put on the different counties, we begin by estimating an equation for bilateral trade as a function of distance, size, and so on. We then use the estimated equation to find fitted values of trade between countries i and j as a share of i's GDP. Finally, we aggregate over j to obtain a geographic component of country i's total trade, T_i. It is this geographic component of T_i that we use as our measure of proximity. The remainder of this section describes the specifics of how we do this.

B. *The Bilateral Trade Equation*

Work on the gravity model of bilateral trade shows that trade between two countries is negatively related to the distance between them and positively related to their sizes, and that a log-linear specification characterizes the data fairly well. Thus, a minimal specification of the bilateral trade equation is

$$(5)\quad \ln(\tau_{ij}/\mathrm{GDP}_i) = a_0 + a_1 \ln D_{ij} + a_2 \ln S_i + a_3 \ln S_j + e_{ij},$$

where τ_{ij} is bilateral trade between countries i and j (measured as exports plus imports), D_{ij} is the distance between them, and S_i and S_j are measures of their sizes.

This specification omits a considerable amount of geographic information about trade. The equation that we estimate therefore differs from (5) in

[4] Using the log of the trade share instead of the level does not affect our conclusions. Examining import and export shares separately yields, not surprisingly, the result that the geographic components of the two shares are almost identical. Thus our approach cannot separate the effects of imports and exports.

[5] Throughout the paper, we use the labor force as our measure of population in computing income per person and in measuring country size. Using total population instead makes little difference to the results.

three ways. First, as described above, we include two measures of size: log population and log area. Second, whether countries are landlocked and whether they have a common border have important effects on trade; we therefore include dummy variables for these factors. And third, a large part of countries' trade is with their immediate neighbors. Since our goal is to identify geographic influences on overall trade, we therefore include interaction terms of all of the variables with the common-border dummy.

The fact that we are measuring trade relative to country i's GDP means that we are already including a measure of i's size. We therefore do not constrain the coefficients on the population measures for the two countries, or the coefficients on the area measures, to be equal. We do, however, constrain the coefficients on the landlocked dummies, and their interactions with the common-border dummy, to be equal for i and j.[6] Thus, the equation we estimate is

$$
\begin{aligned}
(6)\quad \ln(\tau_{ij}/\mathrm{GDP}_i) &= a_0 + a_1 \ln D_{ij} + a_2 \ln N_i + a_3 \ln A_i \\
&\quad + a_4 \ln N_j + a_5 \ln A_j + a_6(L_i + L_j) \\
&\quad + a_7 B_{ij} + a_8 B_{ij} \ln D_{ij} + a_9 B_{ij} \ln N_i \\
&\quad + a_{10} B_{ij} \ln A_i + a_{11} B_{ij} \ln N_j \\
&\quad + a_{12} B_{ij} \ln A_j + a_{13} B_{ij}(L_i + L_j) + e_{ij},
\end{aligned}
$$

where N is population, A is area, L is a dummy for landlocked countries, and B is a dummy for a common border between two countries.

C. *Data and Results*

We use the same bilateral trade data as Frankel et al. (1995) and Frankel (1997); the data are originally from the IFS Direction of Trade statistics. They are for 1985, and cover trade among 63 countries. Following these papers, we drop observations where recorded bilateral trade is zero. Distance is measured as the great-circle distance between countries' principal cities. The information on areas, common borders, and landlocked countries is from Rand McNally (1993). Finally, the data on population are from the Penn World Table.[7]

The results are shown in Table 1. The first column shows the estimated coefficients and standard errors on the variables other than the common-border dummy and its interactions. These estimates are shown in the second column.[8]

The results are generally as expected. Distance has a large and overwhelmingly significant negative impact on bilateral trade; the estimated elasticity of trade with respect to distance is slightly less (in absolute value) than −1. Trade between country i and country j is strongly increasing in j's size; the elasticity with respect to j's population is about 0.6. In addition, trade (as a fraction of i's GDP) is decreasing in i's size and in j's area. And if one of the countries is landlocked, trade falls by about a third.

Because only a small fraction of country pairs share a border, the coefficients on the common-border variables are not estimated precisely. Nonetheless, the point estimates imply that sharing a border has a considerable effect on trade. Evaluated at the mean value of the variables conditional on sharing a border, the estimates imply that a common border raises trade by a factor of 2.2. The estimates also imply that the presence of a common border alters the effects of the other variables substantially. For example, the estimated elasticity with respect to country j's population across a shared border is 0.47 rather than 0.61, and the estimated elasticity with respect to distance is −0.70 rather than −0.85.

Most importantly, the regression confirms

[6] Allowing them to differ changes the results only trivially.

[7] We use Mark 5.6 of the table, which is distributed by the National Bureau of Economic Research. This is an updated version of the data described in Robert Summers and Alan Heston (1991). The capital city is used as the principal city, except for a small number of cases where the capital is far from the center of the country (in terms of population). In these cases, a more centrally located large city is chosen. For the United States, for example, Chicago rather than Washington is used as the principal city.

[8] The coefficient on the common-border variable itself is therefore shown as the coefficient on the interaction of the common-border dummy with the constant.

TABLE 1—THE BILATERAL TRADE EQUATION

	Variable	Interaction
Constant	−6.38	5.10
	(0.42)	(1.78)
Ln distance	−0.85	0.15
	(0.04)	(0.30)
Ln population	−0.24	−0.29
(country i)	(0.03)	(0.18)
Ln area	−0.12	−0.06
(country i)	(0.02)	(0.15)
Ln population	0.61	−0.14
(country j)	(0.03)	(0.18)
Ln area	−0.19	−0.07
(country j)	(0.02)	(0.15)
Landlocked	−0.36	0.33
	(0.08)	(0.33)
Sample size	3220	
R^2	0.36	
SE of regression	1.64	

Notes: The dependent variable is $\ln(\tau_{ij}/\text{GDP}_i)$. The first column reports the coefficient on the variable listed, and the second column reports the coefficient on the variable's interaction with the common-border dummy. Standard errors are in parentheses.

that geographic variables are major determinants of bilateral trade. The R^2 of the regression is 0.36. The next step is to aggregate across countries and see if geographic variables are also important to overall trade.[9]

D. *Implications for Aggregate Trade*

To find the implications of our estimates for the geographic component of countries' overall trade, we aggregate the fitted values from the bilateral trade equation. That is, we first rewrite equation (6) as

$$\ln(\tau_{ij}/\text{GDP}_i) = \mathbf{a}'\mathbf{X}_{ij} + e_{ij}, \tag{7}$$

where $\mathbf{a}$ is the vector of coefficients in (6) (a_0, a_1, ..., a_{13}), and $\mathbf{X}_{ij}$ is the vector of right-hand side variables ($1, \ln D_{ij}, ..., B_{ij}[L_i + L_j]$). Our estimate of the geographic component of country i's overall trade share is then

$$\hat{T}_i = \sum_{j \neq i} e^{\hat{\mathbf{a}}'\mathbf{X}_{ij}}. \tag{8}$$

That is, our estimate of the geographic component of country i's trade is the sum of the estimated geographic components of its bilateral trade with each other country in the world.[10]

All that is needed to perform the calculations in equation (8) are countries' populations and geographic characteristics. We therefore take the sum in (8) not just over the countries covered by the bilateral trade data set, but over all countries in the world.[11] Similarly, we are able to find the constructed trade share, $\hat{T}$, for all countries, not just those for which we have bilateral trade data. Since our income regressions will also require data on trade and income, however, we limit our calculation of $\hat{T}$ to the countries in the Penn World Table. Thus we compute $\hat{T}$ for 150 countries.

E. *The Quality of the Instrument*

Figure 1 is a scatterplot of the true overall trade share, T, against the constructed share, $\hat{T}$. The figure shows that geographic variables account for a major part of the variation in overall trade. The correlation between T and $\hat{T}$ is 0.62. As column (1) of Table 2 shows, a regression of

[9] The standard errors reported in Table 1 are conventional OLS standard errors. It is likely that the residuals of the bilateral trade equation are not completely independent, and thus that the reported standard errors are too low. But as described in Section II, subsection B, uncertainty about the parameters of the bilateral trade equation contributes only a small amount to the standard errors of the cross-country income regressions that we ultimately estimate. For example, doubling the variance-covariance matrix of the estimated parameters of the bilateral trade equation increases the standard error of the coefficient on the trade share in our baseline cross-country regression [column (2) of Table 3] by less than 10 percent.

[10] The expectation of τ_{ij}/GDP_i conditional on $\mathbf{X}_{ij}$ is actually equal to $e^{\hat{\mathbf{a}}'\mathbf{X}_{ij}}$ times $E[e^{e_{ij}}]$. Since we are modeling e_{ij} as homoskedastic, however, $E[e^{e_{ij}}]$ is the same for all observations, and thus multiplies $\hat{T}_i$ by a constant. This has no implications for the subsequent analysis, and is therefore omitted for simplicity.

[11] For convenience, we omit a handful of countries with populations less than 100,000: Antigua and Barbuda, Greenland, Kiribati, Liechtenstein, the Marshall Islands, Micronesia, Monaco, Nauru, St. Kitts and Nevis, and San Marino. In addition, for countries that are not in the Penn World Table, we have data on population but not on the labor force. To estimate the labor force for these countries, we multiply their populations by the average ratio of the labor force to population among the countries in the same continent that are in the Penn World Table. We use the Penn World Table's definitions of the continents.

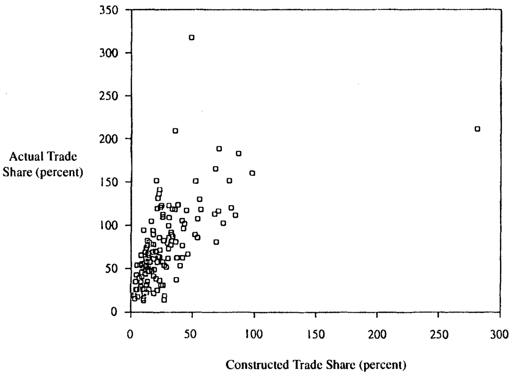


FIGURE 1. ACTUAL VERSUS CONSTRUCTED TRADE SHARE

TABLE 2—THE RELATION BETWEEN ACTUAL AND CONSTRUCTED OVERALL TRADE

	(1)	(2)	(3)
Constant	46.41	218.58	166.97
	(4.10)	(12.89)	(18.88)
Constructed trade share	0.99		0.45
	(0.10)		(0.12)
Ln population		−6.36	−4.72
		(2.09)	(2.06)
Ln area		−8.93	−6.45
		(1.70)	(1.77)
Sample size	150	150	150
R^2	0.38	0.48	0.52
SE of regression	36.33	33.49	32.19

Notes: The dependent variable is the actual trade share. Standard errors are in parentheses.

T on a constant and $\hat{T}$ yields a coefficient on $\hat{T}$ of essentially one and a t-statistic of 9.5.

As described in subsection A of this section, however, the component of the constructed trade share that is correlated with country size cannot be used to estimate trade's impact on income: smaller countries may engage in more international trade but in less within-country trade. That is, our identification of trade's impact on income will come from the component of the excluded exogenous variable (the constructed trade share) that is uncorrelated with the other exogenous variables (the size measures).

The constructed trade share is in fact highly correlated with country size. For example, the five countries with the smallest constructed shares all have areas over 1,000,000 square miles, and the five with the largest constructed shares all have areas under 10,000 square miles. A regression of the constructed trade share on a constant, log population, and log area yields negative and significant coefficients on both size measures and an R^2 of 0.45.

Thus in examining whether geographic variables provide useful information about international trade, we need to ask whether they provide information beyond that contained in country size. Columns (2) and (3) of Table 2 therefore compare a regression of the actual trade share on a constant and the two size measures with a regression that also includes our constructed trade share. As expected, size has a negative effect on trade. Area is highly significant, while population is moderately so. The coefficient on the constructed trade share falls by slightly more than half when the size controls are added.

The important message of columns (2) and (3), however, is that the constructed trade share still contains a considerable amount of information about actual trade. For example, its t-statistic in column (3) is 3.6; this corresponds to an F-statistic of 13.1. As the results in the next section show, this means that the constructed trade share contains enough information about actual trade for IV estimation to produce only moderate standard errors for the estimated impact of trade. Furthermore, the results of Douglas Staiger and James H. Stock (1997), Charles R. Nelson and Richard Startz (1990), and Alastair R. Hall et al. (1996) imply that these first-stage F-statistics are large enough that the finite-sample bias of instrumental variables—which biases the IV estimate toward the OLS estimate—is unlikely to be a serious problem in our IV regressions.

Figure 2, Panel A, shows the partial association between the actual and constructed trade shares controlling for the size measures. The figure shows that although the relationship is not as strong as the simple relationship shown in Figure 1, it is still positive. The figure also shows that there are two large outliers in the relationship: Luxembourg, which has an extremely high fitted trade share given its size, and Singapore, which has an extremely high actual trade share given its size. Figure 2, Panel B, therefore shows the scatterplot with these two observations omitted. Again there is a definite positive relationship.[12]

[12] When these two observations are dropped from the regression in column (3) of Table 2, the coefficient on the constructed trade share rises to 0.69, but the t-statistic falls

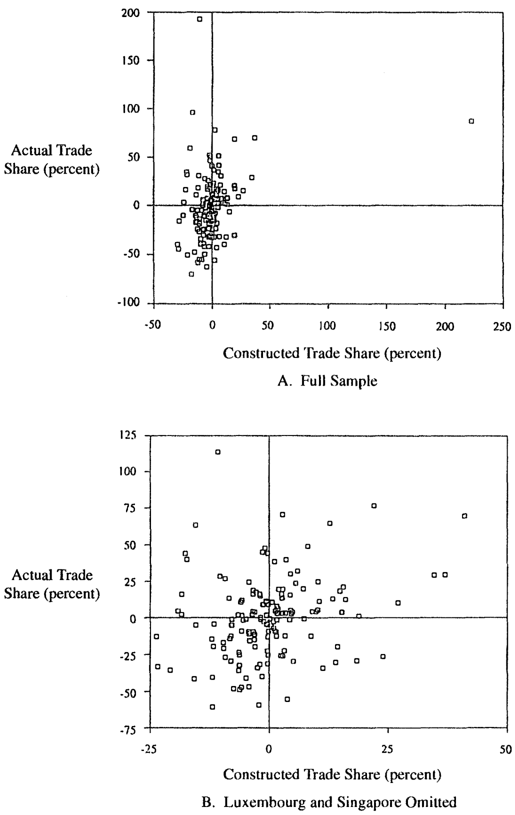


FIGURE 2. PARTIAL ASSOCIATION BETWEEN ACTUAL AND CONSTRUCTED TRADE SHARE

The five countries with the smallest constructed trade shares controlling for size are Western Samoa, Tonga, Fiji, Mauritius, and Vanuatu. All five are geographically isolated islands. Of the five, Western Samoa and Tonga have large negative shares given their size; Fiji and Mauritius have moderate negative shares; and Vanuatu has a moderate positive share. At the other extreme, the five countries with the highest constructed trade shares controlling for size are Luxembourg, Belize, Jordan, Malta, and Djibouti. Of these five, Luxembourg and Jordan have large positive actual trade shares controlling for size, and the other three have moderate positive shares. In sum, countries' geographic characteristics do provide considerable information about their overall trade.

II. Estimates of Trade's Effect on Income

A. *Specifications and Samples*

This section uses the instrument constructed in Section I to investigate the relationship between trade and income. The dependent variable in our regressions is log income per person. The data are for 1985 and are from the Penn World Table. Our basic specification is

$$\ln Y_i = a + bT_i + c_1 \ln N_i + c_2 \ln A_i + u_i, \tag{9}$$

where Y_i is income per person in country i, T_i is the trade share, and N_i and A_i are population and area. This specification differs from the one derived from the simple model in Section I, subsection A, only by including two measures of size rather than one [see equation (4)].

We do not include any additional variables in the regression. There are of course many other factors that may affect income. But our argument about the appropriateness of using geographic characteristics to construct an instrument for trade implies that there is no strong reason to expect additional independent determinants of income to be correlated with our instrument; thus they can be included in the error term. In addition, if we included other variables, the estimates of trade's impact on income would leave out any effects operating through its impact on these variables. Suppose, for example, that increased trade raises the rate of return on domestic investment and therefore increases the saving rate. Then by including the saving rate in the regression, we would be omitting trade's impact on income that operates via saving.

As an alternative to including control variables, in subsection D of this section, we decompose countries' incomes in various ways and ask how trade affects each component. In doing so, we can obtain information about the channels through which trade affects income. For example, we ask how trade affects both income at the beginning of the sample and growth

to 3.2 (corresponding to an F-statistic of 10.1). This F-statistic remains large enough that the finite-sample bias of the IV estimates is still likely to be small.

over the sample period.[13]

Appendix Table A1 reports the basic data used in the tests. It lists, for each country in the sample, its actual trade share in 1985, its constructed trade share, its area and 1985 population, and its income per person in 1985.[14]

We focus on two samples. The first is the full set of 150 countries covered by the Penn World Table. Our instrument is only moderately correlated with trade once we control for size, and much of the variation is among the smallest countries in the sample. Thus it is important to consider a relatively broad sample. And as we describe below, the results for this sample are robust to the exclusion of outliers and of observations where the data are potentially the most subject to error.

Our second sample is the 98-country sample considered by N. Gregory Mankiw et al. (1992). The countries in this sample generally have more reliable data; they are also generally larger, and thus less likely to have their incomes determined by idiosyncratic factors. In addition, data limitations require that we employ a smaller sample when we examine the channels through which trade affects income.

B. *Basic Results*

Table 3 reports the regressions. Column (1) is an OLS regression of log income per person on a constant, the trade share, and the two size measures. The regression shows a statistically and economically significant relationship between trade and income. The t-statistic on the trade share is 3.5; the point estimate implies that an increase in the share of one percentage point is associated with an increase of 0.9 percent in income per person. The regression also suggests that, controlling for international trade, there is a positive (though only marginally significant) relation between country size and income per person; this supports the view that within-country trade is beneficial. The point estimates imply that increasing both population and area by one percent raises income per person by 0.1 percent.

TABLE 3—TRADE AND INCOME

	(1)	(2)	(3)	(4)
Estimation	OLS	IV	OLS	IV
Constant	7.40	4.96	6.95	1.62
	(0.66)	(2.20)	(1.12)	(3.85)
Trade share	0.85	1.97	0.82	2.96
	(0.25)	(0.99)	(0.32)	(1.49)
Ln population	0.12	0.19	0.21	0.35
	(0.06)	(0.09)	(0.10)	(0.15)
Ln area	−0.01	0.09	−0.05	0.20
	(0.06)	(0.10)	(0.08)	(0.19)
Sample size	150	150	98	98
R^2	0.09	0.09	0.11	0.09
SE of regression	1.00	1.06	1.04	1.27
First-stage F on excluded instrument		13.13		8.45

Notes: The dependent variable is log income per person in 1985. The 150-country sample includes all countries for which the data are available; the 98-country sample includes only the countries considered by Mankiw et al. (1992). Standard errors are in parentheses.

Column (2) reports the IV estimates of the same equation. The trade share is treated as endogenous, and the constructed trade share is used as an instrument.[15] The coefficient on trade rises sharply. That is, the point estimate suggests that examining the link between trade and income using OLS understates rather than overstates the effect of trade. The estimates now imply that a one-percentage-point increase in the trade share raises income per person by 2.0 percent. In addition, the hypothesis that the IV coefficient is zero is marginally rejected at conventional levels (t = 2.0). The coefficient is

[13] Fischer (1993) uses a similar approach to investigate the effects of inflation. Frankel et al. (1996) and the working paper version of this paper (Frankel and Romer, 1996) investigate the effects of controlling for physical and human capital accumulation and population growth, and find that this does not change the character of the results.

[14] The other data used in the analysis are available from the authors on request.

[15] Throughout, the standard errors for the IV regressions account for the fact that the instrument depends on the parameters of the bilateral trade equation. That is, the variance-covariance matrix of the coefficients is estimated as the usual IV formula plus $(\partial\hat{\mathbf{b}}/\partial\hat{\mathbf{a}})\hat{\mathbf{\Omega}}(\partial\hat{\mathbf{b}}/\partial\hat{\mathbf{a}})'$, where $\hat{\mathbf{b}}$ is the vector of estimated coefficients from the cross-country income regression, $\hat{\mathbf{a}}$ is the vector of estimated coefficients from the bilateral trade equation, and $\hat{\mathbf{\Omega}}$ is the estimated variance-covariance matrix of $\hat{\mathbf{a}}$. In all cases, this additional term makes only a small contribution to the standard errors. In the regression in column (2) of Table 3, for example, this correction increases the standard error on the trade share from 0.91 to 0.99.

much less precisely estimated under IV than under OLS, however. As a result, the hypothesis that the IV and OLS estimates are equal cannot be rejected ($t = 1.2$).[16]

Moving from OLS to IV also increases the estimated impact of country size. The estimated effect of raising both population and area by one percent is now to increase income per person by almost 0.3 percent. This estimate is marginally significantly different from zero ($t = 1.8$).

One interesting aspect of the results concerning size is that the coefficient on area is positive. One might expect increased area, controlling for population, to reduce within-country trade and thus lower income. One possibility is that the positive coefficient is due to sampling error: the t-statistic on area is slightly less than one. Another is that greater area has a negative impact via decreased within-country trade, but a larger positive impact via increased natural resources. It is because of this possibility that we focus on the sum of the coefficients on log population and log area in our discussion. As described above, this sum shows the effects of increased size with population density held constant. In addition, as we show below, using population alone to measure size has no major impact on the results.

Columns (3) and (4) consider the 98-country sample. This change has no great effect on the OLS estimates of the coefficients on trade and size, but raises both the IV estimates and their standard errors considerably. The t-statistics for the OLS estimates fall moderately, while those for the IV estimates change little.

Frankel et al. (1996) and the working paper version of this paper (Frankel and Romer, 1996) consider a second approach to constructing the instrument for trade. In addition to using information on the proximity of a country's trading partners, these papers use some information about the partners' incomes. Specifically, they include measures of physical and human capital accumulation and population growth. As those papers explain, one can argue that the factor accumulation of a country's trading partners is uncorrelated with determinants of the country's income other than its trade and factor accumulation: once one accounts for the impact of a country's own factor accumulation on its income, the most evident channel through which its partners' factor accumulation may affect its income is by increasing the partners' incomes, and thus increasing the amount the country trades. Thus if one controls for the country's own factor accumulation, its partners' factor accumulation can be used in constructing the instrument.

Not surprisingly, using more information in constructing the instrument increases the precision of the IV estimates of trade's effect on income. But the estimated effect of trade is not systematically different when one moves to the alternative instrument; this supports the argument that it is a valid instrument. The overall results are similar to those we obtain with our basic instrument: the IV estimates of trade's impact on income are much larger than the OLS estimates, and are marginally significantly different from zero.

C. *Robustness*

A natural question is whether the results are robust. We consider robustness along four dimensions.

First, as described above, Luxembourg and Singapore are major outliers in the relationship between the actual and constructed trade shares. Dropping either or both of these observations, however, does not change the basic pattern of the results. The most noticeable change occurs when Luxembourg is dropped from the baseline regressions in columns (1) and (2) of Table 3. This has effects similar to those of moving to the 98-country sample (which does not include Luxembourg): the OLS estimate of the effect of trade changes little, but the IV estimate and its standard error rise. Similarly, adding Luxembourg to the regressions in columns (3) and (4) of Table 3 moderately reduces the IV estimate of the effect of trade.

[16] That is, we perform a Hausman test (Jerry A. Hausman, 1978) of the hypothesis that actual trade is uncorrelated with the residual, and thus that OLS is unbiased. Under the null, asymptotically the OLS and IV estimates of trade's impact differ only because of sampling error. As a result, the difference between the two estimates divided by the standard error of the difference is distributed asymptotically as a standard normal variable. Furthermore, under the null the variance of the difference between the IV and OLS estimates is just the difference in their variances; that is, the standard error of the difference is the square root of the difference in the squares of their standard errors.

A second concern is the possibility that systematic differences among parts of the world are driving the results. That is, it could be that our IV estimates of the impact of trade arise because the countries in certain regions of the world have systematically higher constructed trade shares given their size and also have systematically higher incomes. In this case, our findings might be the result not of trade, but of other features of those regions.

To address this concern, we reestimate the regressions in Table 3 including a dummy variable for each continent. This modification substantially increases the standard errors of the IV estimates of the impact of trade on income. As a result, the estimates are no longer significantly different from zero. In addition, the IV estimate of the impact of trade for the 150-country sample [column (2) of Table 3] falls to only moderately above the OLS estimate. When Luxembourg is dropped from the sample, however, the IV estimate returns to being much larger than the OLS estimate. For the 98-country sample, in contrast, inclusion of the continent dummies raises the IV estimate slightly and lowers the OLS estimate slightly.

As an alternative way of considering the impact of differences across parts of the world, we follow Robert E. Hall and Charles I. Jones (1999) and include countries' distance from the equator as a control variable. This variable may reflect the impact of climate, or it may be a proxy for omitted country characteristics that are correlated with latitude. With this approach, the IV estimate of trade and size's effects are virtually identical to the OLS estimate for the full sample, and only moderately larger than the OLS estimate for the 98-country sample. Thus there is some evidence that systematic differences among regions are important to our finding that the IV estimates of trade's effects exceed the OLS estimates. Even in this case, however, there is still no evidence that the OLS estimates overstate trade's effects.

Third, our data are highly imperfect in various ways. Potentially most important, for the major oil-producing countries much of measured GDP represents the sale of existing resources rather than genuine value added. Since these countries have among the highest measured incomes in the sample, they have the potential to affect the results substantially. In fact, however, they are not important to our findings: they are already absent from the 98-country sample, and excluding them from the 150-country sample changes the estimates only slightly. As a more general check of the possible importance of data problems, we use the Penn World Table's summary assessments of the quality of countries' data to exclude the countries with the poorest data from the sample. Specifically, we exclude all countries whose data are assigned a grade of "D." This reduces the 150-country sample to 99, and the 98-country sample to 77. These reductions in the sample sizes moderately increase the standard errors of both the OLS and IV estimates of the impact of trade. The point estimates change little, however.

Finally, one can imagine reasons that virtually all the variables used in finding the geographic component of countries' trade might have some endogenous component that is correlated with the error term in the income equation. For example, whether countries have access to an ocean may be endogenous in the truly long run, and may be determined in part by other forces that affect income.[17] Similarly, population is endogenous in the very long run. To check that no single variable that could conceivably be endogenous is driving the results, we redo the construction of the instrument and the regressions in Table 3 in five ways: omitting the landlocked variable from the bilateral trade equation; excluding population from this equation; omitting all interactions with the common-border dummy from this equation; using total population rather than the labor force both in measuring countries' sizes and in computing income per person; and excluding area from both equations.

None of these changes has a major effect on the results. Although the changes sometimes affect the IV estimates noticeably, in every case they remain much larger than the OLS estimates. Moreover, there is no systematic tendency for the changes to reduce the IV

[17] The potential endogeneity of characteristics of borders other than whether countries are landlocked is unlikely to cause serious difficulties, for two reasons. First, the location of borders is largely determined by forces other than government policies and other determinants of current income; that is, the endogenous component of borders appears small. Second, because our estimates control for within-country trade, what is key to our estimates is the overall distribution of population, not the placement of country borders.

estimates; indeed, there are several cases where the changes raise the estimate considerably, or reduce its standard error considerably. Thus, our findings do not hinge on the use of any one of these variables in constructing the fitted trade shares.

D. *The Channels Through Which Trade Affects Income*

The results thus far provide no information about the mechanisms through which trade raises income. To shed some light on this issue, we decompose income and examine trade's impact on each component.

We consider two decompositions of income. The first follows Hall and Jones (1999). Suppose output in country i is given by

$$Y_i = K_i^{\alpha}[e^{\phi(S_i)}A_iN_i]^{1-\alpha}, \tag{10}$$

where K and N are capital and labor, S is workers' average years of schooling, $\phi(\cdot)$ gives the effects of schooling, and A is a productivity term. Equation (10) could be used to decompose differences in output per worker into the contributions of capital per worker, schooling, and productivity. As Hall and Jones note, however, an increase in A leads to a higher value of K for a given investment rate. Following Peter Klenow and Andrés Rodriguez-Clare (1997), they therefore rewrite (10) as

$$Y_i = (K_i/Y_i)^{\alpha/(1-\alpha)}e^{\phi(S_i)}A_iN_i. \tag{11}$$

Dividing both sides by N_i and taking logs yields

$$\ln(Y_i/N_i) = \frac{\alpha}{1-\alpha}\ln(K_i/Y_i) + \phi(S_i) + \ln A_i. \tag{12}$$

Equation (12) expresses the log of output per worker as the sum of the contributions of capital depth (reflecting such factors as investment and population growth), schooling, and productivity. Hall and Jones set $\alpha = 1/3$ and let $\phi(\cdot)$ be a piecewise linear function with coefficients based on microeconomic evidence. This allows them to measure each component of (12) other than $\ln A_i$ directly from the data, and then find $\ln A_i$ as a residual.

Our second decomposition of income is simpler. We write log output per worker in 1985 as the sum of its value at the beginning of the sample (1960) plus the change during the sample period:

$$\ln(Y_i/N_i)_{1985} = \ln(Y_i/N_i)_{1960} + [\ln(Y_i/N_i)_{1985} - \ln(Y_i/N_i)_{1960}]. \tag{13}$$

For both decompositions, we regress each component of income on a constant, the trade share, and the size measures. Again, we consider both OLS and IV. Since the decompositions cannot be performed for the full sample, we consider only the 98-country sample. The results are reported in Table 4. The first three pairs of columns show the results for the Hall and Jones decomposition, and the remaining two pairs show the results for the decomposition into initial income and subsequent growth.

With both decompositions, the estimates using instrumental variables suggest that trade increases income through each component of income. For the first decomposition, the estimated impacts of trade on physical capital depth and schooling are moderate, and its estimated impact on productivity is large. The estimates imply that a one-percentage-point increase in the trade share raises the contributions of both physical capital depth and schooling to output by about one-half of a percentage point, and the contribution of productivity to output by about two percentage points. For the second decomposition, trade's estimated effects on both initial income and subsequent growth are large. Here the estimates imply that a one-percentage-point increase in the trade share raises both initial income and the change over the sample period by about one and a half percentage points. Further, in every case, the estimates suggest that country size, controlling for international trade, is beneficial. And in every case the IV estimates of the effects of international trade and country size are substantially larger than the OLS estimates.

The standard errors of the IV estimates are

TABLE 4—TRADE AND THE COMPONENTS OF INCOME

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	$\frac{\alpha}{1-\alpha}\ln(K_i/Y_i)$		$\phi(S_i)$		$\ln A_i$		$\ln(Y/N)_{1960}$		$\Delta\ln(Y/N)$	
Estimation	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Constant	−0.72	−1.29	0.10	−0.37	7.47	3.05	7.45	4.27	−0.50	−2.65
	(0.34)	(0.93)	(0.30)	(0.81)	(0.74)	(2.84)	(1.03)	(3.07)	(0.39)	(1.66)
Trade share	0.36	0.59	0.18	0.37	0.27	2.04	0.38	1.66	0.45	1.31
	(0.10)	(0.36)	(0.08)	(0.31)	(0.21)	(1.10)	(0.29)	(1.19)	(0.11)	(0.65)
Ln population	0.02	0.04	0.06	0.07	0.21	0.32	0.09	0.17	0.12	0.18
	(0.03)	(0.04)	(0.03)	(0.03)	(0.06)	(0.11)	(0.09)	(0.12)	(0.03)	(0.06)
Ln area	0.04	0.07	−0.01	0.01	−0.13	0.08	−0.02	0.13	−0.03	0.07
	(0.02)	(0.05)	(0.02)	(0.04)	(0.05)	(0.14)	(0.07)	(0.15)	(0.03)	(0.08)
Sample size	98	98	98	98	98	98	98	98	98	98
R^2	0.13	0.13	0.09	0.08	0.14	0.06	0.03	0.02	0.24	0.20
SE of regression	0.32	0.33	0.28	0.29	0.69	0.92	0.96	1.06	0.36	0.47
First-stage F on excluded instrument		8.45		8.45		8.45		8.45		8.45

Note: Standard errors are in parentheses.

large, however. For productivity and for growth over the sample period, the estimates of trade's effect are marginally significantly different from zero (t-statistics of 1.8 and 2.0, respectively). For the other dependent variables, the t-statistics are between 1.2 and 1.6. Similarly, the t-statistic for the sum of the coefficients on population and area is 1.9 for productivity, 2.0 for growth, and between 1.3 and 1.5 for the other dependent variables. Thus, although the estimates suggest that international and within-country trade raise income through several different channels, they do not allow us to determine their contributions through each channel with great precision.

The results also provide no evidence that OLS is biased. The IV and OLS estimates of trade's impact never differ by a statistically significant amount. Indeed, the t-statistic for the null that the estimates are equal exceeds 1.5 only once.[18]

E. *Why Are the IV Estimates Greater Than the OLS Estimates?*

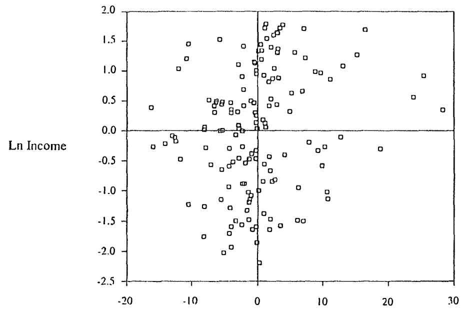
Both the simple model presented in Section I, subsection A, and prevailing views about the association between trade and income suggest that IV estimates of trade's impact on income will be less than OLS estimates. There are four main reasons. First, countries that adopt free-trade policies are likely to adopt other policies

[18] The result that the IV estimates of the effects of trade and country size on each component of income are positive is extremely robust to the exclusion of outliers, the addition of continent dummies and latitude, the omission of countries with the most questionable data, the use of less information in constructing the instrument, and expanding the sample to include as many countries as possible (132 countries for the Hall and Jones decomposition, 127 countries for the decomposition into initial income and subsequent growth). The one exception is that in the IV regression with $\phi(S_i)$ as the dependent variable, both the coefficient on trade and the sum of the coefficients on population and area are very slightly negative when latitude is included as a control. Likewise, the finding that the IV estimates of the effects of trade and size on productivity, initial income, and growth over the sample are larger than the OLS estimates is extremely robust. Again there is only a single exception: when latitude is included, the IV estimates of the impact of trade and of the combined effect of population and area on initial income are slightly smaller than the OLS estimates. The result that the IV estimates of trade's and size's effects on capital depth and schooling are larger than the OLS estimates, on the other hand, is only moderately robust: for several of our robustness checks, the IV estimates are smaller. The difference is never large, however.

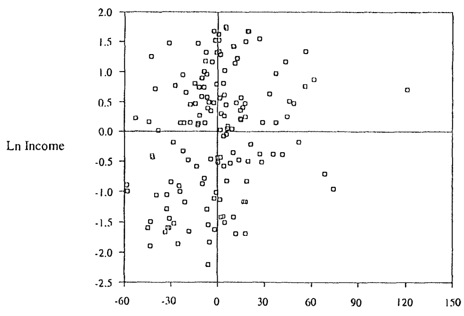
Finally, Hall and Jones's data are for 1988 rather than 1985. This is not important, however: redoing the basic regressions in Table 3 for the 98-country sample using 1988 income per worker changes the results only trivially.

that raise income. Second, countries that are wealthy for reasons other than trade are likely to have better infrastructure and transportation systems. Third, countries that are poor for reasons other than low trade may lack the institutions and resources needed to tax domestic economic activity, and thus may have to rely on tariffs to finance government spending. And fourth, increases in income coming from sources other than trade may increase the variety of goods that households demand and shift the composition of their demand away from basic commodities toward more processed, lighter weight goods. All of these considerations would lead to positive correlation between trade and the error term in an OLS regression, and thus to upward bias in the OLS estimate of trade's effects. And since there is no reason to expect correlation between proximity and these various omitted country characteristics, there is no reason to expect the IV estimate to suffer a similar bias. But we find that the IV estimate is almost always considerably larger than the OLS estimate. Although the difference is not statistically significant, the fact that it is large and positive is surprising.

The OLS estimate is determined by the partial association between income and trade, while the IV estimate is determined by the partial association between income and the component of trade correlated with the instrument. Thus, mechanically, the fact that the OLS estimate is smaller than the IV estimate means that income's partial association with the component of trade that is not correlated with the instrument is weaker than its partial association with the component that is correlated. Figure 3 presents these two partial associations. Since Luxembourg and Singapore are outliers in the relationship between trade and the instrument, they are omitted. Panel A of the figure shows the positive partial association between income and the component of trade correlated with the instrument; it is this association that underlies the positive coefficient on trade in the basic IV regression [column (2) of Table 3]. Panel B of the figure shows that there is also a positive partial association between income and the component of trade not correlated with the instrument. This association, though statistically significant, is smaller than the partial association in Panel A ($\hat{b} = 0.75$, with a standard error of 0.26). It is this smaller relationship that causes the OLS estimate to be less than the IV estimate.



A. Income and the Component of the Trade Share Correlated with the Instrument



B. Income and the Component of the Trade Share Uncorrelated with the Instrument

FIGURE 3. PARTIAL ASSOCIATIONS BETWEEN INCOME AND THE TRADE SHARE

The figures show that the difference between the OLS and IV estimates is not due to a handful of observations. (Recall that including Luxembourg and Singapore reduces the IV estimate of trade's effects. Thus the only major outliers are not the source of the gap between the OLS and IV estimates, but in fact reduce it.) In addition, examining which specific countries are important to the estimates suggests that there is no simple omitted country characteristic that is driving the results.

The observations that are responsible for the high estimated IV coefficient are those at the upper right and lower left of Panel A. The most influential observations at the upper right are Qatar, Belgium, the United States, and Israel. The most influential observations at the lower left are Malawi, Lesotho, Burundi, and Haiti. Similarly, the observations that are lowering the OLS estimate relative to the IV estimate are those in the lower right and upper left of Panel B. The most influential observations at the lower right are Qatar, Trinidad and Tobago, Hong Kong, and Syria, while the most influential observations at the upper left are Lesotho, Mauritania, Togo, and Belize.

Thus, in both cases the low-income countries are, not surprisingly, disproportionately from sub-Saharan Africa. But there is no evident characteristic other than proximity that distinguishes the countries with low proximity and low income from those with high proximity and high income. Similarly, there is no clear characteristic other than trade that differentiates the countries with low residual trade and high income from those with high residual trade and low income. Together with our earlier results, this suggests that the finding that the IV estimate exceeds—or at least does not fall short of—the OLS estimate is robust and not the result of omitted country characteristics.

There are two leading explanations of the fact that the IV estimate of trade's impact exceeds the OLS estimate. The first is that it is due to sampling variation. That is, although there no reason to expect systematic correlation between the instrument and the residual, it could be that by chance they are positively correlated. The principal evidence supporting this possibility is that the differences between the IV and OLS estimates, though quantitatively large, are well within the range that can arise from sampling error. In our baseline regressions [columns (1) and (2) of Table 3], the t-statistic for the null hypothesis that the OLS and IV estimates are equal is 1.2 ($p = 0.25$). And in the variations on the baseline regression that we consider, this t-statistic never exceeds 2, and it is almost always less than 1.5. Moreover, in a few cases it is essentially zero. Thus, if one believes that theory provides strong grounds for believing that OLS estimates are biased up, our IV estimates do not provide a compelling reason for changing this belief.

The second candidate explanation of the finding that the IV estimates exceed the OLS estimates is that OLS is in fact biased down. The literal shipping of goods between countries does not raise income. Rather, trade is a proxy for the many ways in which interactions between countries raise income—specialization, spread of ideas, and so on. Trade is likely to be highly, but not perfectly, correlated with the extent of such interactions. Thus, trade is an imperfect measure of income-enhancing interactions among countries. And since measurement error leads to downward bias, this would mean that OLS would lead to an understatement of the effect of income-enhancing interactions.[19]

To explore this idea, consider the following simple model. Average income in country i is given by

$$(14) \quad \ln Y_i = a + b_1 I_{1i} + b_2 I_{2i} + \cdots + b_N I_{Ni} + cS_i + e_i,$$

where $I_1, I_2, ..., I_N$ are measures of the various ways in which interactions among countries affect income. This equation has the same form as (4), except that trade has been replaced by $I_1, I_2, ..., I_N$. We want to rewrite this model in a way that expresses the idea that trade is a noisy measure of income-promoting interactions. To do this, we define $T_i^* = (b_1 I_{1i} + b_2 I_{2i} + \cdots + b_N I_{Ni})/b$, where $b = \text{Var}(b_1 I_1 + b_2 I_2 + \cdots + b_N I_N)/\text{Cov}(b_1 I_1 + b_2 I_2 + \cdots + b_N I_N, T)$ and where T is again trade. With this definition, we can rewrite (14) as

$$(15) \quad \ln Y_i = a + bT_i^* + cS_i + e_i.$$

It is straightforward to check that with these definitions, $T - T^*$ is uncorrelated with T^*. Thus we can write

$$(16) \quad T_i = T_i^* + u_i,$$

where u_i is uncorrelated with T_i^*. Thus, equations (15)–(16) express the idea that actual trade is a noisy measure of income-promoting interactions.

With this formulation, the relationship between income and actual trade is

[19] We are grateful to an anonymous referee and several seminar participants for suggesting this possibility.

(17) $\ln Y_i = a + bT_i + cS_i + (e_i - bu_i)$.

From (16), T and u are positively correlated. If this correlation is strong enough, it will cause the overall correlation between T and the residual in (17), $e-bu$, to be negative, and thus cause OLS to yield a downward biased estimate of b. And again this bias will not carry over to the IV estimates—proximity and other geographic variables are likely to be strongly correlated with income-enhancing interactions, and there is no evident reason for them to be correlated with the idiosyncratic factors that cause trade to move differently from those interactions.

In the univariate case, the expectation of the OLS estimate of b would be given by $E[\hat{b}_{\text{OLS}}] = (V_{T^*}/V_T)b$, where V_{T^*} and V_T are the variances of T^* and T. When size is controlled for, the expression is analogous:

$$E[\hat{b}_{\text{OLS}}] = \frac{V_{T^*|S}}{V_{T|S}}\, b, \tag{18}$$

where $V_{T^*|S}$ and $V_{T|S}$ are the conditional variances of T^* and T given size.

This discussion implies that trade must be a very noisy proxy for income-promoting interactions to account for the gap between the OLS and IV estimates. In our baseline regression for the full sample [columns (1) and (2) of Table 3], the OLS estimate of b is less than half the IV estimate. Thus for the gap between the two estimates to arise from the fact that trade is an imperfect proxy for income-enhancing interactions, the majority of the variation in trade uncorrelated with size would have to be uncorrelated with income-promoting interactions. To put it differently, u in (16) would have to have a standard deviation of 22 percentage points.

We conclude that the most plausible explanation of the bulk of the gap between the IV and OLS estimates is simply sampling error. This implies that our most important finding is not that the IV estimates of trade's effects exceed the OLS estimates, but rather that there is no evidence that the IV estimates are lower. In addition, it implies that our IV estimates may be substantially affected by sampling error, and thus that the OLS estimates are likely to be more accurate estimates of trade's actual impact on income.

III. Conclusion

This paper investigates the question of how international trade affects standards of living. Although this is an old question, it is a difficult one to answer. The amounts that countries trade are not determined exogenously. As a result, correlations between trade and income cannot identify the effect of trade.

This paper addresses this problem by focusing on the component of trade that is due to geographic factors. Some countries trade more just because they are near well-populated countries, and some trade less because they are isolated. Geographic factors are not a consequence of income or government policy, and there is no likely channel through which they affect income other than through their impact on a country's residents' interactions with residents of other countries and with one another. As a result, the variation in trade that is due to geographic factors can serve as a natural experiment for identifying the effects of trade.

The results of the experiment are consistent across the samples and specifications we consider: trade raises income. The relation between the geographic component of trade and income suggests that a rise of one percentage point in the ratio of trade to GDP increases income per person by at least one-half percent. Trade appears to raise income by spurring the accumulation of physical and human capital and by increasing output for given levels of capital.

The results also suggest that within-country trade raises income. Controlling for international trade, countries that are larger—and that therefore have more opportunities for trade within their borders—have higher incomes. The point estimates suggest that increasing a country's size and area by one percent raises income by one-tenth of a percent or more. And the estimates suggest that within-country trade, like international trade, raises income both through capital accumulation and through income for given levels of capital.

There are two important caveats to these conclusions. First, the effects are not estimated with great precision. The hypotheses that the impacts of trade and size are zero are typically only marginally rejected at standard significance levels. In addition, the hypothesis that the estimates based on the geographic component of trade are the same as the estimates based on overall trade are typically relatively far from rejection. Thus, although the results bolster the case for the benefits of trade, they do not provide decisive evidence for it.

This limitation is probably inherent in the experiment we are considering. Once country size is controlled for, geography appears to account for only a moderate part of the variation in trade. As a result, geographic variables provide only a limited amount of information about the relation between trade and income. Thus, unless additional portions of overall trade that are unaffected by other determinants of income can be identified, it is likely to be difficult to improve greatly on our estimates of the effects of trade.

The second limitation of the results is that they cannot be applied without qualification to the effects of trade policies. There are many ways that trade affects income, and variations in trade that are due to geography and variations that are due to policy may not involve exactly the same mix of the various mechanisms. Thus, differences in trade resulting from policy may not affect income in precisely the same way as differences resulting from geography.

Nonetheless, our estimates of the effects of geography-based differences in trade are at least suggestive about the effects of policy-induced differences. Our point estimates suggest that the impact of geography-based differences in trade are quantitatively large. We also find that the estimated impact is larger than what one obtains by naively using OLS, but is not significantly different from the OLS estimate. These results are not what one would expect if the positive correlation between trade and income reflected an impact of income on trade, or of omitted factors on both variables. In that sense, our results bolster the case for the importance of trade and trade-promoting policies.

APPENDIX

TABLE A1—BASIC DATA

Country	Actual trade share	Constructed trade share	Area (thousands of square miles)	Population (millions)	Income per worker
Algeria	49.66	13.97	919.595	4.859	13434
Angola	69.10	11.51	481.354	3.512	1742
Benin	76.99	42.20	43.483	1.874	2391
Botswana	121.28	24.03	231.800	0.370	6792
Burkina Faso	52.42	14.10	105.870	4.150	940
Burundi	30.82	24.86	10.747	2.539	986
Cameroon	57.67	15.79	183.569	3.831	3869
Cape Verde Islands	118.02	45.11	1.557	0.120	2829
Central African Republic	65.23	15.13	241.313	1.309	1266
Chad	61.43	12.00	495.755	1.791	1146
Comoros	67.06	46.77	0.863	0.181	1400
Congo	112.81	25.77	132.046	0.760	6878
Djibouti	117.06	70.97	8.958	0.105	4647
Egypt	51.97	11.75	386.900	12.719	7142
Ethiopia	34.13	8.44	472.432	18.385	705
Gabon	100.18	30.65	103.346	0.420	9672
Gambia	89.14	52.20	4.093	0.358	1609
Ghana	21.29	18.87	92.100	4.468	2237
Guinea	71.80	23.95	94.926	2.243	1583
Guinea-Bissau	62.74	42.24	13.948	0.425	1354
Ivory Coast	78.19	16.58	124.502	4.030	3740
Kenya	51.69	12.48	224.960	7.980	2014
Lesotho	152.42	20.66	11.720	0.743	2028
Liberia	79.63	29.81	43.000	0.811	2312
Madagascar	30.99	9.90	226.660	4.498	1707
Malawi	54.09	12.67	45.747	3.180	1171
Mali	73.60	12.80	482.077	2.332	1686

TABLE A1—*Continued.*

Country	Actual trade share	Constructed trade share	Area (thousands of square miles)	Population (millions)	Income per worker
Mauritania	141.56	23.44	397.953	0.533	2674
Mauritius	109.10	31.11	0.787	0.577	7474
Morocco	58.50	12.71	172.413	6.714	6427
Mozambique	18.38	11.11	308.642	7.290	1417
Namibia	119.81	21.31	317.818	0.380	8465
Niger	51.27	12.37	489.206	3.343	1098
Nigeria	28.53	8.68	356.700	30.743	2874
Reunion	53.14	39.92	0.969	0.216	7858
Rwanda	30.65	26.20	10.169	3.005	1539
Senegal	70.63	19.87	75.954	2.758	2688
Seychelles	111.95	84.98	0.175	0.029	7058
Sierra Leone	19.15	27.81	27.700	1.372	2411
Somalia	25.64	14.89	246.199	2.774	1574
South Africa	55.43	8.90	471.440	11.240	9930
Sudan	21.34	10.97	967.491	7.121	2436
Swaziland	118.71	56.87	6.704	0.277	5225
Tanzania	21.03	10.97	364.900	10.266	975
Togo	105.52	41.47	21.925	1.277	1516
Tunisia	71.33	23.83	63.379	2.280	8783
Uganda	22.54	12.97	91.343	6.236	1224
Zaire	53.15	8.97	905.365	12.321	1136
Zambia	76.96	13.81	290.586	2.274	2399
Zimbabwe	56.40	11.27	150.699	3.135	3261
Bahamas	124.11	38.03	5.382	0.097	29815
Barbados	130.30	56.10	0.166	0.127	12212
Belize	183.27	87.48	8.866	0.049	8487
Canada	54.48	4.97	3851.809	12.595	31147
Costa Rica	63.19	23.37	19.652	0.920	9148
Dominica	103.09	75.08	0.305	0.030	6163
Dominican Republic	64.24	22.37	18.704	1.912	7082
El Salvador	52.21	28.91	8.260	1.564	5547
Grenada	120.63	81.25	0.133	0.039	4502
Guatemala	24.94	22.04	42.042	2.262	7358
Haiti	38.44	20.44	10.714	2.514	2125
Honduras	54.15	27.58	43.277	1.307	4652
Jamaica	131.89	22.19	4.411	1.059	4726
Mexico	25.74	4.52	761.600	24.669	17036
Nicaragua	36.60	23.46	50.180	0.980	5900
Panama	70.96	23.56	29.761	0.760	10039
Puerto Rico	136.74	22.75	3.515	1.101	21842
St. Lucia	165.77	68.83	0.238	0.057	5317
St. Vincent & Grenadines	152.17	79.41	0.150	0.042	5796
Trinidad & Tobago	61.90	30.33	1.980	0.441	25529
United States	18.01	2.56	3540.939	117.362	33783
Argentina	17.10	5.60	1072.067	10.798	14955
Bolivia	30.27	8.06	424.162	1.978	5623
Brazil	19.34	3.03	3286.470	49.609	10977
Chile	53.85	7.25	292.132	4.303	9768
Colombia	26.33	7.54	439.735	9.433	9276
Ecuador	47.63	11.42	109.484	2.820	9615
Guyana	109.95	25.92	83.000	0.280	3573
Paraguay	49.58	10.43	157.047	1.226	6241
Peru	39.42	7.03	496.222	6.107	8141
Suriname	82.99	30.96	63.251	0.124	10883
Uruguay	47.86	17.07	68.040	1.169	10216
Venezuela	40.76	8.94	352.143	5.789	18362
Bahrain	188.70	71.82	0.240	0.178	22840

TABLE A1—*Continued.*

Country	Actual trade share	Constructed trade share	Area (thousands of square miles)	Population (millions)	Income per worker
Bangladesh	25.78	10.31	55.598	27.684	4265
Bhutan	62.54	37.74	17.954	0.575	1504
China	19.44	2.30	3689.631	612.363	2166
Hong Kong	209.52	35.88	0.398	3.516	16447
India	15.04	3.29	1229.737	295.478	2719
Indonesia	42.66	4.47	735.268	62.136	4332
Iran	15.20	10.06	636.293	13.540	13847
Iraq	49.22	19.14	169.235	4.105	15855
Israel	85.80	54.17	8.020	1.602	21953
Japan	25.54	5.47	143.574	75.526	18820
Jordan	113.50	68.18	37.297	0.601	15655
Korea, Republic of	67.86	14.36	38.031	16.608	10361
Kuwait	96.45	42.55	6.880	0.640	35065
Laos	13.80	27.32	91.429	1.758	2739
Malaysia	104.69	16.82	128.328	6.217	10458
Mongolia	82.72	13.52	604.829	0.894	3966
Myanmar	13.16	10.74	261.220	16.613	1332
Nepal	31.29	13.26	54.463	6.958	2244
Oman	87.06	34.19	82.030	0.368	31609
Pakistan	34.00	8.04	310.400	28.567	4249
Philippines	45.84	8.84	115.830	19.945	4229
Qatar	80.94	69.56	4.412	0.166	36646
Saudi Arabia	79.97	14.98	865.000	3.652	28180
Singapore	318.07	48.90	0.220	1.189	17986
Sri Lanka	62.93	13.94	25.332	5.786	5597
Syria	37.23	37.44	71.498	2.556	17166
Taiwan	94.62	17.92	13.895	8.262	12701
Thailand	51.20	9.45	198.455	26.793	4751
United Arab Emirates	89.66	33.42	32.000	0.694	38190
Yemen	49.34	16.83	128.560	2.369	6425
Austria	81.27	36.64	32.375	3.528	23837
Belgium	151.34	52.46	11.781	4.071	27325
Bulgaria	85.99	31.12	42.823	4.417	9662
Cyprus	107.57	54.39	3.572	0.310	13918
Czechoslovakia	69.45	21.07	49.383	8.137	7467
Denmark	72.99	30.89	16.631	2.780	23861
Finland	57.50	21.64	130.119	2.493	23700
France	47.17	15.26	211.208	24.882	27064
Germany, West	61.52	18.47	96.010	28.085	27252
Greece	53.97	27.01	50.961	3.800	16270
Hungary	82.32	26.92	35.920	5.195	10827
Iceland	81.83	33.08	39.709	0.127	23256
Ireland	118.84	33.85	26.600	1.342	19197
Italy	46.06	13.97	116.500	22.714	27189
Luxembourg	211.94	281.29	0.999	0.157	30782
Malta	160.86	98.14	0.122	0.119	15380
Netherlands	118.76	35.84	16.041	5.855	28563
Norway	86.00	23.54	125.049	2.043	28749
Poland	35.07	13.84	120.728	19.235	8079
Portugal	77.95	18.78	35.550	4.540	11343
Romania	41.62	18.80	91.699	11.275	4021
Spain	43.51	12.38	194.885	13.732	21169
Sweden	69.02	18.22	173.800	4.238	26504
Switzerland	77.69	32.57	15.941	3.222	29848
Turkey	44.40	11.26	300.947	21.829	7091
United Kingdom	56.87	13.47	94.247	27.684	22981
Soviet Union	18.28	3.68	8600.387	142.801	13700

TABLE A1—*Continued.*

Country	Actual trade share	Constructed trade share	Area (thousands of square miles)	Population (millions)	Income per worker
Yugoslavia	57.88	25.82	39.449	10.475	11417
Australia	35.28	4.07	2966.150	7.391	28960
Fiji	89.13	18.56	7.078	0.232	9840
New Zealand	65.25	8.19	103.884	1.438	26039
Papua New Guinea	94.52	10.17	178.704	1.660	3374
Solomon Islands	123.60	25.12	10.954	0.088	5109
Tonga	102.25	43.40	0.288	0.030	6022
Vanuatu	123.33	30.86	4.707	0.042	5707
Western Samoa	92.17	32.77	1.093	0.050	5388

Notes: Actual trade share—Ratio of imports plus exports to GDP, 1985 (Penn World Table, Mark 5.6, Series OPEN).

Constructed trade share—Aggregated fitted values of bilateral trade equation with geographic variables. (See text, Section I, subsections B–D.)

Area—Rand McNally (1993).

Population—Economically active population, 1985 (Penn World Table, Mark 5.6, constructed from real GDP per capita (RGDPCH), real GDP per worker (RGDPW), and total population (POP): RGDPCH*POP/RGDPW).

Income per worker—Real GDP per worker, 1985; 1985 international prices (dollars) (Penn World Table, Mark 5.6, Series RGDPCH).

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